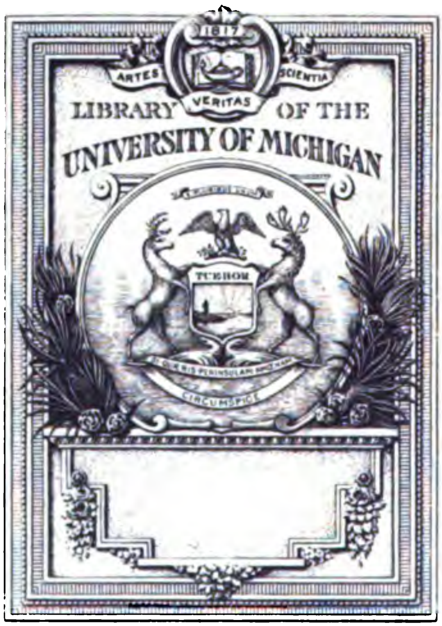
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PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From November 16, 1871, *to June* 20, 1872.

VOL. XX.

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ERRATA.

Page 84, line 10 from bottom, *for* 1846·6 *read* 1848·6.
" 85, " 20 from top, *for* 1846·6 *read* 1848·6.
" 419, " 12, *for* change *read* correction.
" 419, " 17, *delete* "with the signs changed."
" 420, " 4, *for* Dec. 29 *read* Dec. 24.
" 420, " 8 from bottom, *for* 0·000014 *read* 0·000010.
" 421, " 5, *for* 2·22 *read* 22·2; *for* 22·9 *read* 23·3; *for* 15·7 *read* 14·5; and *for* —7·2 *read* —8·8.
" 421, " 11 from bottom, *for* 25·82 *read* 25·70.
" 421, " 10 " *for* 25·96 *read* 25·92.
" 422, " 21, *for* 33·7 *read* 24·1.
" 521, " 13 from bottom, *for* Tiger *read* Lion.

NOTE, vol. xix. p. 526.

I did not perceive till the eve of publication that $L_n=0$; this will give as the equation to determine Q simply, $L_0 \frac{d^n \log_e Q}{dx^n} + L_1 \frac{d^{n-1} \log_e Q}{dx^{n-1}} + \ldots + L_{n-1} \frac{d \log_e Q}{dx} = 0.$

W. H. L. R.—July 31, 1871.

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OBITUARY NOTICES OF FELLOWS DECEASED.

James Yates was born April 30th, 1789, at his father's pleasant residence in the extra-parochial place of Toxteth Park, adjoining the borough of Liverpool. His father, John Yates, was the eloquent and highly respected minister of a Presbyterian congregation of ancient date, which included many of the most opulent and influential families of the town. His mother was a member of one of these families; she was the daughter of John Ashton, who had coal-mines near St. Helen's and salt-works on the Mersey, and wishing to obtain a cheap and easy mode of conveying his coal to the salt-works, projected the first canal in this country.

The subject of this notice was educated in classics under the Rev. Wm. Shepherd, minister of the Presbyterian chapel at Gateacre. In October 1805, James went to Glasgow University, in company with Henry Holland (the present Sir Henry Holland, Bart.), who introduced him as a student. He passed three sessions at Glasgow and one at Edinburgh, and took his M.A. degree in 1812. He had already studied biblical criticism and ecclesiastical history under the Rev. Charles Wellbeloved, tutor of a dissenting college at York. He now became minister of a newly formed congregation in Glasgow, and declared himself a supporter of Unitarian Christianity. This led to a controversy with the Rev. Ralph Wardlaw, the pastor of a large society of Independents. His 'Discourses on the chief points of the Socinian Controversy,' and Mr. Yates's 'Vindication of Unitarianism,' excited a good deal of attention. Mr. Yates at this time also took up the subject of "total abstinence," in consequence of the many excesses witnessed by him in Glasgow, He published some discourses on the question, which helped to lead the way to what has been called the "temperance movement;" but he had some doubt whether the taking of pledges of total abstinence is to be recommended. He thought a teetotaller better than a drunkard, but a temperate man better than either.

In 1817, Mr. Yates became minister of the new meeting-house which was built on the ruins of Dr. Priestley's chapel at Birmingham, and there remained until 1825. On the 24th of February, 1824, he married Dorothea, daughter of John William Crompton, a Brazil merchant, who survives him. Accompanied by her he, in January 1827, visited Munich, and afterwards Berlin, Mr. Yates being anxious to study under some of the most renowned German professors. He attended the lectures of Thiersch on Greek antiquities, of Ritter on geography, of Bopp on Sanscrit, and some others. He returned to England in April or May 1828, and took a house in Upper Bedford Place, in order to be near his half-brother, Dr. John Bostock, F.R.S., to whom he was tenderly attached. In 1831 Mr. Yates had the charge of a congregation in Little Carter Lane, Doctors'

Commons, from which, however, he retired in 1834, when his congregation presented him with a piece of plate.

From this time Mr. Yates devoted himself to scientific and literary pursuits. He became, as early as 1819, a Fellow of the Geological Society, and of the Linnean in 1822. He was one of the early founders of the British Association for the Advancement of Science; and at the termination of its first meeting at York in 1831, he was chosen Secretary to the Council, and continued to fill that office during some years. He became a Fellow of the Royal Society in 1839. He took an active part with Dr. Guest in the formation of the Philological Society in 1842.

In looking over a list of Mr. Yates's publications, it is impossible not to be impressed with the wide range of his subjects. He has several papers in the Transactions of the Geological Society for 1821, 1822, and 1825, and the Edinburgh New Philosophical Journal for 1831 and 1862; various papers on antiquities and language in the Journal of the Royal Asiatic Society for 1837, and in that of the Philological Society for 1842. He has various papers on theological and educational subjects in various journals; he contributed largely to Dr. Smith's 'Dictionary of Greek and Roman Antiquities,' 1842, and published separately, under the title of "Textrinum Antiquorum," an account of the art of weaving among the ancients: part 1 was devoted to raw materials; a second volume which was intended to follow was not completed at the time of his death. He wrote in the 'Classical Museum,' in 1845, on *Acanthion* &c. of the ancient classics. He wrote on the natural order *Cycadaceæ* in the Proceedings of the Linnean Society in 1849, and contributed a number of papers to the Archæological Journal. He also exerted his influence to get introduced into this country an improved system of measures, weights, and coins; and in 1851 received the Telford Medal of the Institution of Civil Engineers, for an essay on the subject. He also contributed information on the metric system to the Statistical Section of the British Association. He has published a variety of papers in connexion with this subject, and was the principal supporter of the British branch of the International Association for obtaining a uniform decimal system of measures, weights, and coins.

As early as 1842 Mr. Yates showed symptoms of an overworked brain, and was ordered by Dr. Tweedie to seek refreshment in travel. Indeed every year he had, as it were, to evade the hay fever, to which he was an easy victim. In 1848 he acquired the lease of Lauderdale House, Highgate. Here he passed the remainder of his life, beloved and respected by all who knew him. Here, too, he gave to scientific men those pleasant garden parties and receptions which received additional interest from the historical associations of the place. He had just completed his 82nd year, when he sank quietly to rest, on May 7th, 1871, and was buried in Highgate Cemetery on the 11th, near the remains of his friend Henry Crabb Robinson.

Mr. Yates took great delight in his garden, and in the cultivation of botany. He was justly proud of his success in getting the *Cycas* to flower,

for which he gained the second prize at the International Horticultural Exhibition and Botanical Congress, held in London in 1866.—C. T.

GEORGE GROTE was born on the 17th of November, 1794, at Clayhill, near Beckenham, Kent. His father was George Grote, son of Andrew Grote, who, in the middle of last century, came to England from Bremen, settled as a merchant in the city of London, and on the 1st of January, 1766, in conjunction with George Prescott, established the banking-house that still bears their joint names. His mother was Selina Mary, only daughter of the Rev. Dr. Peckwell, of Chichester, who belonged to the connexion of the Countess of Huntingdon, and, owing to his eminence as a preacher, was selected by her as one of her chaplains. The wife of Dr. Peckwell was an Irish lady, Miss Blosset, of an ancient French Protestant family of Touraine, named De Blosset, some members of which came to this country at the revocation of the Edict of Nantes, and purchased property in the county of Meath.

George Grote the younger obtained the first rudiments of learning from his mother, who had an excellent understanding and good attainments, especially in history, being at the same time severely pious. In his sixth year he was sent to the Grammar School at Sevenoaks, kept by Mr. Whitehead. In his tenth year he entered Charterhouse, then under the charge of Dr. Raine, an excellent classical scholar; but nothing appears to have been taught there besides the learned languages. He had as companions at Charterhouse, Connop Thirlwall (Bishop of St. David's), the two brothers Waddington (one the late Dean of Durham, the other late Under-Secretary at the Home Office), Henry Havelock, and Cresswell Cresswell; with the first three he maintained a warm friendship through life. He left Charterhouse in his sixteenth year, a good classical scholar, having been uniformly at the top of his class. During his holidays he was provided with a teacher at home for French and Mathematics.

In 1809, on leaving school, he entered the banking-house as clerk, being made a partner when, in 1815, he reached the age of twenty-one. While devoting himself sedulously to business, he employed all his spare hours in reading and study. He followed out his classical scholarship to a thorough mastery of the great works of Greece and Rome. At the same time he applied himself to modern literature, both in belles-lettres and in abstract thought; history in particular he always devoured with avidity. In no very long time he showed a decided tendency towards the Political and Mental Sciences, which tendency was fostered by his choice of companions. In business he became acquainted with Ricardo, who directed his mind to Political Economy, and introduced him to the still more powerful ascendency of James Mill. He has publicly acknowledged his great obligations to Mill; through him he became acquainted with Bentham, and contracted an intimacy with Mr. John Stuart Mill, which was sustained through life. James Mill was himself a competent Greek scholar, and an admirer of the

philosophy and the democratic institutions of Greece: Grote was affected with the like sentiments. They were further agreed in bringing the ancients into comparison with the best modern thinking.

In 1820 Mr. Grote married Miss Harriet Lewin (second daughter of Thomas Lewin, Esq., of the Hollies, Bexley, Kent), who, both in cooperation with him and by her separate works, will leave a mark behind her. The year succeeding his marriage saw his first publication, an anonymous pamphlet entitled "Statement of the Question of Parliamentary Reform: with a reply to the objections of the Edinburgh Review, No. 61." The article in the 'Review' was written by Sir James Mackintosh. The pamphlet is a clear and forcible argumentative statement of the Reform question, then beginning to be seriously canvassed. About 1824 he formed the purpose of writing a 'History of Greece,' his own inclinations being strengthened both by Mr. Mill and by Mrs. Grote. In the 'Westminster Review' for April 1826, he published an article of sixty pages on Grecian History, containing a minute criticism of the misrepresentations of Mitford. He afterwards acknowledged in the preface to his own History that the first idea of it was conceived "at a time when ancient Hellas was known to the English public chiefly through the pages of Mitford."

About this time a small Society was formed for readings in philosophical subjects, the meetings of which took place at Mr. Grote's house, in Threadneedle Street, on certain days, from half-past eight till ten in the morning, when the members had to repair to their respective occupations. The members were Grote, John Stuart Mill, Roebuck, William Ellis, William and Henry Prescott, Eyton Tooke, Charles Buller, two brothers Whitmore, and George John Graham (afterwards assignee in bankruptcy). The mentor of their studies was the elder Mill. The readings embraced the logical treatises of Hobbes, Du Trieu, and Whately; Hartley on Man; and, at a later period, Mill's 'Analysis of the Mind.' These works were read out paragraph by paragraph, commented upon by each member in turn, discussed and rediscussed, until no one had any thing further to say.

The logical discipline thus imparted to these youthful students has unquestionably told in various ways upon the mind and literature of this country and the world.

The death of his father in 1830 made him one of the two chief partners in the banking-house. Although strongly sympathizing with the Reform agitation which had then commenced, he had not as yet taken a public part in it. He now came into prominence in the City of London movement. In 1831 appeared his work entitled "Essentials of Parliamentary Reform," which was characterized by his thoroughness and vigour of thought and language. His "essentials" were as follows:—The first was an extension of the suffrage, to commence with enfranchising whoever gave evidence of possessing £100 a year, to be followed by further extensions according to the advancement of education. The second essential was the Ballot; the others were,—reduction in the number of the House (to about

300), equalizing of constituencies, improved polling arrangements, and greater frequency of election.

Placed at the top of the poll for the City of London, he took his seat in the Reformed Parliament in February 1833. His public career is most noted by his motions on the Ballot, made every year, with one exception, from 1833 to 1839. His handling of this question was exhaustive; the ablest advocates that succeeded him have confessed themselves unable to add either to the number of his arguments or to the cogency of his replies; while the intense respect for individual liberty of action which animated his whole life, gave to a political question a high ethical tone. But this was only one out of many topics that he brought forward during his nine years' work in Parliament. The Bank Charter, the Poor-Law Amendment Act, the Municipal Reform Act, the Corn Laws, the Irish Church, the Irish Coercion Bill, the Canadian Rebellion, the Civil List were among the great and stirring questions that called out his oratorical powers. He sounded the first note on Church Reform and on the removal of the Jewish Disabilities, and was always ready to chime in with every proposal for extending individual liberty, for improving education, as well as for contributing to the general well-being of the community. His last important speech was (in 1841) on the Syrian question, producing a very marked impression.

In 1841 he retired from Parliament, and in 1843 withdrew from active partnership in the banking-house, to devote himself to the composition of his 'History,' which he completed in twelve volumes in 1856. The first two volumes contained an independent examination of the Homeric poems, an original view of the Spartan institutions and legislation of Lycurgus, and, greatest of all, an entirely new handling of the early legends. The next two volumes detailed the rise of the Athenian Democracy, in which a distinction was carefully drawn between the institutions of Solon and the amendments of Cleisthenes. The fifth and sixth volumes traced the growth of the Athenian Empire, and the commencement of the great struggle known as the Peloponnesian War. The same subject occupied two more volumes, the seventh and eighth, which were concluded by the celebrated chapters on the Sophists and on Socrates. Some critics ventured the opinion that the work had attained its climax, that the author had exhausted his subject and himself, and could impart no new surprise. The opening of the ninth volume proved the rashness of the criticism; in nothing did Mr. Grote's peculiar powers show to better advantage than in recounting the advance and retreat of the Ten Thousand. He could now also point out to the haters of democracy what Greece had gained by the substitution of the Spartan headship for the Athenian; and very soon came the reverses and the prostration of that headship by the Thebans, under the leadership of Epaminondas. The extraordinary revolutions in Sicily—the career of the *Siculi tyranni*, and their overthrow by Timoleon—formed a splendid theme, and had ample justice done to it. The eleventh volume was occupied with the struggle against Philip of Macedon, in which the leading figure was

Demosthenes. The author had vindicated the Athenians and their democratic constitution from many unfair aspersions; he now exposed their real vices and degeneracy, their growing reluctance to personal military service and to the sacrifices of war. The last and crowning exploit of the historian was to unmask the world's conqueror and favourite, Alexander. With the most ample acknowledgments of Alexander's military genius and indefatigable activity, Mr. Grote has stripped his enterprizes and his conduct of every other virtue or merit; and in so doing has read a moral lesson against that adoration of force which has always lent a powerful aid to the oppressors of mankind.

It has been common to express a regret that the history was not continued down to the Roman conquest of Greece. The reason was characteristic of the author. In the preface to the first volume he announced it as his purpose to exhaust the *free life* of collective Hellas; and at the close of the twelfth, having to relate the honours bestowed upon Demochares, the nephew of Demosthenes, for acting on suppliant embassies to the foreign masters of Greece, he said that when such begging missions are the titles to honour of Athenian citizens, "the historian accustomed to the Grecian world as described by Herodotus, Thucydides, and Xenophon, feels that the life has departed from his subject, and, with sadness and humiliation, brings his narrative to a close."

There is no doubt that Mr. Grote was especially cast for an historian, although in his time he played many parts. He had an intense interest in human beings, and a strong passion for narrative; he had read the history of every country that had a history; while his memory for historical facts and dates was of the first order. In his study of science, and especially of the moral and political sciences, he was strongly arrested by the modes of arriving at scientific doctrines, and by the struggles of the mind in the pursuit. This more particularly qualified him to be the historian of philosophy.

The original scheme of the 'History of Greece' comprised a full account of Greek literature and Greek speculation. The chapters on Homer, on Lyric Poetry and the Seven Wise Men, on the Ionic Philosophers, on the Drama, Rhetoric, Dialectic, and the Sophists, and, finally, on Socrates were in fulfilment of the scheme. The remaining portion, including Plato and Aristotle, was postponed for a separate work. "Plato and the other Companions of Socrates," the first instalment, occupied nine years, and was published in 1865.

This great work brought into prominence characteristics of the author's mind that had been but little apparent in the 'History of Greece.' It comprises a full analysis of all the Platonic writings, with a rediscussion of the numerous questions (Logical, Ethical, Psychological, and Metaphysical) that are broached by Plato. He introduces a marked division between two classes of the Dialogues,—the one class Socratic, or Dialogues of Search, whose conclusion is usually negative; the other class Dialogues

of Exposition, wherein is announced some positive dogma. The great interest, in Mr. Grote's eyes, attaches to the first, the Dialogues of Search; never before were the peculiarities manifested by these put in such strong relief. They were the consummation of Grecian Dialectic, or free discussion and argumentation between opposing opinions, which had been begun by Zeno, and was practised by Socrates in his indiscriminate cross examination of the Athenian citizens; from Socrates it was adopted by Plato. "A string of objections never answered, and of difficulties without solution, may appear to many persons nugatory, as well as tiresome. To Plato they did not appear so. At the time when most of his dialogues were composed, he considered that the search after truth was at once the noblest occupation and the highest pleasure of life." This negative procedure involved with it the assertion of an unfettered judgment upon all questions whatsoever; in short, perfect toleration of differences of opinion. For this Socrates cheerfully suffered martyrdom. Plato also, in his negative days, made noble appeals on behalf of freedom of thought. From intense fellow-feeling with the same cause, Mr. Grote dwells emphatically upon this aspect of Plato. "The orthodox public do not recognize in any individual citizen a right to scrutinize their creed, and to reject it if not approved by his own rational judgment." "'Nomos' (Law and Custom), 'King of all,' exercises plenary power, spiritual as well as temporal, over individual minds, moulding both emotions and intellect according to the local type." To the protest by Socrates and Plato against this despotism in the ancient world, Grote, on every opportunity, adds an equally strenuous remonstrance against the like despotism in modern times.

From a very early period of life he had studied profoundly the influence of men's feelings or emotions in corrupting their sense of truth. Several of his most famous disquisitions have consisted in the illustration of this subject, more especially his handling of the Grecian Legends and his chapters on Socrates. As an historian of philosophy, he habitually traces the operation of this bias, which he held to be very far from extinct in the present day, although less often present in Physical science than in Ethical and Psychological doctrines.

At the age of 70 he commenced his work on Aristotle. Neither he nor any of his intimate friends could detect the slightest falling off in his intellectual vigour, yet he was painfully conscious of a diminution in his working pace; so that after six years he had executed but a fraction of his survey of the Aristotelian writings. What he has left amply maintains his reputation gained by the 'Plato.' He has exhaustively analyzed, in his peculiarly luminous style, the whole of the treatises comprised under the 'Organon,' being all the works that have been the fountains of the common scholastic logic—*Categories*, *De Interpretatione*, *Analytica priora* and *posteriora*, and *Sophistic Elenchi*; together with an elaborate account of the large treatise, the *Topica*, seldom adverted to, but valuable in his eyes, as exhibiting the methodized Dialectical debate of the Athenian schools.

He has also given an introductory sketch of the *Metaphysica*, together with a full abstract of the greater part of it. Of the physical treatises which he coupled with the *Metaphysica*, he has given an analysis of *De Cœlo*. He had previously published a careful account of the peculiarly difficult and important treatise *De Anima*.

Mr. Grote was one of the original founders of the London University, afterwards called University College, and was an active member of Council from the commencement, in 1827, up to the year 1831. He entered with zeal into the scheme, as proposing to impart an education that should be at once extensive and unsectarian. He again joined the Council in 1849, and from that time till his death took a leading part in the administration of the College. In 1860 he became Treasurer; and on the death of Lord Brougham, in 1868, he was elected President. He has bequeathed to the College six thousand pounds, the interest of which is to be paid to the Professor of Philosophy of Mind and Logic for the time being, on the conditions that his teaching is satisfactory, and that he is not a minister of religion.

His was one of seven names added by the Crown, on the 19th of March, 1850, to the Senate of the University of London, the others being Lords Monteagle and Overstone, Sir James Graham, Thomas B. Macaulay, Sir George Cornewall Lewis, and Henry Hallam. From the date of his appointment he gave unremitting attention to the business of the Senate, entering into every question that arose, and taking a lead in the most critical decisions of the University during the twenty-one years of his connexion with it. The first subject of great importance that came up after Mr. Grote's appointment was the admission of the Graduates to a position in the government of the University. On the 26th of February, 1850, there was laid before the Senate a declaration and statement, signed by 361 Graduates, desiring that the Graduates might be admitted into the corporate body. This was the commencement of a protracted agitation and struggle, terminated, in 1858, by the issue of a new Charter, which conceded what had been so long fought for. Mr. Grote cordially supported the claims of the Graduates. His aid in this cause was warmly acknowledged in a resolution of the Annual Committee of Convocation passed shortly after his death.

On the 1st of February, 1854, a memorial was presented to the Senate, signed by eleven persons (including Sir Rowland Hill and his three brothers, and Mr. William Ellis), in favour of throwing open the degrees of the University to all classes, irrespective of the manner or place of their education. On the 5th of April was presented another petition to the same effect, more numerously signed. No notice appears to have been taken of these applications. On the 19th of November, 1856, the Senate admitted the London Working Men's College among the affiliated colleges of the University. Mr. Grote opposed this step, on the ground that so long as the University required attendance on classes, a line should

be drawn between those who could give up their whole time to study and those that spent their day in industrial avocations. He had been hitherto favourable to the combining of certified class instruction with examinations as requisites to the degrees. The admission of the Working Men's College (carried chiefly by members of Senate opposed to the restricting of the degrees to students in the affiliated colleges) shook his faith in the value of the class certificates. About the same time it became known to the Senate that certificates were granted by some of the affiliated colleges on mere nominal studentship. This completed the conviction in Mr. Grote's mind that the degrees should be thrown open, and granted on the exclusive test of examinations. Accordingly, when the subject came up in connexion with the Draft Charter, by which the Graduates were to be admitted, he supported the insertion of a clause for abrogating the original constitution as to affiliated colleges; which clause was carried in the Senate by a large majority. Many remonstrances followed this decision, especially from the affiliated colleges. The Senate entrusted to Mr. Grote and Mr. Warburton the drawing up a report on these remonstrances, which was presented to the Senate on the 22nd of July, 1857. This report was Mr. Grote's composition, and contains an exhaustive discussion of the arguments of the remonstrants.

On the 8th of July, 1857, while the Draft Charter was under discussion, a memorial was laid before the Senate, signed by twenty-four men of science, Fellows of the Royal Society, suggesting the institution of degrees and honours for proficiency in mathematical and physical science. On the 14th of April, 1858, the Senate appointed a Committee to consider the propriety of establishing degrees in science; of this Committee Mr. Grote was a member, along with the Chancellor, Mr. Warburton, Sir James Clark, Dr. Arnott, Mr. Faraday, Mr. Brande, Mr. Walker, and Mr. Hopkins. The Committee held a series of meetings, and examined the memorialists individually as to their views and wishes, and afterwards drew up a report in favour of the principle of degrees in science. Being reappointed by the Senate to prepare a definite scheme, the Committee agreed that there should be a Bachelor's Degree, which should rest on a broad and comprehensive basis of scientific acquirement, and a Doctor's Degree for eminence in special branches. A draft-scheme for the several Degrees was, at the Committee's request, prepared by Dr. Arnott. It was very much owing to Mr. Grote's advocacy that the Moral Sciences were retained in the programme, several members of the Committee being disposed to limit the subjects of examination to the Physical and Natural-History Sciences. On the 7th of July, the Senate, with one dissentient voice, adopted the report. The degrees were instituted accordingly. The other universities are slowly entering upon a similar course.

On the 27th of February, 1862, he succeeded Sir John Lefevre as Vice-Chancellor.

It was a singular testimony to the largeness of his views, that Mr. Grote's

life-long classical studies and associations left him free to appreciate fully the great importance of science in education. In point of fact, however, he combined with his own erudite pursuits an intense avidity for the physical sciences, along with the metaphysical, and they formed a considerable portion of his reading to the last. He uniformly resisted all proposals to limit the study of logic and moral philosophy, or to lower its position in the degrees where it had obtained a place; and, generally, he was an advocate for the breadth of culture maintained in the Matriculation and Degree Examinations, as contrasted with the restricted number of subjects required for the Oxford and Cambridge degrees.

He bequeathed his library to the University.

On the death of Hallam, in 1859, Mr. Grote became a Trustee of the British Museum, and, as in the case of University College and the University of London, he gave unfailing attendance on the meetings of the Trustees, and sat on the most laborious Committees. In 1864 he was also elected a Trustee of the Hunterian Museum. His election to the Royal Society was in 1857.

He was pressed by Mr. Gladstone to accept a peerage, that he might take part in the business of the House of Lords; but he declined to enlarge the sphere of his active duties.

The disease that cut him off made its appearance last December. It did not obscure his faculties or impair his usual tone of spirits, and he received in his own house the Committees of the University of London, transacted business, and conversed with his friends till the last three weeks of his life. He died on the 18th of June, and on the 24th was interred in Westminster Abbey.

Mr. Grote's published writings, coupled with the barest outline of his life and work, reveal the lineaments of a great character, the intellectual and the moral ingredients supporting each other. In his public career, and in his wide literary research, a clear, powerful, and originating intellect was guided by the purest aims and the most scrupulous arts. With scholarly resources of language, his rhetoric is the servant of truth. At all points merging his own self-importance, he never passes by opposing considerations, and does justice to every rival. The reverse of sanguine as to human progress, he yet laboured for every good cause that satisfied his mind,—science, education, and the self-acting judgment of the individual. Differing in many points from the prevailing opinions of the time, he avoided giving needless offence, and cooperated with men of all shades of doctrine, political and religious. In the depths of his character there was a fund of sympathy, generosity, and self-denial rarely equalled among men; in the exterior, his courtesy, affability, and delicate consideration of the feelings of others were indelibly impressed upon every beholder; yet this amiability of demeanour was never used to mislead, and in no case relaxed his determination for what he thought right. Punctual and exact in his engagements, he inspired a degree of confidence and respect which acted most beneficially on

all the institutions and trusts that he took a share in administering; and his loss to them must be accounted a positive calamity.—A. B.

Dr. Augustus Waller was the son of Mr. William Waller, of Elverton Farm, near Faversham in Kent, and was born on the 21st of December, 1816. The family left England to reside at Nice, where Mr. Waller died when his son was only fourteen years old. The son came to England, and lived for some time with Dr. Lacon Lambe, of Tewkesbury, but afterwards resided with and studied under Dr. William Lambe, who was a well-known vegetarian; and the youth was brought up to the age of eighteen on a purely vegetable diet, his father having been a strict disciple of Dr. Lambe.

Augustus Waller went through his course of medical study at Paris, and took his degree of M.D. in that University in 1840, when he was twenty-four years of age. In 1842 he entered upon general medical practice at Kensington, and two years later married Matilda, only daughter of John Walls, Esq., of North End, Fulham, who survives him.

But while steadily following his vocation as a medical man, his mind was irresistibly bent on scientific investigation, and what time he could spare from attendance on patients was employed in carrying on original researches in physiology and physiological anatomy. Accordingly during this period of his career he published several important contributions to those sciences, two of which obtained a place in the Philosophical Transactions*; and he was elected a Fellow of the Royal Society in 1851.

Meanwhile his scientific pursuits came to engross more and more of his time and thought, and, after a few years, he resolved to give up his practice, which had already become considerable, and to devote himself to science as the main occupation of his life. In pursuance of this determination, and with a view to meet with more favourable opportunities of carrying on his scientific work, he left England in 1851 and took up his residence for some time at Bonn. The influence of his more favourable position soon became manifest. In association with Professor Budge, he engaged in experimental inquiries on the influence of the nervous system upon the motion of the iris, by which it was shown that the influence of the cervical portion of the sympathetic nerve in maintaining the dilatation of the pupil is derived from a particular region of the spinal cord; and fresh light was at the same time thrown on the constitution and central relations of that nerve. These researches were made known in three memoirs published in the 'Comptes Rendus' for 1851 and 1852, in the joint names of the authors, and were honoured with the award of the Monthyon Prize of the Academy of Sciences for 1852. Dr. Waller subsequently carried on

* "On the Minute Structure of the Papillæ and Nerves of the Tongue of the Frog and Toad," Phil. Trans. 1849; and "Experiments on the Section of the Glosso-pharyngeal and Hypoglossal Nerves of the Frog, and observations on the alterations produced thereby in the Structure of their Primitive Fibres," Phil. Trans. 1850.

his experimental investigations independently, and had an important share in the discovery of the influence of the sympathetic nerve on the contractility of the blood-vessels of the head; and he first demonstrated, by positive experiment, the relation of the cilio-spinal region of the spinal cord to the nerves governing these vessels.

In the 'Comptes Rendus' for 1851 Dr. Waller gave an account of a New Anatomical Method of investigating the nervous system. When a nerve is cut across, the distal portion undergoes disorganization; its fibres become entirely altered in their microscopic characters, and may thus be distinguished from those of uninjured nerves. Dr. Waller took advantage of this effect of section as a means of tracing and discriminating the fibres of a given nerve when mixed with fibres of other nerves in their peripheral distribution. Moreover he showed that this method might be used to elucidate the functions of nerves; for the disorganization of nerve-fibres consequent on their section involves the loss of their functional properties, and accordingly, when a nerve contains fibres from two or more sources, its function may be analyzed, as it were, by the separate section and disorganization of its different tributaries, and the consequent elimination of the special operation of each from the general effect.

The introduction of this method must be regarded as one of the most valuable contributions of recent times to the advancement of our knowledge of the nervous system; and it was not only happily applied by himself in several neurological investigations, but it has been eagerly and successfully taken up by others, who gladly acknowledged its authorship, and have recognized it as the "Wallerian method." In the course of these researches Dr. Waller also discovered that the nervous ganglia exert a certain influence in maintaining the integrity of nerve-fibres which are connected with them. In recognition of his merit in devising and applying this new method of investigation, the French Academy of Sciences conferred on him the Monthyon Prize for the year 1856, and the President and Council of the Royal Society awarded him one of the Royal Medals for 1860.

From Bonn Dr. Waller went to Paris to continue his labours; but he there contracted a low form of fever with severe cerebral symptoms: this was in 1856; and he was so much prostrated by the effects of this illness, that he was for a long time quite incapacitated for work. He accordingly returned to England, and passed the succeeding two years in comparative repose; but, his health having improved, he accepted the appointment of Professor of Physiology in Queen's College, Birmingham, and the place of Physician to the Hospital, and again resumed his scientific investigations. These appointments, however, he did not long retain. Threatenings of the heart-affection which eventually proved fatal to him, induced him again to seek for quiet, and, after two years' longer stay in England, he retired to Bruges, and afterwards to the Pays de Vaud. With renewed promise of health and activity, he in 1868 took up his abode in Geneva with the purpose of practising there as a physician. In the cultivated scientific circles

of that city Dr. Waller found agreeable companionship. He was elected a Member of the Société de Physique et d'Histoire Naturelle, and again applied himself with zeal to scientific work. Having been appointed to deliver the Croonian Lecture before the Royal Society in the spring of 1870, he made a short visit to London for that purpose. The subject was "On the Results of the Method introduced by the Author of investigating the Nervous System, more especially as applied to the elucidation of the Functions of the Pneumogastric and Sympathetic Nerves," and it appears to have been his last work. He had for several years been subject to attacks of angina pectoris, and the accession of a fit of more than usual severity caused his death on the 18th of September, 1870.

Whilst Dr. Waller's researches and discoveries concerning the nervous system constitute his most conspicuous claim to distinction as a man of science, he successfully took up other subjects of physiological inquiry. As especially worthy of note may be mentioned his observations on the escape of white blood-corpuscles from within the small vessels*. This remarkable fact had been already pointed out by Dr. William Addison, and its relation to the nutrition of tissue, as well as to secretion, inflammation, and suppuration, instructively discussed by him. Dr. Waller described the corpuscles as actually piercing the wall of the containing vessel, as if by some softening or solvent operation. The subject has again been brought forward by Dr. Cohnheim of Breslau, to whom his countrymen are for the most part in the habit of assigning the discovery.

Dr. Waller was endowed with a remarkable aptitude for original investigation. Quick to perceive new and promising lines of research, and happy in devising processes for following them out, he possessed consummate skill and address in experimental work. During his early studies in Paris he had paid much attention to physical science, and this stood him in good stead in his future investigations. By those who knew him personally, he was much esteemed for his moral qualities, as well as for his intellectual endowment. His sudden death, before he had completed his fifty-fourth year, whilst the cause of poignant grief to his relatives and friends, must also be accounted a serious loss to physiological science.

The Rev. WILLIAM VENABLES VERNON HARCOURT, M.A. Oxon., was born in June 1789, close to the ancient home of his family at Sudbury, in the Rectory House, then occupied by his father, the Hon. and Rev. Edward Venables Vernon, who became Bishop of Carlisle, and afterwards Archbishop of York, and took the name of Harcourt on acceding to the property of the deceased Earl. Mr. Harcourt, the fourth son in a family of sixteen children, had the advantage of his father's instruction, and did not proceed to a public school.

His first destination was for the Navy, in which he served for five years; but then his literary tastes and predilection for the church prevailed, and

* Phil. Mag. 1846.

he entered Christ Church, Oxon., with the advantage of the personal friendship of Cyril Jackson, the Dean. At that time Conybeare and Peel were conspicuous members of his College, and the University was leading to eminence Buckland, Keble, and Whately. Dr. Kidd had been for some years an admirable teacher of Chemistry, and to this attractive subject (to which his attention had first been drawn by Dr. Isaac Milner, who was Dean of Carlisle when his father was Bishop of that diocese) Mr. Harcourt clung with affectionate fidelity through all his subsequent life.

Soon after leaving the University (in 1811) Mr. Harcourt began his duties as clergyman at Bishopthorpe, close to his father's residence, and speedily manifested the good effects of his Oxford career by associating himself with the movement then beginning in Yorkshire, in favour of institutions for the cultivation of science. He constructed a laboratory, and became greatly occupied in chemical analysis, not a little aided and encouraged in this pursuit by his early friends, Davy and Wollaston. The latter explained to him some of the methods of qualitative analysis on a small scale, in which he was unrivalled. The great ideas of the former he kept steadily in view. From Buckland and the brothers Conybeare he acquired a settled partiality for the then rapidly advancing science of Geology.

In 1821 the famous cavern of Kirkdale was opened in Yorkshire, and zealously explored by Buckland. Some of the treasures of this rich repository of prehistoric life-forms were divided among several explorers in Yorkshire, and three of these, viz. Mr. Atkinson, Mr. Salmond, and Mr. Thorpe, concurred in a resolution to reunite them in one collection, as a basis for a Yorkshire museum of natural history and antiquities. Of the institution which was established in consequence of this arrangement, the "Yorkshire Philosophical Society," Mr. Harcourt was chosen President, and by all the means at his disposal extended its influence, and animated and directed its energies.

The geology of Yorkshire had begun to attract attention; and one of the earliest engagements for public lectures was contracted in 1824 with Mr. Smith, the author of the first geological map of the county, as well as of the first map of the strata of England and Wales. In 1826 Mr. Phillips was appointed to be the keeper of the Yorkshire Museum, then contained in a small house; but from this time it grew so rapidly as to require the erection of a spacious building, with library, lecture-room, and laboratory, at a cost of £9000. During many years Mr. Harcourt and his younger friend just named might be often met engaged in geological explorations*. The laboratory, now removed to Wheldrake, was never unemployed†; the monthly meetings of the Society heard always from

* See "Geology of the district round Caves," in Phil. Mag. May 1826; and 'On a Deposit of Bones at Northcliff,' 1829.

† See papers on Phosphates of Lead, on Scarbroite, on Lapis Lazuli, on Cadmium in Zinc Ore, &c., in Phil. Mag. & Ann.

the President some useful notices of the progress of science, and efforts, by no means unsuccessful, were made to spread through Yorkshire a spirit of local inquiry, which is still active in that large natural district.

It was fortunate for the British Association that its constituent meeting was, by advice of Sir David Brewster, arranged at York, by the ready zeal of a Society so active and under such good auspices. At this meeting (in 1831) the general plan of proceeding, and the essential basis of the Association, were drawn up by Mr. Harcourt, and proposed on the part of the Society which he represented. Discussed and accepted by a body of 367 members, among whom Murchison was conspicuous, they have remained practically the same, though in many points improved by experience through forty years of work.

Over this increasing enterprise Mr. Harcourt, as General Secretary, watched with unceasing vigilance for several years, and found many occasions for the employment of his personal influence in furthering the advancement of science, both by consultation with its acknowledged leaders and by appeals to the members of the Government. Elected President of the Association at Birmingham in 1839, he took occasion to discuss very fully the history of the discovery of the composition of water, supporting the claim of Cavendish by original documents, which were published in the annual volume. Not less remarkable in this address was the resolute vindication of the claim of science to entire freedom of inquiry into the constitution of nature, and a high-minded rebuke of the narrow views which refused to accept geological and astronomical truths on account of a supposed conflict of these with particular passages in the Bible—passages of matchless grandeur and beauty, but not destined to teach or control the teaching of the principles of natural science. Breathing, as we do now, the freer air of advancing inquiry, there may be some danger of forgetting the obligations we owe to churchmen such as Harcourt and Conybeare, who boldly employed their great influence to resist the heavy pressure of well-meaning but ill-reasoning theologians, who sometimes appeared to forget that they were not endowed with "supernatural knowledge of the mysteries of nature."

One of the subjects to which Mr. Harcourt directed his attention with success was the effect of heat on inorganic compounds. With a view to extensive experiments on the effects of high heats in a manageable form, he began the construction of a furnace for burning hydrogen gas under great pressure; and by the aid of Sir William Fairbairn and the late Mr. Bryan Donkin, this furnace was provided with pressure-gauges, and became a safe and manageable instrument. It was employed for many years in the processes of fusion required for the production of various kinds of glass for optical uses*. Following similar ideas, he assisted as a Member of a Committee of the British Association in the examination of furnace-slags, on which the Report was presented by Dr. Percy†. As

* Reports of the British Association, 1844. † *Ibid.* 1846.

early as in 1834 another train of researches on the same general subject was set on foot by Mr. Harcourt, when he made arrangements for trying the effects of long-continued heat on rocks and minerals. For this purpose the furnaces of Low Moor, in Yorkshire, were employed by placing under them a large and varied series of rocks and minerals, and artificial mixtures selected and weighed to represent various natural crystals interesting to geologists in connexion with metalliferous veins and metamorphic rocks. The furnaces being usually kept in action for several years, it was expected that the long-continued and moderate heat would be sufficient to fuse some metals, and probably cause new combinations of oxides, silicates, or sulphides.

These operations gave indeed such results; and though, contrary to expectation, the direct influence of high temperature was found to be too great, the conglutination of sand to quartzite, the formation of iron veins, the occurrence of cavities lined with a web-like silicate, and what was almost a chaos of changes and displacements, showed the power of the agencies employed. It is much to be desired that those curious experiments should be repeated with precautions which are suggested by those on record *.

Accustomed to the use of the gas-furnace, Mr. Harcourt turned it to experiments on transparent compounds of fusion, which might be made to have refractive indices beyond the ordinary ranges, combined with scales of dispersion more favourable to achromaticity. In this he was guided by the trials of Faraday to prepare glass for optical purposes. Many years since, the writer, who was often helpful in this way, ground one of the earliest of the Harcourt glasses into a lens, and found it indeed a highly refractive clear substance, but too much traversed by striæ to be of practical use.

When, some years since, Mr. Harcourt removed his residence to the family seat at Nuneham, near Oxford, he constructed furnaces of a different kind for the carrying on of these experiments, and followed them with the zeal, resolution, and patience which had always characterized his firm and well-regulated mind. At an age when most men cease from continuous literary and scientific work, he with failing sight, but perfect memory, was indefatigable in training an assistant and superintending his work; making many new combinations with substances untried before, and now selected for quality of fusion, resistance to atmospheric vicissitudes, range of refraction and specific action on different rays of the spectrum. Thus it was hoped finally to acquire glasses of definite and mutually compensative dispersions, so as to make perfectly achromatic combinations. After innumerable trials, and the production of glass of extremely various quality, Mr. Harcourt, continuing his inspection to the last, had the satisfaction of believing that, though he could not remain to witness it, a good result had been assured, and that Professor Stokes, to

* British Association Reports, 1860.

whom all the specimens were submitted for scrutiny, would be able to construct a lens of sufficient size to be fairly tried, and thus crown the long-continued labour with a permanent benefit to science.

It was hoped that a full account of these experiments might have been prepared by the author of them, for his strong mind felt little of the weight of eighty years and overruled the bodily infirmities of age. But it was a character of the man never to cease experimental or literary research till he was satisfied; resolute to contend with difficulties till all were overcome, and too truly a lover of knowledge, with faith in its progress, to be hasty in publishing views on account of their novelty, which might be made valuable by proofs of their truth. Prof. Stokes has already presented to the British Association a notice* of these researches, and to him we must now look for further records of a work to which he has cheerfully contributed a large amount of valuable aid.

The scientific pursuits of Mr. Harcourt were followed in the midst of great occupation as a clergyman, not only in charge of his parish, but open to perpetual demands for help in public institutions of an educational and charitable character. The York School for the Blind, founded in honour of Wilberforce, the Yorkshire Hospital, the Castle Howard Reformatory, experienced the benefit of his guidance; indeed hardly any great movement in Yorkshire in favour of useful learning and comprehensive Christianity was carried on without his help, often given when his own health required cessation from labour. At many public meetings for these objects his place was to preside—a duty for which his thoughtful words and dignified presence, and a certain natural union of gentleness and firmness, admirably qualified him.

Though never a person of robust health, Mr. Harcourt was not much troubled by positive illness till towards the close of his life, when he became confined to a home rich in books and monuments of art, surrounded by a cheerful family, to which the graceful hospitality which had become a habit of his life brought many additions from the large range of his personal friendships and the more limited circle of men devoted to literature and science resident in his own neighbouring University. He had been elected a Fellow of the Royal Society in 1824. His death occurred at Nuneham in April 1871.—J. P.

In JOHN FREDERICK WILLIAM HERSCHEL British science has sustained a loss greater than any which it has suffered since the death of Newton, and one not likely to be soon replaced. Though none of his discoveries were as brilliant as Davy's decomposition of the alkalies, or Faraday's magneto-electric induction, yet they ranged over a wider field than either of these philosophers could explore, and many of them are of first-rate importance. And of even higher value was the influence of his teach-

* Read to the British Association Meeting in Edinburgh, August 1871.

ing and example in wakening the public to a perception of the power and beauty of science, and stimulating and guiding its pursuit.

He was born March 7, 1792, at Slough, a spot consecrated by the work of his illustrous father; an only child, he grew up with nothing to weaken the effect of the glorious pursuits and magnificent objects with which he was familiar. He was educated at home, but most successfully. He was a good classical scholar, a poet, an accomplished draughtsman, an excellent musician; he spoke fluently several foreign languages, and was well versed in their literature. In 1809 he entered St. John's College, Cambridge, through which he passed with the highest honours, graduating as Senior Wrangler above Peacock. In conjunction with Peacock and another distinguished man of the following year, Babbage, he took an active part in a controversy then raging at Cambridge. From reverence to the memory of Newton, from limited intercourse with the continent, and perhaps from national prejudice, British mathematicians had advanced scarcely a step beyond their great master; and it was scarcely possible for them to do so while they retained the Fluxional notation.

If we compare the treatises of Fluxions which at the time in question were current in the University with the French or German text-books on the same branch of analysis, we must be thoroughly ashamed, and almost disposed to admit the contemptuous statement of an Edinburgh reviewer, that there were in Great Britain only four men who could read the 'Mecanique Céleste,' and that three of them were Scotchmen*. In all such transitions, besides the effect of habit, the adherents of an old theory are often bound to it by personal feelings, as if the giving up their former convictions implied some intellectual inferiority; and it may happen that the champions of the new one do not bear their triumph meekly. At Cambridge, however, the struggle was not long; for Woodhouse, the originator of the movement, was powerfully seconded by Herschel and his friends even while undergraduates. From them came the memoirs of the Analytical Society, the translation of Lacroix's 'Differential and Integral Calculus,' and the examples of the same calculus, which virtually decided the question. The treatise on the calculus of finite differences which this last volume contains, and which was the work of Herschel exclusively, is specially valuable. Henceforward he entered on a wider field of labour, where the limits of this notice do not permit us to follow him; for of him it may be said, as in Goldsmith's epitaph, "Scientiæ nullum non genus tetigit, nullum quod tetigit non ornavit." For sixty years he enriched the Royal and other Societies with memoirs, precious not merely for the truths which they reveal, but for their suggestive character and their lucid developments. Their number and quality might seem great enough to overtask the energy of most men, yet they were supplemented by several sepa-

* Probably he meant by the three, himself, Brougham, and Ivory; of Ivory this was true, but the competence of the other two for such a feat is very questionable.

rate treatises of great importance*, and by a profusion of lectures, reviews, and other contributions to periodical literature†. Out of this wonderful collection a few specimens may be selected.

Of all his writings, that which has been most universally admired and whose influence has been most widely felt is the "Discourse on the Study of Natural Philosophy," which in 1830 appeared in 'Lardner's Cabinet Cyclopædia.' For power and elegance of language, for clearness of illustration, for sound and far-sighted judgment, this little book cannot be surpassed. Of course the progress of research enables us now to see further in some directions than he could then anticipate, and the principles enunciated in the first chapter have roused the ire of some of the philosophists of the day. They, however, are proof against the cavils of such rash speculators, for they are in perfect unison with the noblest aspirations and highest intuitions of human nature. Of his purely physical works the astronomical portion must hold the first place as a stupendous monument of unwearied labour, guided and enlightened by high theoretic anticipations and consummate skill.

As his father's greatest triumphs were achieved in the regions of double stars and nebulæ, it was natural that his thoughts should be turned in the same direction, and that he should be impelled to complete as far as possible what Sir William had left imperfect. As early as 1821 he is found associated with Sir James South in forming a Catalogue of Double Stars, which appeared in our 'Transactions' for 1824, and was honoured with our medal. This union of labour was soon interrupted. South moved his observatory to Passy in quest of a clearer atmosphere than that of Blackman Street. Herschel also went to the continent. His friend Babbage had been overwhelmed by a domestic calamity, and Herschel abandoned his own pursuits to aid time and change of scene in alleviating the affliction of his companion‡.

On his return to Slough he resumed the double stars, and by 1832 he had observed 5075 of these objects, a large portion of which were new to astronomers. Most of these were observed with the 20-feet Herschelian reflector; and only they who have experienced the difficulty of obtaining good measures of a double star even with a steady equatorial can rightly appreciate the merit of such work performed with an instrument mounted as this was (see the drawing of it in the frontispiece of the 'Cape Observations'). The results were published in six memoirs, the last of which

* Two of these are of great value,—that on sound in the 'Encyclopædia Metropolitana,' and that on telescopes in the 'Encyclopædia Britannica' (this contains a description of the polishing-apparatus of his father).

† He was often requested by the Council of the Royal Society to report on papers submitted to it for publication. Some of these Reports which the writer has seen are so full of valuable matter as to make him regret that the rules of the Society do not permit their publication.

‡ In their tour they were received with great kindness by Laplace, who was fully cognizant of what they had done at Cambridge, and referred to it with much pleasure.

did not appear till 1836, for the necessary reductions (in which he had no assistance except from his aunt, the celebrated Caroline Herschel) required much time and labour.

His catalogues differ from those of his father in one important particular: they give the place of an object by its right ascension and polar distance for 1830, so that it can be easily found by a graduated instrument; whilst Sir William gave only its distance and angle of position with respect to the nearest of Flamsteed's stars, both of them mere estimates—a plan which, besides its uncertainty, was necessarily tedious.

But along with this great work he was engaged with the revision of his father's catalague of nebulæ and the discovery of new ones; and here the boon which he conferred on astronomy was still greater, such, indeed, as none but himself could have bestowed, for his telescope far exceeded any then existing in illuminating power. His earliest publication on this subject was the fine monograph and drawing of the nebulæ in Orion, dating from 1824; but this was nobly followed out by his great Catalogue of Nebulæ, which appeared in our Transactions for 1833. It contains 2307 nebulæ and clusters, of which more than 500 were discovered by himself. In this also the right ascension and polar distance of each object are given for 1830, and it is enriched with many admirable drawings of such as offer some striking peculiarity. As might be expected, some of these have been found to omit details which are shown by the instruments of greater power which have been since constructed; but their general accuracy is acknowledged by all conversant in this kind of observation; and their value is shown in more than one instance by enabling subsequent observers to detect changes in the nebulæ *.

The memoir which accompanies this catalogue is of high interest, full of vivid description, of far-reaching views, and enlightened speculation. Having so thoroughly explored the northern sky, he might have rested well satisfied with his work; but he felt the importance of extending it to the southern hemisphere, and of leaving to posterity a complete survey of the heavens, which, as made by the same observer and with the same powerful telescope, would be a record to which future observers might refer with confidence. With this view, and by his unaided resources, he removed, in the beginning of 1834, his family and instruments to the Cape of Good Hope, where he remained for four years, observing with intense activity, and exerting on the leading spirits of that colony an enlightening influence, the effects of which are still felt there. The results which he obtained were given to the world, not in separate memoirs, but in one volume, published in 1847 by the munificent aid of the then Duke of Northumberland. It contains catalogues of 2102 double stars and 1707 nebulæ, with elaborate drawings of many of the latter and minute surveys of the stars

* In comparing these drawings with the present appearance of the nebulæ, the telescope used should be made equiluminous with the 20-feet; for this purpose in a Newtonian the aperture should $=23{\cdot}14$, in an achromatic $=16{\cdot}86$.

adjacent to them. Some idea of the labour thus expended may be gained by the fact that for the two "Magellanic Clouds" 1143 stars, nebulæ, and clusters were carefully measured, and 1203 stars for the great nebula in Argo. Of the splendid monograph of this wonderful object which he has given it is impossible to speak too highly, especially as, notwithstanding some conflicting testimony, the comparison of it with recent observations gives reason to believe that this nebula has undergone surprising changes in the last thirty years. Independent of these precious catalogues the volume is like a perfect gem; besides the charm of its style, it is a rich treasury of varied knowledge. As an example may be named the section on the causes which injure the action of reflecting telescopes, the chapters on astrometry, on Halley's comet, and on the solar spots; they all bear the stamp of a Master's hand, and contain suggestions all striking, and of which some have since been applied with the best results to practice. His labours on nebulæ were completed much later in life by a general catalogue of them containing 5079, which appeared in our 'Transactions' for 1864. This includes all contained in his former catalogues and all discovered by others up to that time. It contains the places for 1860, and their precessions for 1880; so that it will be easily available for observers to the end of the century, and will long be their Manual of Nebulæ.

His contributions to optics rank next to his astronomical in importance and number; but we shall only mention two, which gave a great impulse to the progress among us of this branch of physics. The first is a remarkable memoir on the aberrations of compound lenses and object-glasses, which appeared in our 'Transactions' for 1821. Before it opticians (at least in this country) corrected the spherical aberration of their object-glasses by empirical rules, and its theory was given in rude and unsymmetrical forms. By a happy choice of symbols and an elegant analysis he presents the theory of aberration in all its generality, and in as simple a manner as the nature of the question admits. He gives examples of the application of his theory to the construction of aplanatic doublets, and then to that of object-glasses. In discussing this he considers the dispersive power as composed of several terms, of which the first only is taken into account by opticians, and the rest constitute the irrationality of the spectra. This defect cannot be removed in a double object-glass unless the dispersive powers of higher orders are proportional to the first; and he recommends that the attention of future inquirers should be directed to the discovery of such a medium (a result which there is reason to believe has at last been obtained by the combined investigation of the late W. Vernon Harcourt and Professor Stokes).

The condition which he assumes for correcting the spherical aberration is, that the compound shall be aplanatic for near as well as remote objects. He tabulates the results for the various crown and flint glasses then used in England with a completeness which leaves nothing to be desired. It may be feared, however (notwithstanding the popular explanation of his method

which he afterwards published in the sixth volume of the 'Edinburgh Philosophical Journal'), that this paper has had little influence on the practice of British workmen, who then and now are far behind the requirements of the age in the knowledge of geometry.

It is evident that when he wrote this memoir Herschel did not accept the wave-theory; but in 1830 appeared in the 'Encyclopædia Metropolitana' his Treatise on Light, where it is brilliantly developed. In this work, which is still read with admiration and profit, the first part is an excellent system of ordinary optics, in which is included the substance of the previous memoir: then he states the emission and wave-theories of Light, applies each of them to explain the laws of reflection and refraction and the phenomena of Newton's rings and diffraction fringes, states the objections to each, and shows the superiority of the latter as irresistible. He next describes with singular clearness the phenomena of Polarized Light (not a few of which were discovered by himself), and gives their theory; and completes the work with the absorption of light and the effect of mechanical force on the optical properties of transparent media.

In other departments of science it must suffice to notice his investigations of the hyposulphites, to which photography is so deeply indebted; his researches on photography, including some curious and beautiful processes, and of which it may be remarked that he very nearly anticipated Mr. Fox Talbot in the discovery of the paper process; and his invention of the Actinometer. Nor is to be forgotten that on several occasions he gave powerful aid to the establishment of those simultaneous magnetic surveys which, under the auspices of Sir Edward Sabine, have thrown such light on the study of terrestrial magnetism.

It remains to say a few words as to his personal character; for a man, however high and varied his knowledge of physics or geometry, is a very imperfect specimen of his race if he be deficient in those higher qualities which should regulate morals and the duties of life. Herschel could abide this test. He was deeply and unostentatiously religious; exemplary in all the social relations. If his devotion to his father might seem excessive, it should be remembered what a man that father was. Far above the petty jealousies which haunt meaner minds, he was always ready to do the fullest justice to the labours of others, regarding them as fellow-labourers in a great work, not as rivals; and when it was his lot to be engaged in controversy, which he avoided as much as possible, he never allowed it to hurry him into any thing unworthy of a Christian gentleman. A beautiful trait in his character was the interest he took in those who, as yet unknown, were beginning to climb the ascents of science, and the encouragement he was always ready to afford them. One instance of this which it was the writer's lot to witness struck him so much that he ventures to mention it here. At an early meeting of the British Association a rough-looking man brought forward a paper on the strength of iron; unaccustomed to such an audience, his presence of mind failed, and he sat down muttering

"I can't go on." He sat down next to Herschel, and another paper was proceeded with; but Herschel after a little began to whisper to him: at first, in his agony of confusion, he seemed not to hear; but Herschel persevered, asked what his views were, spoke well of them, and, as his countenance brightened, asked leave to look over his paper, and having done so, encouraged him to bring it forward next day. But for this wise kindness that man might have relapsed into obscurity; but, as it was, he became ultimately a Fellow of our Society, and one of the highest authorities in a very important branch of mechanical engineering.

Herschel married Miss Stewart in 1829, and had a large family, of whom two sons are well known to us—Professor Alexander by his study of Meteorites, and Captain John, attached to the Indian Survey, distinguished by spectroscopic observations of the sun and other heavenly bodies.

He was created a Baronet in 1838. In 1850 he, as Newton before him, was appointed Master of the Mint. That establishment was undergoing an important change, its emancipation from the old Corporation of Moneyers; and the whole of its reorganization devolved on him. He also introduced an important check, which, however, has been discontinued since his successor's death. The official assays were controlled by others, made by two unofficial chemists, those whom he employed being Graham and Allen Miller. This office his failing health obliged him to resign after five years.

He was a President of the British Association and of the Astronomical Society. In 1855 he was nominated one of the eight Foreign Associates of the French Academy of Sciences, and was a member of almost every Philosophical or Literary Society of Europe.

His mind continued as clear and active as ever to the very last. He died on the 11th of May, 1871, and he was buried in Westminster Abbey by the side of Newton.—T. R. R.

William Baird, M.D., F.R.S., F.L.S., &c., was the youngest son of the Rev. James Baird, and was born at the Manse of Eccles, in Berwickshire, in 1803. He was educated at the High School of Edinburgh, and afterwards studied medicine and surgery in the University of that city, and in Dublin and Paris.

In the year 1823, Dr. Baird, having previously made a voyage to the West Indies and South America, entered the maritime service of the East-India Company as surgeon, and remained in that service until 1833. During this period he visited India and China five times, also other countries; and in all his voyages availed himself zealously of the opportunities for studying his favourite science of Natural History which his position presented to him. In 1829 Dr. Baird assisted in the foundation of the well-known Berwickshire Naturalists' Club (to the publications of which he was afterwards a frequent contributor). The admirable example afforded by that institution has led to the formation of similar Associations

in other parts of the United Kingdom, and has thus contributed greatly to advance the knowledge of the natural history of particular districts, as well as to spread a taste for this attractive science generally throughout the country.

On quitting the East-India Company's service, Dr. Baird practised his profession in London for some years, until, in 1841, he accepted an appointment in the Zoological Department of the British Museum, where he remained until his death, on the 27th of January, 1872.

Dr. Baird's qualifications as a zoologist were of a high order, and his published writings are numerous and valuable: they consist chiefly of scattered papers on various subjects in the 'Edinburgh Philosophical Journal,' 'Loudon's Magazine of Natural History,' and its successor 'The Annals and Magazine of Natural History,' in the 'Zoologist,' and the 'Proceedings of the Zoological Society.'

His most important work, however, is the 'Natural History of the British Entomostraca,' published by the Ray Society in 1850. It contains an excellent account of the structure, physiology, and habits of the minute Crustacea which swarm in such abundance in our fresh and salt waters, and is justly regarded as a work of great ability and research.

He was also the author of a popular 'Cyclopædia of the Natural Sciences,' published in 1858, and of a valuable paper "On Pearls and Pearl Fisheries," as well as one "On the Luminosity of the Sea."

During the latter years of his life his attention was principally given to the Entozoa, of the known species of which he had, as early as 1843, drawn up a catalogue, which was published by the Trustees of the British Museum. Numerous papers on the same subject were also contributed by him to the 'Proceedings of the Zoological Society' and the 'Transactions of the Linnean Society.'

Latterly he was engaged in preparing a new and general catalogue of the Entozoa, comprehending the information acquired up to the present time. With this object he had accumulated a vast amount of material, and, had he lived to bring his undertaking to a close, it would have doubtless supplied a most valuable contribution to science.

But it is not merely by his publications that his great attainments must be judged. His knowledge of the various branches of Natural History was extensive and accurate, and his readiness in imparting it to others will long be remembered by those who were in the habit of studying in the British Museum.

As a man of science he was highly regarded by scientific men, and he was greatly esteemed for his genial and kindly nature by all who knew him. In private life especially he was much beloved, on account of the unvarying amiability of his disposition and the kindliness of his manners.

Dr. Baird was a Fellow of the Linnean Society, and Member of the Imperial and Royal Botanical Society of Vienna. He was elected into the Royal Society in 1867.

Wilhelm Karl Ritter v. Haidinger was the fourth and youngest son of Bergrath Karl Haidinger, one of the earliest cultivators of Mineralogy and Geology in the Austrian Empire, Professor of Mathematics and Mechanics in the Mining Academy of Schemnitz, who afterwards held an office in the Mint and Mining Department in Vienna, and was the author of:—Dispositio Rerum naturalium Musæi Cæsarei Vindobonensis; on certain rare fossils; on the minerals of the Wieliczka Salt-mines; on v. Born's method of amalgamation; sketch of a systematic classification of rocks; and on the minerals sapphire, ruby, and spinelle.

W. K. Haidinger was born in Vienna on the 5th of February, 1795, and was educated in the Academic Gymnasium of that city. In the autumn of 1812 he went to Gratz in order to profit by the teaching of Mohs, who in that year gave his first course of lectures on Mineralogy at the Johanneum; and about the end of the year 1817 followed him to Freiberg, sharing in his mineralogical researches, and making many new observations on the characters of several mineral species, which were never published separately in his own name. In May 1822 he accompanied Count August Breunner in a journey, undertaken in the interests of science, to France and England. In the autumn of 1823 he went to live in the house of Mr. Thomas Allan, Banker in Edinburgh, who was the possessor of a fine collection of minerals; and from the summer of 1825 till the autumn of 1826 he travelled with Mr. Allan's son Robert through Norway, Sweden, Denmark, Germany, North Italy, and France. In 1825 he published an English translation of the Treatise on Mineralogy by Mohs, with many additions and improvements. During the years 1822–1827 he contributed to the Transactions of the Royal Society of Edinburgh, the Memoirs of the Wernerian Society, and Brewster's and Jameson's Journals no less than forty papers on the geometrical and physical characters of minerals, including several newly discovered and described for the first time by himself. In 1827 he joined his brothers Eugen and Rudolph in the management of a flourishing porcelain manufactory which they had established at Elbogen in Bohemia in 1815; his eldest brother, Moritz, a lieutenant in the Austrian army, had died in 1809 of wounds received at Landshut. For thirteen years he took an active part in conducting the business of the porcelain works, but not without maintaining an uninterrupted connexion with Natural Science by making many communications to the Bohemian Academy of Science, Poggendorff's 'Annalen,' the 'Zeitschrift für Physik,' edited by Baumgartner and v. Ettingshausen, and a periodical bearing the same name edited by v. Holger; and in 1829 he published a work entitled "Anfangsgründe der Mineralogie."

In April 1840 he was appointed to succeed Mohs (who died at Agordo in the Venetian Alps on the 29th of the preceding September) as lecturer on Mineralogy and Custodian of a newly formed collection of Minerals, mainly the gift of Count Breunner, deposited in the Austrian Mint at

Vienna, known then as the collection of the "k. k. Hofkammer in Münz und Bergwesen," and later as the "Montanistische Museum." Having completed the arrangement of the collection and published a comprehensive catalogue of it, he began his first course of lectures on the 9th of February, 1843. He gave seven courses, the last being in 1849. During this period he made it one of his chief objects to encourage the members of his class, consisting chiefly of mining students, to undertake original investigations. In the year 1843 he ascertained the existence of trichoism in andalusite from Minas Geraes and in diaspore from Schemnitz, and constructed his "Dichroiscopische Loupe" for exhibiting the contrast of colours between the two fields of oppositely polarized light when certain coloured crystals are viewed through it. On the 1st of April 1844 he discovered the remarkable phenomenon of the "Polarisationsbüschel," or Haidinger's Brushes as it is more commonly named. In 1845 he published his 'Handbuch der bestimmenden Mineralogie,' and a Report upon the results of mineralogical research during the year 1843. In November 1845 the Society of Freunde der Naturwissenschaften was formed, with Haidinger as its President and editor of their publications, consisting of 'Naturwissenschaftliche Abhandlungen,' in four volumes, 4to, and 'Berichte über die Mittheilungen von Freunden der Naturwissenschaften in Wien,' in seven volumes, 8vo. On the 14th of May, 1847, Haidinger was appointed one of the forty Members of the newly founded Imperial Academy of Sciences of Vienna. A Geological Map of the Austrian Empire, prepared, under Haidinger's immediate superintendence, by the most distinguished Members of the Montanistische Museum, and revised by Franz Ritter v. Hauer, was published in the same year. The Montanistische Museum and its Collections became the nucleus of the "k. k. Geologische Reichsanstalt," an institution founded on the 15th of November, 1849, with Haidinger for its Director, and a staff of local Directors and assistant Geologists. Its province was to collect materials for elucidating the geological knowledge of the whole Austrian Empire. This institution gave a powerful impetus to the culture of the natural sciences throughout the Austrian Empire. During the seventeen years that Haidinger presided over it, he strove with painful accuracy to acknowledge the services of every one of its Members who laboured under his direction. Through Haidinger's exertions the Geographical Society of Vienna was established in December 1855, and he became its first President. To his influence also may be traced the origin of the "Werner Verein" for the geological examination of Moravia and Silesia, the "Geologische Verein" for Hungary, and the "Società Geologica" of Milan, afterwards enlarged into the "Società Italiana di Scienze Naturali."

After a long and severe illness he retired on a pension from the office of Director of the Geologische Reichsanstalt on the 7th of October, 1866, and was succeeded by his former pupil Franz Ritter v. Hauer. Though enfeebled by illness and frequently unable to quit his room, he

preserved to the day of his death a wonderful elasticity of mind, which not only enabled him to take a lively interest in every new discovery made by others, but to continue his own scientific labours. He died, after a short illness, on the 19th of March, 1871, at Dornbach near Vienna, and was buried in the cemetery of that place on the 22nd.

He was the author of at least three hundred and twenty papers on the characters of many new minerals, mineralogical optics, pleochroism, pseudomorphs, geology, meteorology, and on the freezing and breaking up of the ice of rivers. During the years 1859–1870 he wrote a number of remarkable memoirs on meteors and meteorites. These are for the most part to be found, in addition to the Journals and Transactions already mentioned, in the 'Naturwissenschaftlichen Abhandlungen,' 'Berichte über die Mittheilungen von Freunden der Naturwissenschaften in Wien,' the 'Denkschriften' and 'Sitzungsberichte der k. Akademie der Wissenschaften:' one was published in the 'Proceedings of the Royal Society' for December 1868; it is entitled "On the Phenomena of Light, Heat, and Sound accompanying the Fall of Meteorites." The last scientific papers he wrote, on the Meteorite of Meno, and on laboratory crystals of pyrite obtained by Professor Wöhler of Göttingen, were published in the 'Realschule,' a Journal edited by his son-in-law, Dr. Eduard Döll. He was Member, or Correspondent, of upwards of a hundred learned Societies, Knight of various Orders, including the Austrian Order of Franz Joseph and the Prussian of Pour le Mérite. He was elected a Foreign Member of the Royal Society in 1856. On the 29th of April, 1856, a Gold Medal, subscribed for by numerous friends, in recognition of his merits as the founder of a new scientific era for Austria, was presented to him. On the 5th of February, 1865, the seventieth anniversary of his birthday, his bust in marble, the cost of which was defrayed by the subscriptions of residents in almost every country in Europe and in many of the most distant colonies, was installed in one of the rooms of the Geologische Reichsanstalt.

Heinrich Gustav Magnus, elected Foreign Member of the Royal Society in 1863, was born on the 2nd of May, 1802, in Berlin, where his father, Johann Matthias Magnus, had founded a large mercantile establishment. At an early age he exhibited great aptitude for the study of Mathematics and the Natural Sciences, and was fortunately sent to a school in which particular care was bestowed upon instruction in the subjects which chiefly interested him. The easy circumstances of his family having enabled him to select the career that best suited his tastes and capacity, he determined to devote all his powers to the cultivation of Chemistry, Physics, and Technology. He entered the University of Berlin in 1822. Under no obligation to hurry through his Academic studies, he employed the following five years in attending lectures on Chemistry, Physics, and Mathematics. He also worked diligently in the University Laboratory, and

occupied his vacations in making mineralogical and technological excursions. He had resolved to qualify himself as a teacher of Technology, but was unwilling to undertake any definite duties without first seeking to complete his education by studying in other Universities. Accordingly in 1828 he went to Stockholm, where he worked under Berzelius, and had the good fortune to lay the foundation of a warm and life-long friendship with his master; and he visited Paris in 1829 and attended the lectures of Gay-Lussac, Thénard, and other eminent chemists and physicists. In 1831 he obtained his habilitation as a teacher of Technology and Physics in the University of Berlin, and laboured as a teacher with unexampled success for nearly forty years. He devoted the utmost care to the arrangement of his lectures, and spared neither trouble nor expense in procuring diagrams, models, instruments, and other requisites for their illustration. In 1834 he became Extraordinary Professor in the University. In the same year he passed several weeks in Paris for the purpose of inspecting various manufactories, especially those of chemical products. In the following year he came to England and visited various manufacturing establishments in Worcester, Birmingham, Manchester, and Liverpool. On the 27th of January, 1840, he was elected a Member of the Berlin Academy. In the earlier part of his career he lectured on Chemistry at the Gewerbeschule during the absence of Professor Wöhler; from 1832 to 1840 he lectured on Physics at the Artillery and Engineer Academy; and from 1850 to 1856 on Chemical Technology at the Gewerbeinstitut. In 1845 he became Ordinary Professor. In 1861 he was elected "Rector magnificus" of the University. His services to the Academy were not limited to the publication of original researches in the 'Monatsberichte' and 'Abhandlungen;' he devoted much time and labour to its financial affairs. He gave the first impulse to the establishment of the Humboldt foundation for aiding scientific travellers. He was a Member of the Section of Physics and Chemistry of the Prussian Society of Arts, and one of the twelve Naturforschende Freunde, and for many years took his share in the scientific labours of that Society. He was a Member of the Jury in the London and Paris Exhibitions of 1851, 1855, 1862, 1867. In 1865 he represented Prussia in the Conference on weights and measures, which held its session in Frankfort. The deliberations of this Conference finally led to the adoption of the Metric System throughout Germany.

In the autumn of 1869 he visited England, attended the Meeting of the British Association at Exeter, and then passed some weeks in the Isle of Wight; but travelling and change of air no longer produced their accustomed restorative effects. After returning to Berlin his labours were often interrupted by serious indisposition. For months his vigorous constitution struggled against the encroaching sickness. With a sense of duty that overcame the most violent pain, he continued to lecture on Physics, though with many interruptions. On the 25th of February he lectured for the last time. During the month of March he was seldom able to leave his

bed, but his intellect remained unclouded. He regarded his approaching end with composure, and died on the 4th of April, 1870. He was buried on the 8th in the Dorotheenstadt Cemetery.

The researches of Magnus, on a large number of widely different subjects, extend over a space of not less than forty-five years. Almost all of them appeared in Poggendorff's 'Annalen,' and the most part also in the publications of the Berlin Academy of Sciences. His first memoir, published in 1825, while yet a student, was on the reduction of the oxides of cobalt, nickel, and iron by hydrogen, and on the spontaneous inflammability of these metals in a state of fine division. In 1827 he published researches on the solubility of sulphur and selenium, without oxidation, in sulphuric acid. Between 1827 and 1833 he investigated the combination of platinous chloride with the elements of ammonia, the composition of sikrosmine, brochantite, vesuvian, the diminution of density of vesuvian after fusion, and discovered sulphovinic, æthionic, and isæthionic acids and their salts, and, conjointly with Amermüller, periodic acid. He experimented on the gases contained in blood, the exhaustion of soils, and on the nutrition of plants. He also invented a maximum thermometer, and employed it to find the temperature of an artesian well at Rüdersdorf, and afterwards, in an improved form, to find the temperature of a similar well at Pitzpuhl. He undertook researches on the boiling of mixed fluids, and the temperature of the vapour of saline solutions. Between 1841 and 1844 he carried out a most important series of experiments on the expansion of atmospheric air, hydrogen, and other gases, and on the pressure of the vapour of water at temperatures between 6° C. and 104° C. Without being aware that Magnus was engaged in these researches, Regnault was occupied with the same investigation; and the results obtained by these two experimenters were published almost simultaneously. Their close agreement affords a striking proof of the care with which they observed and the excellence of their methods, and affords a guarantee of the accuracy of the determination of the expansion of air and the pressure of the vapour of water, data of the utmost importance in physical and chemical investigations. He made observations on electromagnetic phenomena and thermoelectric currents, on the forms of streams of spouting fluids, the deviation of rifled balls from a vertical plane, the condensation of gases on the surfaces of solids. His later researches were mainly on the transmission of heat through gases and vapours, the differences of the nature of the heat radiated from smooth and rough surfaces, and the polarization of heat. His last communication to the Academy of Sciences was made on the 11th of October, 1869.

He was Corresponding Member of the French Institute, and actual or Corresponding Member of many other Academies and learned Societies.

The preceding outline of the life and labours of Magnus is extracted from a Biography by Professor Hofmann read before the Chemical Society of Berlin.

Sir Roderick Impey Murchison, Bart., K.C.B., &c.—Among the recent losses sustained by the Society we must place in the foremost rank that caused by the death of Sir Roderick Impey Murchison. It would not be easy to define precisely the place which he held among us. Without claiming to stand on the highest platform of scientific intellect, a patient gatherer of facts rather than a brilliant generalizer from them, he yet gained by common consent the position of a leader in the commonwealth of science, under whom men of all ranks, even though of higher ability and greater attainments than his own, were willing to serve. It was not merely his achievements in geology which gave him that preeminence; he owed nothing to success in other branches of science, for he seldom travelled beyond what he knew to be his proper domain, nor to grace of literary style. He wrote only on geological and geographical subjects, and that too in a plain matter-of-fact way not likely to attract readers for whom the subject-matter had not previously possessed an interest. His influence sprang from his personal character—noble-heartedness, indomitable energy, great tact and courtesy, and an influential social position, which enabled him to befriend science and scientific men, and to gain for them increasing consideration in society.

Descended from a line of ancestors who had attached themselves to the fortunes of the Earls of Seaforth, Murchison was born on February 19th, 1792, at the little estate of Tarradale, in Eastern Ross-shire, purchased by his father, who earned a small fortune as a medical practitioner in India. While still a child he was removed into Dorsetshire, and spent nearly the whole of his boyhood in England. He received his general education at Durham Grammar School, and being destined for a military career, was sent to the Military College of Great Marlow for the requisite professional training. When only fifteen years of age he obtained a commission in the 36th regiment of foot, and sailed for the Peninsula under Sir Arthur Wellesley. He went through the brief campaign which terminated with the Convention of Cintra, and carried the colours of his regiment at Vimiera. In the succeeding campaign under Sir John Moore, which began at Lisbon and ended at Corunna, he underwent great hardship, and was one of the footsore stragglers who narrowly escaped capture by the French towards the end of that retreat.

After the army returned to England, Murchison again went abroad on military duty, but being with the troops in Sicily, saw no more active warfare. At the peace of 1815 he quitted the army, and married the daughter of General Hugonin.

For the next ten years of his life he spent his time chiefly in England. He was much attached to field-sports, and used to speak of this as the fox-hunting period of his career. Yet under the influence of his amiable and intelligent wife he made some long journeys through the continent, specially devoting himself to the study of art in public and private collections, and making copious notes on this subject.

Partly induced by the natural-history tastes of his wife, and partly urged by the recommendation of Sir Humphry Davy (who, meeting him at the house of Mr. Morritt, of Rokeby, and being struck with his active habits of mind and body, pointed out to him the rising science of geology as a fitting field for his exertions), Murchison turned his mind in that direction, and began to attend lectures on different branches of science, particularly those delivered at the Royal Institution.

Science was thus taken up by him not as an original pursuit, but after his powers of observation were matured, and when they had been further quickened by an active life at home and abroad. Being thus in a manner self-taught, and having thrown himself with all the energy of his nature into the study of geology, he was happily left to acquire his knowledge direct from nature, with but little bias from the controversies then so keenly carried on between the followers of Hutton and Werner. His devotion to the new sphere of activity which he had now chosen was not long in bearing fruit. In 1825, when thirty-three years of age, he published his earliest paper, "A Geological Sketch of the North-western extremity of Sussex and the adjoining parts of Hants and Surrey." Subsequently he explored parts of his native Highlands with Professor Sedgwick, and travelled through the volcanic district of central France with Sir Charles Lyell. From these early researches he was gradually led into that special field which he made his own, and from which his most important contributions to science were reaped.

His rambles through different parts of England had shown him how admirably the order of succession among the Secondary rocks had been worked out there by William Smith. But there was one great group of English rocks to which the Wernerian term "Transition" had been applied, yet of which the true stratigraphical relations had still to be traced. Murchison resolved to devote himself to the study of these rocks, in the hope of being able to reduce them into something like the same intelligible order which had been introduced among the later formations. The district chosen by him as the scene of his labours was that border land between England and Wales once the abode of the old British Silures. From time to time brief notices of his progress were communicated to the Geological Society, and at last, after five years of patient and enthusiastic labour, he produced his 'Silurian System.' This work undoubtedly forms a landmark in the history of geology. Dealing with rocks which had previously been but imperfectly known, he showed them to be capable of subdivision, and that when grouped in their true order they were found to embody some of the earliest chapters of the history of life upon the earth. His classification was based on the local characters of the rocks with which he was dealing; but that he had proceeded on broad and sound principles is shown by the way in which this classification has been adopted in all parts of the world.

This work laid the foundation of its writer's fame. In his subsequently

published 'Siluria,' which has gone through several editions, he recast the original work, introducing much detail regarding the extension of Silurian and older Palæozoic rocks into other countries; but while, in the later publication, the results given were necessarily often the work of other observers, the 'Silurian System' remains a monument of the independent labour of a mind quick in observation, sagacious in inference, patient in the accumulation of data, and full of that instinctive appreciation of the value of facts not yet fully understood which is near akin to genius.

Upwards of a hundred memoirs in the Transactions of Societies, chiefly on British and Continental Geology, addresses without number to Societies and Associations, besides more than twenty memoirs written in conjunction with other authors, remain as a monument of his industry. But to this mass of work must be added what he published in separate volumes—the 'Silurian System,' with its successor 'Siluria,' and his splendid volumes on 'Russia and the Ural Mountains.'

From the time when he began to devote himself to geology, Murchison continued to be one of the most active men of science of his day. Living chiefly in London, and coming daily in contact with men of every science, he took a prominent part in the work of more than one learned Society. In summer or autumn he usually made a tour for geological research, either in this country or on the continent, and continued this habit up to within two years of his death. In his Russian exploration he was absent from England for two or three years.

Of the honours heaped upon him from all parts of the world it is not necessary to speak. There was hardly an Academy anywhere which had not enrolled him among its Associates. He was elected a Fellow of the Royal Society in 1826, and in 1849 the Copley Medal was awarded to him for his three great works, 'On the Silurian System,' 'On the Geology of Russia,' and 'On the Structure of the Alps.' In 1855 he succeeded Sir Henry de la Beche as Director-General of the Geological Survey of Great Britain, an office which he held to the time of his death. On his return from Russia he had received the honour of knighthood, and at a later period he was nominated a K.C.B. and raised to the dignity of a baronet.

Sir Roderick Murchison was distinctly and specially a geologist. The attachment which his early Silurian labours had given him to Palæozoic rocks never waned; and though led now and then to make and record observations on later formations, he seemed always to return to the older deposits as his natural domain. He was not a palæontologist, yet no geologist could use more skilfully than he the data furnished by palæontology. This faculty he acquired in the Silurian region, and it continued to mark his work in the field both at home and abroad: it enabled him to apply to distant countries the principles which he had so successfully used in his own. Perhaps the leading idea of his scientific life should be regarded as a desire to establish the order of succession among rocks. It was this

desire which he so successfully realized in the Silurian region; and it seemed to be always present with him in every district to which by choice or accident he might be led. He had a singularly quick eye for the geological structure of a country, seizing often with striking accuracy, from what to others seemed but slender evidence, the leading features of the grouping of the rocks.

No notice of his life and labours would be in any measure adequate if it did not allude to the prominence with which during the last few years of his life Sir Roderick's name was brought forward by the chivalrous devotion with which he maintained the belief in the safety of Dr. Livingstone. Yet this was only one of innumerable examples of his tenacious friendship and active benevolence. As President of the Geographical Society (a Society which is in a sense his own creation) he had many opportunities of befriending not only the cause of geography but the personal interests of travellers; and it gave him a genuine pleasure to make use of these opportunities. It will be long ere the recollection will pass away of his stately courtesy of manner, suiting well that military bearing which dated from the old school of Wellesley and Moore, or of the kindliness which made him shrink from allowing even ingratitude to alienate his friendship.—A. G.

Colonel William Henry Sykes was born in 1790. His father was Samuel Sykes, Esq., descended from an old Yorkshire family, of which he was the representative. At an early age Colonel Sykes entered the military service of the East-India Company, and as a young officer of the Bombay Army bore his part in the active military operations carried on in India in the early years of the present century. He was present at the memorable siege of Bhurtpore under Lord Lake. He served in the Deccan from 1817 to 1820, and was in command of a regiment at the battles of Kirkee and Poona, and took part in the capture of the Hill Forts. Throughout his active military career he enjoyed the confidence of the distinguished commanders under whom he served, as well as the esteem and regard of his brother officers. In 1824 he was appointed Statistical Reporter to the Government of Bombay, and finally quitted India in 1831. In 1833 he was promoted to the rank of Colonel; but having returned to England he thenceforward applied himself to the pursuits of civil life. The knowledge he had acquired of Indian affairs, and the reliance placed on his intelligence and integrity, led to his being twice elected on the Board of Directors of the East-India Company; and when the vast dominion they had so long administered was about to pass under the immediate government of the Crown, the Company gave a further proof of their confidence in Colonel Sykes by appointing him their Chairman at that important epoch. Meanwhile he was (1824) elected Lord Rector of the Marischall College of Aberdeen, and three years afterwards was chosen to represent that city in Parliament, and enjoyed the entire confidence of his constituents to the end of his life. As a Member of the Legislature he was an advocate

of economy, peace, and social progress, and in the divisions of the House of Commons, from which he was not often absent, his name was almost invariably to be found on the extreme liberal side.

During his life in India, as well as afterwards at home, Colonel Sykes was a zealous scientific observer. His favourite pursuits were zoology, meteorology, and fossil geology; and numerous contributions by him on these subjects, especially the first, are to be found in the Proceedings of the Zoological, Asiatic, and Geological Societies, and in the Reports of the British Association, of which he was one of the most active members, both in the Sectional Meetings and in the deliberations of the Council. One of his most considerable papers on Indian Meteorology was published in the Philosophical Transactions for 1850. Another subject which largely engaged his attention, both in India and at home, was Statistics. He was one of the founders of the Statistical Society of London, and continued to take a lively interest in its work; in 1863 he was elected President. He was also President of the Royal Asiatic Society, and Chairman of the Society of Arts. He became a Fellow of the Royal Society in 1834, and was more than once elected on the Council. Colonel Sykes married, in 1824, Elizabeth, youngest daughter of William Hay, Esq., of Renistoun; he died at his residence in London on the 16th of June, 1872.

PROCEEDINGS

OF

THE ROYAL SOCIETY.

November 16, 1871.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

Notice was given that at the next Meeting of the Society, H.M. Pedro II., Emperor of Brazil, would be proposed for election and immediate ballot.

Dr. Allman, Dr. Blakiston, Mr. Etheridge, Mr. Newmarch, and Dr. Odling, having been nominated by the President, were elected by ballot Auditors of the Treasurer's Accounts on the part of the Society.

Dr. William Budd, Mr. Carl Schorlemmer, and Mr. Edward Burnet Tylor were admitted into the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following communications were read :—

I. "Considerations on the Abrupt Change at Boiling or Condensing in reference to the Continuity of the Fluid State of Matter." By Professor JAMES THOMSON, LL.D., Queen's College, Belfast. Communicated by Dr. ANDREWS. Received July 4, 1871.

When we find a substance capable of existing in two fluid states different in density and other properties while the temperature and pressure are the same in both, and when we find also that an introduction or abstraction of heat without change of temperature or of pressure will effect the change from the one state to the other, and also find that the change either way is perfectly *reversible*, we speak of the one state as being an ordinary

gaseous, and the other as being an ordinary liquid state of the same matter; and the ordinary transition from the one to the other we would designate by the terms boiling or condensing, or occasionally by other terms nearly equivalent, such as evaporation, gasification, liquefaction from the gaseous state, &c. Cases of gasification from liquids or of condensation from gases, when any chemical alteration accompanies the abrupt change of density, are not among the subjects proposed to be brought under consideration in the present paper. In such cases I presume there would be no perfect reversibility in the process; and if so, this would of itself be a criterion sufficing to separate them from the proper cases of boiling or condensing at present intended to be considered. If, now, the fluid substance in the rarer of the two states (that is, in what is commonly called the gaseous state) be still further rarefied, by increase of temperature or diminution of pressure, or be changed considerably in other ways by alterations of temperature and pressure jointly, without its receiving any abrupt collapse in volume, it will still, in ordinary language and ordinary mode of thought, be regarded as being in a gaseous state. Remarks of quite a corresponding kind may be made in describing various conditions of the fluid (as to temperature, pressure, and volume), which would in ordinary language be regarded as belonging to the liquid state.

Dr. Andrews (Phil. Trans. 1869, p. 575) has shown that the ordinary gaseous and ordinary liquid states are only widely separated forms of the same condition of matter, and may be made to pass into one another by a course of continuous physical changes presenting nowhere any interruption or breach of continuity. If we denote geometrically all possible points of pressure and temperature jointly, by points spread continuously in a plane surface, each point in the plane being referred to two axes of rectangular coordinates, so that one of its ordinates shall represent the temperature and the other the pressure denoted by that point, and if we mark all the successive boiling- or condensing-points of temperature and pressure as a continuous line on this plane, this line, which may be called *the boiling-line*, will be a separating boundary between the regions of the plane corresponding to the ordinary liquid state and those corresponding to the ordinary gaseous state. But, by consideration of Dr. Andrews's experimental results, we may see that this separating boundary comes to an end at a point of pressure and temperature which, in conformity with his language, may be called the *critical point* of pressure and temperature jointly; and we may see that, from any ordinary liquid state to any ordinary gaseous state, the transition may be effected gradually by an infinite variety of courses passing round outside the extreme end of the boiling-line.

Now it will be my chief object in the present paper to state and support a view which has occurred to me, according to which it appears probable that, although there be a practical breach of continuity in crossing the line of boiling-points from liquid to gas or from gas to liquid, there may exist, in the nature of things, a theoretical continuity across this breach having

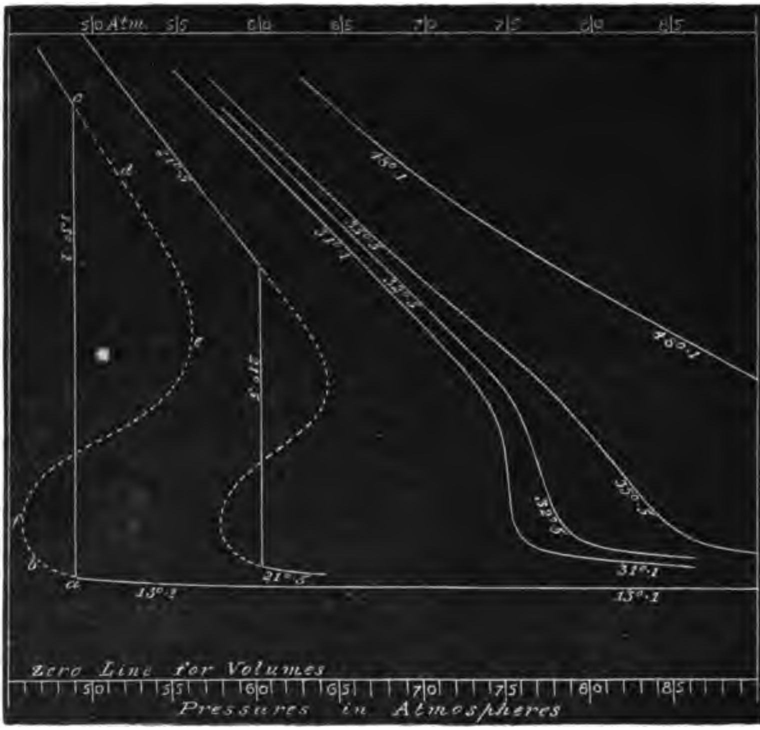
some real and true significance. This theoretical continuity, from the ordinary liquid state to the ordinary gaseous state, must be supposed to be such as to have its various courses passing through conditions of pressure, temperature, and volume in unstable equilibrium for any fluid matter theoretically conceived as homogeneously distributed while passing through the intermediate conditions. Such courses of transition, passing through unstable conditions, must be regarded as being impossible to be brought about throughout entire masses of fluids dealt with in any physical operations. Whether in an extremely thin lamina of gradual transition from a liquid to its own gas, in which it is to be noticed the substance would not be homogeneously distributed, conditions may exist in a stable state having some kind of correspondence with the unstable conditions here theoretically conceived, will be a question suggested at the close of this paper in connexion with some allied considerations.

It is first to be observed that the ordinary liquid state does not necessarily cease abruptly at the line of boiling-points, as it is well known that liquids may, with due precautions, be heated considerably beyond the boiling temperature for the pressure to which they are exposed. This condition is commonly manifested in the boiling of water in a glass vessel by a lamp placed below, when the temperature of the internal parts of the water, or, in other words, of the parts not exposed to contact with gaseous matter, rises considerably above the boiling-point for the pressure, and the water boils with bumping *. At this stage it becomes desirable to refer to Dr. Andrews's diagram of curves showing his principal results for carbonic acid, and to consider carefully some of the remarkable features presented by those curves. In doing so, we have first, in the case of the two curves for 13°·1 and 21°·5, which pass through the boiling interruption of continuity, to guard against being led, by the gradually bending transition from the curve representing obviously the liquid state into the line seen rapidly ascending towards the curve representing obviously the gaseous state, to suppose that this curved transition is in any way indicative of a gradual transition from the liquid towards the gaseous state. Dr. Andrews has clearly pointed out, in describing those experimental curves, that the slight bend at about the commencement of the rapid ascent from the liquid state is to be ascribed to a trace of air unavoidably present in the carbonic acid; and that if the carbonic acid had been absolutely pure, the ascent from the liquid to the gaseous state would doubtless have been quite abrupt, and would have shown itself in his diagram by a vertical straight

* It has even been found by Dufour (Bibliothèque Universelle, Archives, year 1861, vol. xii., "Recherches sur l'ébullition des Liquides) that globules of water floating immersed in oil, so as neither to be in contact with any solid nor with any gaseous body, may, under atmospheric pressure, be raised to various temperatures far above the ordinary boiling-point, and occasionally to so high a temperature as 178° C., without boiling.

On this subject reference may also be made to the important researches of Donny, "Sur la cohésion des Liquides et sur leur adhérence aux Corps solides," Ann. de Chimie, year 1846, 3rd ser. vol. xvi. p. 167.—Jul 28, 1871.

line, when we regard the coordinate axes for pressures and volumes as being horizontal and vertical respectively. Now in the diagram here submitted the continuous curves (that is to say, those which are not dotted)



are obtained from Dr. Andrews's diagram, with the slight alteration of substituting, in accordance with the explanations just given, an abrupt meeting instead of the curved transition between the curve for the liquid state and the upright line which shows the boiling stage. Looking to either of the given curves which pass through boiling, and, for instance, selecting the curve for 13°·1, we perceive, from what has been said as to the conditions to which boiling by bumping is due, that for the temperature pertaining to this curve the liquid state does not necessarily end at the boiling pressure for this temperature, and that thus in the diagram the curve showing volumes for the liquid state must not cease at the foot of the upright line which marks the boiling stage of pressure, but must extend continuously, for some distance at least, into lower pressures in some such way as is shown by the dotted continuation from *a* to *b*. But now the question arises, Does this curve necessarily end at any particular point *b*? We know that the extent of this curve in the direction from *a* towards or past *b*, along which the liquid volume will continue to be re-

presented before the explosive or bumping change to gas occurs, is very variable under different circumstances, being much affected by the presence of other fluids, even in small quantities, as impurities in the fluid experimented on, and by the nature of the surface of the containing vessel, &c.

The consideration of the subject may be facilitated, and aid towards the attainment of clear views of the mutual relations of temperature, pressure, and volume in a given mass of a fluid may be gained, by actually making, or by conceiving there to be made, for carbonic acid, from the data supplied in Dr. Andrews's experimental results, a solid model consisting of a curved surface referred to three axes of rectangular coordinates, and formed so that the three coordinates of each point in the curved surface shall represent, for any given mass of carbonic acid, a temperature, a pressure, and a volume which can coexist in that mass. It is to be noticed here that in his diagram of curves the results for each of the several temperatures experimented on are combined in the form of a plane curved line referred to two axes of rectangular coordinates, one of each pair of ordinates representing a pressure, and the other representing the volume corresponding to that pressure at the temperature to which the curve belongs. Now to form a model such as I am here recommending, and have myself made, Dr. Andrews's curved lines are to be placed with their planes parallel to one another, and separated by intervals proportional to the differences of the temperatures to which the curves severally belong, and with the origins of coordinates of the curves situated in a straight line perpendicular to their planes, and with the axes of coordinates of all of them parallel in pairs to one another, and then the curved surface is to be formed so as to pass through those curved lines smoothly or evenly*. The curved surface so obtained exhibits in a very obvious way the remarkable phenomena of the voluminal conditions at and near the critical point of temperature and pressure, in comparison with the voluminal conditions throughout other parts of the range of gradually varying temperatures and pressures to which it extends, and even throughout a far wider range into which it can in imagination be conceived to be extended. It helps to afford a clear view of the nature and meaning of the continuity of the liquid and gaseous states of matter. It does so by its own obvious continuity throughout its expanse round the end of the range of points of pressure and temperature where an abrupt change of volume can occur by boiling or condensing. On the curved surface in the model Dr. Andrews's curves for the temperatures 13°·1, 21°·5, 31°·1, 32°·5, 35°·5, and 48°·1 Centigrade, which afford the data for its construction, may with advantage be all shown drawn in their proper

* For the practical execution of this, it is well to commence with a rectangular block of wood, and then carefully to pare it down, applying, from time to time, the various curves as templets to it, and proceeding according to the general methods followed in a shipbuilder's modelling-room in cutting out small models of ships according to curves laid down on paper as cross sections of the required model at various places in its length.

places. The model admits of easily exhibiting in due relation to one another a second set of curves, in which each would be for a constant pressure, and in each of which the coordinates would represent temperatures and corresponding volumes. It may be used in various ways for affording quantitative relations interpolated among those more immediately given by the experiments.

We may now, aided by the conception of this model, return to the consideration of continuity or discontinuity in the curves in crossing the boiling stage. Let us suppose an indefinite number of curves, each for one constant temperature, to be drawn on the model, the several temperatures differing in succession by very small intervals, and the curves consequently being sections of the curved surface by numerous planes closely spaced parallel to one another and to the plane containing the pair of coordinate axes for pressure and volume. Now we can see that, as we pass from curve to curve in approaching towards the critical point from the higher temperatures, the tangent to the curve at the steepest point or point of inflection is rotating, so that its inclination to the plane of the coordinate axes for pressure and temperature, which we may regard as horizontal, increases till, at the critical point, it becomes a right angle. Then it appears very natural to suppose that, in proceeding onwards past the critical point to curves successively for lower and lower temperatures, the tangent at the point of inflection would continue its rotation, and the angle of its inclination, which before was acute, would now become obtuse. It seems much more natural to make such a supposition as this than to suppose that in passing the critical point from higher into lower temperatures the curved line, or the curved surface to which it belongs, should break itself asunder, and should come to have a part of its conceivable continuous course absolutely deficient. It thus seems natural to suppose that in some sense there is continuity in each of the successive curves by courses such as those drawn in the accompanying diagram as dotted curves uniting continuously the curves for the ordinary gaseous state with those for the ordinary liquid state.

The physical conditions corresponding to the extension of the curve from *a* to some point *b* we have seen are perfectly attainable in practice. Some extension of the gaseous curve into points of temperature and pressure below what I have called the boiling- or condensing-line (as, for instance, some extension such as from *c* to *d* in the figure) I think we need not despair of practically realizing in physical operations. As a likely mode in which to bring steam continuing gaseous to points of pressure and temperature at which it would collapse to liquid water if it had any particle of liquid water present along with it, or if other circumstances were present capable of affording some apparently *requisite conditions for enabling it to make a beginning of the change of state* *, I would suggest the

* The principle that "the particles of a substance when existing all in one state only, and in continuous contact with one another, or in contact only under special circum-

admitting speedily of dry steam nearly at its condensing temperature for its pressure (or, to use a common expression, *nearly saturated*) into a vessel with a piston or plunger, all kept hotter than the steam, and then allowing the steam to expand till by its expansion it would be cooled below its condensing-point for its pressure; and yet I would suppose that if this were done with very careful precautions the steam might not condense, on account of the cooled steam being surrounded entirely with a thin film of superheated steam close to the superheated containing vessel. The fact of its not condensing might perhaps best be ascertained by observations on its volume and pressure. Such an experiment as that sketched out here would not be easily made, and unless it were conducted with very great precautions, there could be no reasonable expectation of success in its attempt; and perhaps it might not be possible so completely to avoid the presence of dust or other dense particles in the steam as to make it prove successful. I mention it, however, as appearing to be founded on correct principles, and as tending to suggest desirable courses for experimenral researches. The overhanging part of the curve from *e* to *f* seems to represent a state in which there would be some kind of unstable equilibrium; and so, although the curve there appears to have some important theoretical significance, yet the states represented by its various points would be unattainable throughout any ordinary mass of the fluid. It seems to represent conditions of coexistent temperature, pressure, and volume in which, if all parts of a mass of fluid were placed, it would be in equilibrium, but out of which it would be led to rush, partly into the rarer state of gas, and partly into the denser state of liquid, by the slightest inequality of temperature or of density in any part relatively to other parts. I might proceed to state, in support of these views, several considerations founded on the ordinary statical theory of capillary or superficial phenomena of liquids, which is dependent on the supposition of an attraction acting very intensely for very small distances, and causing intense pressure in liquids over and above the pressure applied by the containing vessel and measurable by any pressure-gauge. That statical theory has fitted remarkably well to many observed phenomena, and has sometimes even led to the forecasting of new results in advance of experiment. Hence, although dynamic or kinetic theories of the constitution and pressure of fluids now seem likely to supersede any statical theory, yet phenomena may still be discussed according to the principles of the statical theory;

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stances with other substances, experience *a difficulty of making a beginning of their change of state*, whether from liquid to solid, or from liquid to gaseous, or probably also from solid to liquid," was proposed by me, and, so far as I am aware, was first announced in a paper by me in the Proceedings of the Royal Society for November 24, 1859 (vol. x. p. 158), and in a paper submitted to the British Association in the same year.

In the present paper, at the place to which this note is annexed, I adduce the like further supposition that *a difficulty of making a beginning of change of state* from gaseous to liquid may also probably exist.

and there may be considerable likelihood that conditions explained or rendered probable under the statical theory would have some corresponding explanation or confirmation under any true theory by which the statical might come to be superseded. With a view to brevity, however, and to the avoidance of putting forward speculations perhaps partly rash, though, I think, not devoid of real significance, I shall not at present enter on details of these considerations, but shall leave them with merely the slight suggestion now offered, and with the suggestion mentioned in an earlier part of the present paper, of the question whether in an extremely thin lamina of gradual transition from a liquid to its own gas, at their visible face of demarcation, conditions may not exist in a stable state having a correspondence with the unstable conditions here theoretically conceived.

II. "Contributions to the History of the Opium Alkaloids.—Part III." By C. R. A. Wright, D.Sc., Lecturer on Chemistry in St. Mary's Hospital Medical School. Communicated by Dr. H. E. Roscoe. Received July 21, 1871.

§ 1. *Action of Hydriodic Acid on Codeia in presence of Phosphorus.*

In Parts I. and II. of these researches the action of hydrobromic acid on codeia and its derivatives has been partially investigated; and as the action of this acid appears to be in some respects similar to, but in others different from, that of hydrochloric acid, it appeared to be of interest to examine the action of hydriodic acid also.

Some preliminary experiments on this subject made two or three years ago in conjunction with the late Dr. A. Matthiessen, showed that when codeia is boiled with a large excess of strong hydriodic acid, no appreciable quantity of methyl iodide is evolved even after some hours' treatment; a brown tarry mass containing much free iodine was produced, but at the time nothing fit for analysis was obtained from this; since then, Dr. Matthiessen and Mr. Burnside* have corroborated the non-formation of methyl iodide under these circumstances.

If, however, phosphorus be added simultaneously with the hydriodic acid, so as to prevent the accumulation of free iodine, methyl iodide is evolved at 100° and upwards in quantity close upon that required for the equation

$$\underset{\text{Codeia.}}{C_{18}H_{21}NO_3} + HI = CH_3I + \underset{\text{Morphia.}}{C_{17}H_{19}NO_3};$$

hitherto, however, no body of this latter formula has been isolated from the products of the reaction, the substances ultimately formed being derived from a base containing H_2 more than morphia.

The hydriodic acid was obtained in the first instance by the action of

* Proc. Roy. Soc. vol. xix. p. 71.

hydric sulphide on iodine and water: in the dilute acid thus got, iodine was dissolved, and the whole digested at a very gentle heat with phosphorus, more iodine solution being added from time to time; finally the whole was distilled several times from potassium iodide. A colourless acid of sp. gr. 1·7–1·75, and containing 50–55 per cent. of HI, was thus obtained, and preserved colourless by keeping a stick of phosphorus in the bottle. The codeia used in these experiments was part of a further supply most liberally presented by Messrs. Macfarlane, of Edinburgh.

On heating on the water-bath a mixture of 10 parts codeia, 30 to 50 of this acid, and 1 of phosphorus, the evolution of methyl iodide is noticed in a few minutes; simultaneously the liquid becomes brown, indicating the separation of free iodine; after two or three hours the brown colour disappears, and the evolution of methyl iodide ceases. If the liquid be heated to gentle ebullition, at first the same effects ensue, but more quickly; the resulting product, however, varies in composition according to the temperature at which the reaction was effected.

In one experiment 55 grammes of codeia yielded by condensation 22·5 grammes of methyl iodide, the theoretical yield being 24·6 from crystallized codeia, $C_{18}H_{21}NO_3, H_2O$; hence upwards of 90 per cent. of the theoretical yield was obtained. In order to prove the elimination of $\frac{1}{18}$ part of the carbon in the form of methyl iodide, 4·3045 grammes of codeia, dried at 140°–150° C., was heated to gentle ebullition with 30 grammes of 55 per cent. hydriodic acid and about 2 of phosphorus; the vapours evolved were passed through a flask to condense aqueous vapour, and then through a combustion-tube filled with red-hot lead chromate, the CO_2 produced being absorbed in the usual way, an aspirator being attached at the far end so as to create a diminished pressure throughout the apparatus, and thus guard against loss of methyl-iodide vapour by leakage at any of the numerous corks and joints*. After three hours a current of pure oxygen was led through the apparatus, to sweep out the last traces of methyl-iodide vapour from the flasks and ensure the perfect combustion of deposited carbonaceous particles.

4·3045 grms. codeia thus gave 0·617 grm. CO_2.

0·3720 grm. codeia, burnt in the usual way, gave 0·9830 CO_2.

	Found.	Calculated.
(A) Percentage of carbon evolved as CH_3I	3·91	4·013
(B) " " in codeia used ..	72·07	72·241
Ratio of A to B	$\frac{3·91}{72·07}=\frac{1}{18·2}$	$\frac{4·013}{72·241}=\frac{1}{18}$

In another experiment, not carried to a complete conclusion, the CO_2 collected represented 3·7 per cent. of the codeia used.

The methyl iodide produced was found, after washing with water, dry-

* This device may be applied with advantage to the ordinary processes for combustion, blowing out of the tube as well as loss by traces of leakage being thus avoided.

ing over $Ca\,Cl_2$, and distillation, to be free from traces of dissolved phosphorus, to boil at 42°–45° C., and to correspond in every respect with the ordinary methyl iodide.

If the reaction with hydriodic acid takes place on the water-bath, the resulting product appears to have the composition $C_{68}\,H_{86}\,I_2\,N_4\,O_{12}$, 4HI; but if the mixture be heated to gentle ebullition throughout, the temperature not being allowed to exceed 110°–115° from loss of aqueous fluid by evaporation, the substance obtained contains the elements of two molecules of water less, $=C_{68}\,H_{82}\,I_2\,N_4\,O_{10}$, 4HI; whilst if the mixture be rapidly boiled, so that by evaporation the boiling-point rises to 130° and upwards, the ultimate product contains less oxygen than this last body, being $C_{68}\,H_{82}\,I_2\,N_4\,O_6$, 4HI. These three formulæ might each be halved; but inasmuch as compounds containing not less than C_{68} have been got from these products by simple treatments, the higher formulæ are more probable.

All three substances are, while moist, colourless tars, drying at 100° to brittle waxy-looking masses, not fusing at 100° when perfectly dry; they are soluble in hot water, a decomposition being thereby produced; while moist they appear to absorb oxygen with avidity, rapidly becoming yellow or orange. They are also extremely hygroscopic; and from the high percentage of iodine contained, the ease with which they decompose on heating, and the difficultly combustible carbon left, their analysis is a matter of some considerable difficulty. From all these circumstances combined, the numbers obtained do not always accord quite as closely as might be expected in the case of crystalline and easily purified substances.

To obtain the compound $C_{68}\,H_{86}\,I_2\,N_4\,O_{12}$, 4HI, 10 parts of codeia, 30 of 55 per cent. hydriodic acid, and 1 of phosphorus may be heated on the water-bath for three to four hours, at the end of which time the evolution of methyl iodide has entirely ceased: by filtering the syrupy hot liquid through asbestos to separate particles of amorphous phosphorus and addition of a little water when cold, a colourless tar is precipitated, which soon sets to a hard brittle mass; this is broken up and thoroughly washed with water to separate the phosphorus acids produced simultaneously, and finally freed from moisture as far as possible by pressure between filter paper, and dried at 100°.

The same body may also be obtained by dissolving the original substance in slightly warm water, precipitating with sodium carbonate, and extraction of the mass thus thrown down with ether and agitation of the first portions of the ether extract with hydriodic acid: the tar thus got is identical in all respects with the original substance. After drying at 100° the following numbers were obtained*:—

(A) Prepared by first method:

0·3785 grm. gave 0·588 CO_2 and 0·173 $H_2\,O$.

0·359 grm. gave 0·2535 AgI.

* All combustions given in this paper were made with lead chromate and oxygen; and the iodine determinations by boiling with nitric acid and silver nitrate.

(B) Prepared by ether process:
0·357 grm. gave 0·5655 CO_2 and 0·165 H_2O.
0·2635 grm. gave 0·1895 AgI.

(C) Another specimen prepared by ether process:
0·316 grm. gave 0·493 CO_2 and 0·135 H_2O.
0·2865 grm. gave 0·2025 Ag.

	Calculated.		Found.			
			A.	B.	C.	Mean.
C_{68}	816	42·59	42·36	43·20	42·54	42·70
H_{90}	90	4·70	5·08	5·14	4·75	4·99
I_6	762	39·77	38·16	38·87	38·19	38·41
N_4	56	2·92				
O_{12}	192	10·02				
$C_{68}H_{86}I_2N_4O_{12}$, 4HI	1916	100·00				

The falling short in the percentage of iodine found in these specimens is readily accounted for by the action of the water which necessarily adheres to the tarry product got by either of the above processes; it will be subsequently shown that by the action of water on this body the elements of HI are removed from it.

This compound is apparently formed by the reaction

$$\underset{\text{Codeia hydriodate.}}{4(C_{18}H_{21}NO_3, HI)} + 14HI = 4CH_3I + I_8 + \underset{\text{New body.}}{C_{68}H_{86}I_2N_2O_{12}, 4HI}.$$

The iodine thus set free is of course reconverted into HI by the action of the phosphorus, a mixture of phosphorous and phosphoric acids being thereby produced. The reaction

$$3I_2 + P_2 + 6H_2O = 2H_3PO_3 + 6HI$$

requires for 50 grms. of codeia 3·45 grms. of phosphorus to be converted into phosphorous acid; whilst the equation

$$5I_2 + P_2 + 8H_2O = 2H_3PO_4 + 8HI$$

requires 2·07 grms. to be converted into phosphoric acid. In one experiment 2·8 grms. of phosphorus, as nearly as could be estimated, were found to have become converted into the mixture of the two acids, 50 grms. of codeia having been employed.

On attempting to procure the free base $C_{68}H_{86}I_2N_2O_{12}$ from the hydriodate got as above, by precipitation with sodium carbonate, a snow-white mass was obtained containing, besides a small quantity of the desired base (soluble in ether), a large quantity of two other bases derived from this one (but sparingly soluble in ether). The description of the products thus got will be given in a subsequent section.

By treating codeia with hydriodic acid and phosphorus as above described, but at a temperature of gentle ebullition not rising above 115°, a

product is got on filtration through asbestos and precipitation by water containing apparently $2H_2O$ less than the preceding compound. Dried at 100°,

0·302 grm. gave 0·475 CO_2 and 0·135 H_2O.
0·248 grm. gave 0·1845 AgI.

	Calculated.		Found.
C_{68}	816	43·40	42·89
H_{86}	86	4·58	4·97
I_6	762	40·53	40·20
N_4	56	2·98	
O_{10}	160	8·51	
$C_{68}H_{82}I_2N_4O_{10}, 4HI$	1880	100·00	

Hence this substance is formed from the preceding one by the reaction

$$C_{68}H_{86}I_2N_4O_{12}, 4HI = 2H_2O + C_{68}H_{82}I_2N_4O_{10}, 4HI.$$

If codeia, hydriodic acid (3 to 5 parts), and phosphorus be heated to rapid ebullition, so that most of the aqueous portion distils off along with the CH_3I formed, the boiling-point gradually rises to 130°, or a little above, at which temperature the colourless liquid begins again to become slightly brown: on precipitation of the filtered product with water &c., as before, the following numbers were obtained after drying at 100°:—

Specimen A.	0·3575 grm.	gave	0·589 CO_2 and 0·168 H_2O.
	0·402	,,	0·307 AgI.
Specimen B.	0·358	,,	0·583 CO_2 and 0·164 H_2O.
	0·369	,,	0·2815 AgI.
Specimen C.	0·3855	,,	0·635 CO_2 and 0·178 H_2O.
	0·363	,,	0·276 AgI.

	Calculated.		Found. A.	B.	C.	Mean.
C_{68}	816	44·93	44·92	44·41	44·91	44·75
H_{86}	86	4·73	5·22	5·09	5·13	5·15
I_6	762	41·96	41·27	41·23	41·09	41·20
N_4	56	3·09				
O_6	96	5·29				
$C_{68}H_{82}I_2N_4O_6, 4HI$	1816	100·00				

Hence this substance is formed from the preceding one by the reaction

$$C_{68}H_{82}I_2N_4O_{10}, 4HI + 8HI = I_8 + 4H_2O + C_{68}H_{82}I_2N_4O_6, 4HI.$$

§ 2. *Action of Water on the foregoing Compounds.*

When either of the two first compounds just described is dissolved in a large bulk of hot water, and the solution allowed to cool, solid white flakes

are obtained containing more C and H, and less I, than the original substance. By repeating this treatment several times successively, the same ultimate compound appears to be produced in each case, being apparently formed by the reactions

$$C_{68}H_{86}I_2N_4O_{12}, 4HI = 2HI + 2H_2O + C_{68}H_{80}N_4O_{10}, 4HI,$$
$$C_{68}H_{82}I_2N_4O_{10}, 4HI \quad = \quad 2HI \quad + C_{68}H_{80}N_4O_{10}, 4HI.$$

The removal of the last traces of basic HI is very difficult; it may be accelerated by adding a few drops of sodium carbonate to the boiling solution and filtering hot from the small amount of precipitated base; the product is apt, however, to be more strongly yellow-coloured when obtained in this way than when got by treatment with water alone.

Specimen A. Got from compound $C_{68}H_{82}I_2N_4O_{10}$, 4HI by water and a little sodium carbonate:—

0·366 grm. gave 0·672 CO_2 and 0·177 H_2O.
0·406 grm. gave 0·227 AgI.

Specimen B. From same compound by water alone:—

0·369 grm. gave 0·674 CO_2 and 0·174 H_2O.
0·4555 grm. gave 0·2675 AgI.

Specimen C. From compound $C_{68}H_{86}I_2N_4O_{12}$, 4HI by water alone:—

0·4265 grm. gave 0·762 CO_2 and 0·220 H_2O.
0·3845 grm. gave 0·2275 AgI.

	Calculated.		Found. A.	B.	C.	Mean.
C_{68}	816	50·25	50·08	49·81	48·73	49·54
H_{84}	84	5·17	5·36	5·24	5·73	5·41
N_4	56	3·45				
O_{10}	160	9·85				
I_4	508	31·28	30·21	31·74	31·98	31·31
$C_{68}H_{80}N_4O_{10}$, 4HI	1624	100·00				

Specimen A, dissolved in hot water and precipitated by sodium carbonate, yielded a yellowish-white substance, rapidly becoming darker and finally almost black. Dried rapidly at 100°,—

0·370 grm. gave 0·930 CO and 0·235 H_2O.

About 0·5 grm. examined qualitatively for iodine gave only traces of AgI.

	Calculated.		Found.
C_{68}	816	68·46	68·55
H_{80}	80	6·71	7·05
N_4	56	4·70	
O_{15}	240	20·13	
$C_{68}H_{80}N_4O_{10}+O_5$	1192	100·00	

From these numbers it appears that the free base, like bromo- and chloro-tetra-codeia, rapidly absorbs oxygen from the air.

Probably there exists a compound intermediate between the hydriodate just described and the original body, $C_{68}H_{82}I_2N_4O_{10}$, 4HI; thus one batch of flakes got by two treatments with water of this original substance gave the following numbers after drying at 100°:—

0·3225 grm. gave 0·556 CO_2 and 0·153 H_2O.
0·3205 grm. gave 0·212 AgI.

Another specimen obtained similarly:—

0·4175 grm. gave 0·269 AgI.

	Calculated.		Found.	
C_{68}	816	46·58	47·02	
H_{85}	85	4·85	5·27	
I_5	635	36·24	35·75	34·82
N_4	56	3·20		
O_{10}	160	9·13		
$C_{68}H_{81}IN_4O_{10}$, 4HI	1752	100·00		

It is not impossible that this substance is not a definite compound, but only a mixture; nevertheless, a free base of this composition and its hydriodate have been obtained by the action of sodium carbonate on the compound $C_{68}H_{86}I_2N_4O_{12}$, 4HI, from whence it appears probable that the body analyzed is really a definite compound, formed by the reaction

$$C_{68}H_{82}I_2N_4O_{10}, 4HI = HI + C_{68}H_{81}IN_4O_{10}, 4HI.$$

Both the final and intermediate products have a very curious structure under the microscope; although they simulate in a high degree the appearance of crystals as they separate from a hot aqueous solution, yet on microscopic examination they are found to consist of strings of coalesced globules not unlike the yeast-plant. In qualitative reactions all the bodies hitherto described are very similar: ferric chloride gives no coloration to the aqueous solution of the hydriodate; silver nitrate is reduced on standing, producing a yellow tint; nitric acid gives an intense yellow; sulphuric acid and potassium dichromate only separate iodine; sodium carbonate throws down a white precipitate scarcely soluble in excess, and soon becoming yellow, salmon-colour, and finally dark brown; ammonia gives a similar precipitate somewhat more soluble in excess, while caustic potash readily dissolves the white precipitate first formed. In many of these reactions this group of codeia derivatives utterly differs from the bodies got by the action of HCl or HBr; most of these latter derivatives give colours with ferric chloride and sulphuric acid and dichromate; all give a blood-red with nitric acid, while the free bases turn more or less *green* by exposure to air.

On similarly treating with boiling water the compound $C_{68}H_{82}I_2N_4O_8$, 4HI,

formed by the action of hydriodic acid on codeia at about 130°, a substance similar in characters to that got from the other two compounds is produced. The final product, however, differs somewhat in its physical characters from those just mentioned; instead of coming out from the hot aqueous solution in solid flakes, it appears in very minute solid oil-globules which do not readily subside, and give to the liquid a great resemblance to fresh milk; sometimes the globules do not subside for many days.

Dried at 100° these globules give numbers indicating a compound analogous to that of the non-iodized base just described; it is, however, much more difficult in this instance to remove the last portions of basic HI; moreover, four molecules of water appear to be taken up, probably in lieu of the oxygen lost.

Specimen A. Original substance treated three times with large excess of water:—

0·3315 grm. gave 0·589 CO_2 and 0·175 H_2O.
0·299 grm. gave 0·178 Ag I.

Specimen B. Original with water four times:—
0·321 grm. gave 0·585 CO_2 and 0·169 H_2O.
0·411 grm. gave 0·754 CO_2 and 0·218 H_2O.
0·2835 grm. gave 0·165 Ag I.

Specimen C. Original with water five times:—
0·4095 grm. gave 0·746 CO_2 and 0·219 H_2O.
0·3995 grm. gave 0·237 Ag I.

	Calculated.		Found. A.	Found. B.		Found. C.	Mean.
C_{68}	816	50·00	48·46	49·71	50·03	49·68	49·47
H_{92}	92	5·64	5·87	5·85	5·89	5·94	5·89
I_4	508	31·13	32·17	31·45		32·06	31·89
N_4	56	3·43					
O_{10}	160	9·80					
$C_{68}H_{88}N_4O_{10}, 4HI$	1632	100·00					

Hence this compound is formed by the reaction

$$C_{68}H_{82}I_2N_4O_6, 4HI + 4H_2O = 2HI + C_{68}H_{88}N_4O_{10}, 4HI.$$

Carbonate of soda threw down from specimen C a white precipitate, becoming yellow on standing: this precipitate contained a small amount of iodine, showing (as the above numbers indicate) that the transformation of the original substance was not absolutely complete.

The qualitative reactions of this substance are the same as those of the bodies previously described.

§ 3. *Action of Sodium Carbonate on the compound* $C_{68}H_{86}I_2N_4O_{12}, 4HI$.

On adding sodium carbonate to the scarcely warm aqueous solution of

this compound a voluminous white precipitate is produced, which is apparently a mixture of three bases, two of which contain iodine, whilst the third is free from that ingredient. The first one, which forms only a small fraction of the whole, is the free base of the original compound, $C_{68}H_{86}I_2N_4O_{12}$; this is readily soluble in ether, and may be obtained as hydriodate (as previously stated) by digesting the precipitate with ether, and agitation of the extract with hydriodic acid, whereby the original substance is reproduced. To prevent frothing, the precipitate must be well drained from the aqueous portion. By continuing the extraction until some 6 litres of ether have been employed for exhausting the precipitate from 40 grms. of original substance, the whole of this base is removed, or nearly so. Attempts to prepare the base itself by evaporation of the ether yielded only a tarry substance which could not be removed from the vessel employed; treatment with water or alcohol more or less decomposes it.

By employing a large bulk of ether after this first base has been almost wholly removed, an extract is obtained from which on evaporation solid flakes separate; these are much less soluble in ether than the first base, and after drying at 100° gave the following numbers, indicating the formula

$$C_{68}H_{81}IN_4O_{10}:—$$

0·1985 grm. gave 0·4705 CO_2 and 0·119 H_2O.
0·1210 grm. gave 0·0260 Ag I.

	Calculated.		Found.
C_{68}	816	65·81	64·65
H_{81}	81	6·53	6·66
I	127	10·24	11·61
N_4	56	4·52	
O_{10}	160	12·90	
$C_{68}H_{81}IN_4O_{10}$	1240	100·00	

Apparently these flakes still retained a trace of the first base; the mother liquor from which they separated, when evaporated to dryness, left a small amount of residue containing 14·18 per cent. of iodine. Treated with hydriodic acid, these flakes gave a hydriodate yielding these numbers after drying at 100°:—

0·3675 grm. gave 0·6335 CO_2 and 0·181 H_2O.
0·371 grm. gave 0·2405 Ag I.

	Calculated.		Found.
C_{68}	816	46·58	47·00
H_{85}	85	4·85	5·47
I_5	635	36·24	35·02
N_4	56	3·20	
O_{10}	160	9·13	
$C_{68}H_{81}IN_4O_{10}, 4HI$	1752	100·00	

Evidently a slight loss of iodine has occurred from the action of the adhering moisture while drying, as the original flakes contained rather too high a percentage of iodine.

This base is formed from the original one by the reaction

$$C_{68}H_{86}I_2N_4O_{12}, 4HI = 2H_2O + HI + C_{68}H_{81}IN_4O_{10}, 4HI,$$

identical with that taking place on treatment with water.

A portion of the substance left after extraction with ether was treated several times successively with large bulks of ether (about 4 litres of ether to 10 grms. of precipitate each extraction). After the majority of the substance had thus been dissolved, a portion of the last ether extracts was evaporated down and yielded flakes agreeing approximately with the composition required for a mixture of one molecule of $C_{68}H_{81}IN_4O_{10}$, and two molecules of $C_{68}H_{80}N_4O_{10}$.

0·3565 grm. gave 0·926 CO_2 and 0·237 H_2O.
0·376 grm. gave 0·255 AgI.
0·2455 grm. gave 0·170 AgI.

	Calculated.	Found.	
C	70·67	70·84	
H	6·96	7·39	
I	3·67	3·66	3·73

From the foregoing experiments it is clear that the action of sodium carbonate on the compound $C_{68}H_{86}I_2N_4O_{12}$, 4HI is identical with that of water described in § 2, the two bases $C_{68}H_{81}IN_4O_{10}$ and $C_{68}H_{80}N_4O_{10}$ being the principal products.

On precipitating in a similar way the compound $C_{68}H_{82}I_2N_4O_{10}$, 4HI, the same reaction appears to take place; from the precipitate ether extracts only traces at first, indicating probably that the base $C_{68}H_{81}I_2N_4O_{10}$ is not produced in any quantity, as apparently the more highly iodized bases are more soluble in ether.

On treating the compound $C_{68}H_{82}I_2N_4O_6$, 4HI in the same way, an analogous reaction seems to ensue; the precipitate is very sparingly soluble in ether, and on treatment with hydriodic acid furnished a hydriodate of which 0·233 grm. dried at 100° gave 0·142 AgI: hence I=32·94 per cent.; the compound $C_{68}H_{83}N_2O_{10}$, 4HI requires 31·13 per cent., whilst the original substance requires 40·53 per cent.

§ 4. *Action of Hydriodic Acid on some of the foregoing Substances.*

As the action of water on the three compounds first described is to remove the elements of HI associated with the carbon radicals of the bases, it was thought probable that by treating the products of the action of water on these compounds with strong boiling hydriodic acid, the HI thus lost might be again added on. A reaction of this nature does indeed take place, but does not always stop at the reproduction of the original bodies, another

equivalent of HI being also added on; moreover in some instances a number of molecules of H_2O are likewise taken up, and are not separated from the compounds ultimately formed even by some days' exposure to a temperature of 100°.

On treating the compound $C_{68}H_{80}N_4O_{10}$, 4HI with about ten parts of 55 per cent. of hydriodic acid (a little piece of phosphorus being also added to prevent separation of iodine from the HI by the heat) and heating to boiling, a syrupy liquid is obtained, from which water precipitates (after filtration from phosphorus) a tar resembling in all its physical characters the compound $C_{68}H_{86}I_2N_4O_{12}$, 4HI; it contains, however, the elements of $HI+10H_2O$ more than this substance. The same substance apparently is generated by treating the intermediate compound $C_{68}H_{81}IN_4O_{12}$, 4HI with hydriodic acid in the same manner.

(A) From the compound $C_{68}H_{80}N_4O_{10}$, 4HI, dried at 100° till constant,

0·3565 grm. gave 0·483 CO_2 and 0·158 H_2O.
0·3835 grm. gave 0·278 AgI.

(B) (A) dried twelve hours more at 100°, had turned a much darker colour, probably indicating absorption of oxygen,—

0·329 grm. gave 0·434 CO_2 and 0·155 H_2O.
0·561 grm. gave 0·419 AgI.

(C) From the compound $C_{68}H_{81}IN_4O_{10}$, 4HI,

0·377 grm. gave 0·485 CO_2 and 0·175 H_2O.
0·387 grm. gave 0·2855 AgI.

	Calculated.		Found. A.	B.	C.	Mean.
C_{68}	816	36·69	36·95	35·98	35·09	36·01
H_{111}	111	4·98	4·93	5·24	5·16	5·11
I_7	889	39·98	39·16	40·35	39·87	39·79
N_4	56	2·52				
O_{22}	352	15·83				
$C_{68}H_{107}I_3N_4O_{22}$, 4HI	2224	100·00				

Hence this body is formed by the equations

$$C_{68}H_{80}N_4O_{10}, 4HI+3HI+12H_2O=C_{68}H_{107}I_3N_4O_{22}, 4HI,$$

$$C_{68}H_{81}IN_4O_{10}, 4HI+2HI+12H_2O=C_{68}H_{107}I_3N_4O$$

On treating the compound $C_{68}H_{89}N_4O_{10}$, 4HI in the r
duct is obtained only differing from the original substan
lent of HI; dried at 100°,

0·3995 grm. gave 0·6800 CO_2 and 0·197 H_2O.
0·3215 grm. gave 0·2200 AgI.

	Calculated.		Found.
C_{68}	816	46·36	46·41
H_{93}	93	5·28	5·48
I_5	635	36·09	36·97
N_4	56	3·18	
O_{10}	160	9·09	
$C_{68}H_{89}IN_4O_{10}, 4HI$	1760	100·00	

Hence this body appears to have been formed by the reaction

$$C_{68}H_{88}N_4O_{10}, 4HI + HI = C_{68}H_{89}IN_4O_{10}, 4HI,$$

no water having been taken up; whilst in the case of the other non-iodized base, 3 molecules of HI and 12 of H_2O are assimilated.

In order to see if the combined action of phosphorus and hydriodic acid would transform the compound $C_{68}H_{86}I_2N_4O_{12}, 4HI$ into the body $C_{68}H_{82}I_2N_4O_8, 4HI$, the former compound was dissolved in about 10 parts of 55 per cent. hydriodic acid, and boiled until most of the acid had volatilized; a considerable quantity of phosphoric acid was formed during the reaction, and on precipitating the compound produced with water, and drying at 100°, the following numbers were obtained:—

0·2795 grm. gave 0·3900 CO_2 and 0·136 H_2O.
0·4195 grm. gave 0·327 Ag I.
0·4225 grm. gave 0·333 Ag I.

	Calculated.		Found.	
C_{68}	816	38·42	38·06	
H_{107}	107	5·04	5·41	
I_7	889	41·85	42·12	42·59
N_4	56	2·64		
O_{16}	256	12·05		
$C_{68}H_{103}I_3N_4O_{16}, 4HI$	2124	100·00		

From these numbers it appears that the substance produced may be considered as formed by the reactions

$$C_{68}H_{86}I_2N_4O_{12}, 4HI + 8HI = I_8 + 6H_2O + C_{68}H_{82}I_2N_4O_8, 4HI,$$

$$C_{68}H_{82}I_2N_4O_8, 4HI + HI + 10H_2O = C_{68}H_{103}I_3N_4O_{16}, 4HI.$$

It is not easy to explain why the reaction should stop short at the end of the first stage, when codeia is treated with hydriodic acid and boiled up to 130°; possibly the presence of a much larger quantity of phosphorus acids in this case may tend to prevent the second reaction ensuing.

§ 5. *Discussion of the foregoing Results.*

From the complex constitution of the substances described above, it is at present thought unadvisable to attempt to give names to them.

All the bodies previously mentioned may be regarded as being derived from one or other of the two bases $C_{17}H_{21}NO_3$ and $C_{17}H_{21}NO_2$ by multiplication of the molecule, and addition or subtraction of the elements of water and of hydriodic acid.

These two (hypothetical) bases contain H_2 more than morphia and deoxymorphia respectively; denoting the first by the symbol X, and the second by Y, the following general formulæ will indicate all the compounds previously described:—

$$4X+m\mathrm{HI}\pm n\mathrm{H_2O},$$
$$4Y+m\mathrm{HI}\pm n\mathrm{H_2O}.$$

Thus the following Table illustrates the mutual relations of the compounds described:—

Source of Compound.	Formulæ.	
(A) Codeia, HI, and P at 100°	$C_{68}H_{86}I_2N_4O_{12}$, 4HI	$=4X+6\mathrm{HI}$.
(B) Do. at 110°–115°	$C_{68}H_{82}I_2N_4O_{10}$, 4HI	$=4X+6\mathrm{HI}-2\mathrm{H_2O}$.
(C) (A) treated with water or Na_2CO_3	$C_{68}H_{81}IN_4O_{10}$, 4HI	$=4X+5\mathrm{HI}-2\mathrm{H_2O}$.
(D) Free base of (C). (A) treated with Na_2CO_3..	$C_{68}H_{81}IN_4O_{10}$,	$=4X+\mathrm{HI}-2\mathrm{H_2O}$.
(E) Further action of water on (C)	$C_{68}H_{80}N_4O_{10}$, 4HI	$=4X+4\mathrm{HI}-2\mathrm{H_2O}$.
(F) Action of HI on (E) .	$C_{68}H_{107}I_3N_4O_{22}$, 4HI	$=4X+7\mathrm{HI}+10\mathrm{H_2O}$.
(G) Codeia, HI, and P at 135°	$C_{68}H_{82}I_2N_4O_6$, 4HI	$=4Y+6\mathrm{HI}-2\mathrm{H_2O}$.
(H) Action of water on (G)	$C_{68}H_{88}N_4O_{10}$, 4HI	$=4Y+4\mathrm{HI}+2\mathrm{H_2O}$.
(I) Action of HI on (H)..	$C_{68}H_{89}IN_4O_{10}$, 4HI	$=4Y+5\mathrm{HI}+2\mathrm{H_2O}$.
(J) Action of HI on (A)..	$C_{68}H_{103}I_3N_4O_{16}$, 4HI	$=4Y+7\mathrm{HI}+8\mathrm{H_2O}$.

Although it may well happen that further researches may show that some of the above formulæ require some slight correction, the analytical numbers not always agreeing together absolutely accurately, yet the following points may be considered established:—

(1) The action of hydriodic acid on codeia polymerizes it, the ultimate compounds formed being derived from at least four molecules of codeia. Hydrobromic acid also polymerizes codeia, but not so completely as hydriodic acid, there being formed, in addition to the tetra-bases, compounds which (from their crystalline character and other physical properties) are apparently derived from one molecule only of codeia. Hydrochloric acid does not appear to have a marked polymerizing effect on codeia.

(2) Hydriodic, hydrobromic, and hydrochloric acids all eliminate methyl from codeia, forming ultimately compounds containing nC_{17}.

(3) The compounds got by the action of hydriodic acid in presence of

phosphorus indicate that the carbon groups contained in codeia are in an eminently "unsaturated" condition, being capable of taking up several molecules of HI and of H_2O, forming compounds not decomposed at 100°, 2 equivalents of hydrogen for every C_{17} being also added on in every case.

III. "On a Periodic Change of the Elements of the Force of Terrestrial Magnetism discovered by Professor HORNSTEIN." Communicated by the Foreign Secretary. Received July 22, 1871.

[From the Anzeiger der k. Akademie der Wissenschaften in Wien for June 15, 1871.]

Professor Hornstein, of Prague, has communicated to the Imperial Academy of Sciences of Vienna a paper entitled "On the dependence of the Earth's Magnetism on the Rotation of the Sun."

He shows that the changes of each of the three elements of the force of terrestrial magnetism (declination, inclination, and horizontal force) indicate a period of 26⅓ days. The periodic change of declination for Prague (1870) amounts to 0·705 sin $(x+190° 20')$, where $x=0°$ at the commencement of 1870, and $x=360°$ at the commencement of 1871. For Vienna the range is a little larger. The range of inclination is nearly one-third of that of declination, that of the intensity nearly 24 units of the 4th decimal (the intensity in June 1870 was nearly 2·0485).

Professor Hornstein regards these changes of the earth's magnetism as the effect of the sun's rotation, and by a mean of several determinations finds for the duration of the period 26·33 days. This number may consequently be regarded as the result of the first attempt to determine the synodic period of the sun's rotation by means of the magnetic needle. The resulting true periodic time of the sun's rotation is 24·55 days, almost exactly agreeing with the time of rotation of the sun-spots in the sun's equator deduced from astronomical observations (according to Spörer 24·541 days).

IV. "Corrections to the Computed Lengths of Waves of Light published in the Philosophical Transactions of the year 1868." By GEORGE BIDDELL AIRY, C.B., Astronomer Royal. Received October 2, 1871.

(Abstract.)

The author, after adverting to the process by which in a former paper he had attempted the computation of the Lengths of Waves of Light, for the entire series measured in the Solar Spectrum by Kirchhoff, from a limited number of measured Wave-Lengths, and to the discordances between the results of these computations and the actual measure of numerous wave-lengths to which he subsequently had access, calls attention to his remark that means existed for giving accuracy to the whole. The object of the present paper is so to use these means as to produce a table of corrections

applicable through the entire range of Kirchhoff's lines, and actually to apply the corrections to those computed wave-lengths which relate to spectral lines produced by the atmosphere and by many metals.

Adopting as foundation the comparisons with Ångström's and Ditscheiner's measures given in the former paper, and laying these down graphically, the author remarks that in some parts of the spectrum the agreement of the two experimenters is very close, that in some parts they are irreconcilable, and that in one part (where they agree) there is a peculiarity which leads to the supposition that some important change was made in Kirchhoff's adjustments. He then explains the considerations on which he has drawn a correction-curve, whose ordinates are to give the corrections applicable to his former computed numbers. A general table of corrections is then given, and this is followed by tables of the Lengths of the Light-Waves for the air and metals as corrected by the quantities deduced from that general table.

The author remarks that he has not yet succeeded in discovering any relation among the wave-lengths for the various lines given by any one metal &c. which can suggest any mechanical explanation of their origin.

V. "Corrections and Additions to the Memoir on the Theory of Reciprocal Surfaces, Phil. Trans. vol. clix. (1869)." By Prof. CAYLEY, F.R.S. Received July 22, 1871.

1. I am indebted to Dr. Zeuthen for the remark that although the "off-points" and "off-planes," as explained in the memoir, are real singularities, they are not the singularities to which the θ, θ' of the formulæ refer. The most convenient way of correcting this is to retain all the formulæ with θ, θ' as they stand, but to write ω, ω' for the number of "off-points" and "off-planes" respectively; viz. we thus have

ω, off-points,
θ, unexplained singular points,

and

ω', off-planes,
θ', unexplained singular planes,

the formulæ as they stand, taking account of the unexplained singularities θ and θ', but not taking any account at all of the off-points and off-planes ω, ω'. The extended formulæ in which these are taken into account are:—

$$\begin{aligned}
a(n-2)&=\kappa-B+\rho+2\sigma+3\omega,\\
b(n-2)&=\rho+2\beta+3\gamma+3t,\\
c(n-2)&=2\sigma+4\beta+\gamma+\theta+\omega,\\
a(n-2)(n-3)&=2(\delta-C-3\omega)+3(ac-3\sigma-\chi-3\omega)+2(ab-2\rho-j),\\
b(n-2)(n-3)&=4k+(ab-2\rho-j)+3(bc-3\beta-2\gamma-i),\\
c(n-2)(n-3)&=6h+(ac-3\sigma-\chi-3\omega)+2(bc-3\beta-2\gamma-i),
\end{aligned}$$

which replace Salmon's original formulæ (A) and (B).

2. In the formulæ

$$q=b^2-b-2k-3\gamma-6t,$$
$$r=c^2-c-2h-3\beta,$$

it is assumed that the nodal curve has no actual multiple points other than the t triple points, and no stationary points other than the γ points which lie on the cuspidal curve; and similarly that the cuspidal curve has no actual multiple points, and no stationary points other than the β points which lie on the nodal curve; and this being so, q is the class of the nodal curve and r that of the cuspidal curve. But we may take the formulæ as *universally* true; viz. q may be considered as standing for $b^2-b-2k-3\gamma-6t$, and r as standing for $c^2-c-2h-3\beta$; only then q and r are not in all cases the classes of the two curves respectively.

3. In the formulæ No. 6 *et seq.*, introducing the new singularity ω, we have as follows:—

$$(a-b-c)(n-2) \qquad =(\kappa-\mathrm{B}-\theta+2\omega)-6\beta-4\gamma-3t,$$
$$(a-2b-3c)(n-2)(n-3)=2(\delta-\mathrm{C}-3\omega)-8k-18h-12(bc-3\beta-2\gamma-i);$$

and substituting these in $n'=a(a-1)-2b-3c$, and writing for n' its value $=a(a-1)-2\delta-3\kappa$, we have, as in the memoir,

$$\begin{aligned}n'=n(n-1)^2-n(7b+12c)+4b^2+8b+9c^2+15c\\ -8k-8h+18\beta+12\gamma+12i-9t\\ -2\mathrm{C}-3\mathrm{B}-3\theta;\end{aligned}$$

viz. there is no term in ω.

Writing $(n-2)(n-3)=a+2b+3c+(-4n+6)$ in the equations which contain $(n-2)(n-3)$, these become

$$\begin{aligned}a(-4n+6)&=2(\delta-\mathrm{C})-\ a^2-4\rho-9\sigma-2j-3\chi-15\omega,\\ b(-4n+6)&=\ \ 4k\quad -2b^2-9\beta-6\gamma-3i-2\rho-j,\\ c(-4n+6)&=\ \ 6h\quad -3c^2-6\beta-4\gamma-2i-3\sigma-\chi-3\omega,\end{aligned}$$

(Salmon's equations (C)); and adding to each equation four times the corresponding equation with the factor $(n-2)$, these become

$$\begin{aligned}a^2-2a&=2(\delta-\mathrm{C})+4(\kappa-\mathrm{B})-\sigma-2j-3\chi-3\omega,\\ 2b^2-2b&=4k-\beta+6\gamma+12t-3i+2\rho-j,\\ 3c^2-2c&=6h+10\beta+4\theta-2i+5\sigma-\chi+\omega.\end{aligned}$$

Writing in the first of these $a^2-2a=n'+2\delta+3\kappa-a$, and reducing the other two by means of the values of q, r, the equations become

$$\begin{aligned}n'-a&=-2\mathrm{C}-4\mathrm{B}+\kappa-\sigma-2j-3\chi-3\omega,\\ 2q+\beta+3i+j&=2\rho,\\ 3r+c+2i+\chi&=5\sigma+\beta+4\theta+\omega.\end{aligned}$$

The reciprocal of the first of these is

$$a'=a-n+\kappa'-2j'-3\chi'-2\mathrm{C}'-4\mathrm{B}'-3\omega';$$

viz. writing $a=n(n-1)-2b-3c$, and $\kappa=3n(n-2)-6b-8c$, this is

$$\sigma'=4n(n-2)-8b-11c-2j'-3\chi'-2C'-4B'-3\omega';$$

and it thus appears that the order σ' of the spinode curve is reduced by 3 for each off-plane ω'.

4. As to the other two equations, writing for ρ, σ their values, these become

$$j+6t+3i+5\beta+6\gamma=b(2n-4)-2q,$$
$$2\chi+3\omega+4i+18\beta+5\gamma=c(5n-12)-6r+3\theta,$$

equations which admit of a geometrical interpretation. In fact when there is only a nodal curve, the first equation is

$$j+6t=b(2n-4)-2q,$$

which we may verify when the nodal curve is a complete intersection, $P=0$, $Q=0$; for if the equation of the surface is $(A, B, C\between P, Q)^2=0$, where the degrees of A, B, C, P, Q are $n-2f$, $n-f-g$, $n-2g$, f, g respectively, then the pinch-points are given by the equations $P=0$, $Q=0$, $AC-B^2=0$, and the number j of pinch-points is thus

$$=fg(2n-2f-2g)=(2n-4)fg-2fg(f+g-2);$$

but for the curve $P=0$, $Q=0$ we have $t=0$, and its order and class are $b=fg$, $q=fg(f+g-2)$, or the formula is thus verified.

Similarly when there is only a cuspidal curve, the second equation is

$$2\chi+3\omega=c(5n-12)-6r+3\theta,$$

which may be verified when the cuspidal curve is a complete intersection, $P=0$, $Q=0$; the equation of the surface is here $(A, B, C\between P, Q)^2=0$, where $AC-B^2=MP+NQ$, and the points χ, ω are given as the intersections of the curve with the surface $(A, B, C\between N, -M)^2=0$.

Now $AC-B^2$ vanishing for $P=0$, $Q=0$ we must have $A=\Lambda\alpha^2+A'$, $B=\Lambda\alpha\beta+B'$, $C=\Lambda\beta^2+C'$, where A', B', C' vanish for $P=0$, $Q=0$; and thence $M=\Lambda M'+M''$, $N=\Lambda N'+N''$, where M'', N'' vanish for $P=0$, $Q=0$. The equation $(A, B, C\between N, -M)^2=0$, writing therein $P=0$, $Q=0$, thus becomes $\Lambda^3(N'\alpha-M'\beta)^2=0$; and its intersections with the curve $P=0$, $Q=0$ are the points $P=0$, $Q=0$, $\Lambda=0$ each three times, and the points $P=0$, $Q=0$, $N'\alpha-M'\alpha=0$ each twice; viz. they are the points $2\chi+3\omega$.

But if the degree of Λ is $=\lambda$, then the degrees of N', M', α^2, $\alpha\beta$, β^2 are $2n-3f-2g-\lambda$, $2n-2f-3g-\lambda$, $n-2f-\lambda$, $n-f-g-\lambda$, $n-2g-\lambda$, whence the degree of $\Lambda^3(N'\alpha-M'\beta)$ is $=5n-6f-6g$, and the number of points is $=fg(5n-6f-6g)$, viz. this is $=fg(5n-12)-6fg(f+g-2)$, or it is $=c(5n-12)-6r$; so that θ being $=0$, the equation is verified.

5. It was also pointed out to me by Dr. Zeuthen that in the value of $24t$ given in No. 10 the term involving χ should be -6χ instead of $+6\chi$,

and that in consequence the coefficients of χ are erroneous in several others of the formulæ. Correcting these, and at the same time introducing the terms in ω, and writing down also the terms in θ as they stand, we have

$$\begin{aligned}
4i&=\ldots-2\chi+3\theta-3\omega,\\
24t&=\ldots-6\chi+9\theta-9\omega,\\
2\sigma&=\ldots\quad-\theta-\omega,\\
8\rho&=\ldots+6\chi-9\theta+9\omega,\\
8\kappa&=\ldots-6\chi+17\theta-25\omega,\\
2\delta&=\ldots+6\chi-9\theta+15\omega,\\
8n'&=\ldots-30\chi+21\theta-45\omega,\\
c'&=\ldots-12\chi+10\theta-20\omega.
\end{aligned}$$

The equations of No. 11 used afterwards, No. 53, should thus be

$$\begin{aligned}
4i+6r&=(5n-12)c-18\beta-5\gamma-2\chi+3\theta-3\omega\\
-24t-8q+18r&=(-8n+16)b+(15n-36)c-34\beta+9\gamma+4j-6\chi+9\theta-9\omega;
\end{aligned}$$

and from these I deduce

$$\begin{aligned}
44q+\tfrac{63}{2}r=(44n-88)b+\left(\tfrac{105}{4}n-63\right)c-\tfrac{409}{2}\beta-\tfrac{633}{4}\gamma-132t\\
-87i-22j-\tfrac{21}{2}\chi+\tfrac{63}{4}\theta.
\end{aligned}$$

6. In No. 32 we have (without alteration) $\theta=16$; but in the application (Nos. 40 and 41) to the surface $FP^2+GR^2Q^3=0$ we have $\theta=0$, and there are $\omega=fpq$ off-points, $F=0$, $P=0$, $Q=0$, and $\chi=gpq$ close-points, $G=0$, $P=0$, $Q=0$. The new equations involving ω are thus satisfied.

7. I have ascertained that the value of β' obtained, Nos. 51 to 64 of the memoir, is inconsistent with that obtained in the "Addition" by consideration of the deficiency, and that it is in fact incorrect. The reason is that, although, as stated No. 53, the values of two of the coefficients D, E may be assumed at pleasure, they cannot, in conjunction with a given system of values of A, B, C, be thus assumed at pleasure; viz. A, B, C being $=110, 272, 44$ respectively, the values of D, E are really determinate. I have no direct investigation, but by working back from the formula in the Addition I find that we must have $D=\frac{477}{4}$, $E=315$; the values of the remaining coefficients then are

$$F=\tfrac{63}{2},\ G=-\tfrac{715}{2},\quad H=-\tfrac{1005}{4},\ I=-198;$$

or the formula is

$$\begin{aligned}
\beta'=2n(n-2)(11n-24)\\
-(110n-272)b+44q\\
-(\tfrac{477}{4}n-315)c+\tfrac{63}{2}r\\
+\tfrac{715}{2}\beta+\tfrac{1005}{4}+198t\\
-hC-gB-xi-\lambda j-\mu\chi-\nu\theta-f\omega\\
-h'C'-g'B'-x'i'-\lambda'j'-\mu'\chi'-\nu'\theta'-f'\omega';
\end{aligned}$$

but I have not as yet any means of determining the coefficients f, f' of the terms in ω, ω'.

From the several cases of a cubic surface we obtain as in the memoir; but applying to the same surfaces the reciprocal equation for β, instead of the results of the memoir, we find

$$\begin{aligned} h' &= -\ \ 4, \\ g'+16\nu &= -198, \\ g'+2\mu &= \ \ 45, \\ g+g' &= \ \ 18, \\ \lambda &= \ \ \ 5 \end{aligned}$$

(so that now $\lambda+\lambda'=-2$, as is also given by the cubic scroll). And combining the two sets of results, we have

$$\begin{aligned} h &= \ \ 24, \\ \lambda &= \ \ 5, \\ \mu &= \tfrac{27}{2}+\tfrac{1}{2}g, \\ \nu &= -\tfrac{27}{2}+\tfrac{1}{16}g, \\ h' &= -\ 4, \\ g' &= \ \ 18-g, \\ \lambda' &= -\ 7, \\ \mu' &= 6-\tfrac{1}{2}g, \\ \nu' &= \tfrac{9}{4}-\tfrac{1}{16}g; \end{aligned}$$

but the coefficients g, x, x', f, f' are still undetermined. To make the result agree with that of the Addition, I assume $x=-86$, $x'=-1$, $g=+28$; whence we have

$$\begin{aligned} \beta' = {} & 2n(n-2)(11n-24) \\ & -(110n-272)b+44q \\ & -(\tfrac{477}{4}n-315)c+\tfrac{63}{2}r \\ & +\tfrac{715}{2}\beta+\tfrac{1005}{4}\gamma+198t \\ & -24C-28B+86i-5j-\tfrac{55}{2}\chi+\tfrac{47}{4}\theta-f\omega \\ & +4C'+10B'+i'+7j'+8\chi'-\tfrac{1}{2}\theta'-f'\omega'; \end{aligned}$$

and if we substitute herein the foregoing value of $44q+\frac{63}{2}r$, we obtain

$$\begin{aligned} \beta' = {} & 2n(n-2)(11n-24) \\ & +(-66n+184)b \\ & +(-93n+252)c \\ & +153\beta+93\gamma+66t \\ & -24C-28B-i-27j-38\chi+\tfrac{55}{2}\theta-f\omega \\ & +4C'+10B'+i'+7j'+8\chi'-\tfrac{1}{2}\theta'-f'\omega', \end{aligned}$$

which, except as to the terms in ω, ω', the coefficients of which are not determined, agrees with the value given in the Addition.

Dr. Zeuthen considers that in general $i'=i$; I presume this is so, but have not verified it.

November 23, 1871.

General Sir EDWARD SABINE, K.C.B., President, followed by Mr. FRANCIS GALTON, Vice-President, in the Chair.

In pursuance of the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Officers and Council proposed for election was read as follows:—

President.—George Biddell Airy, C.B., M.A., D.C.L., LL.D. (Astronomer Royal).

Treasurer.—William Spottiswoode, M.A.

Secretaries.— { William Sharpey, M.D., LL.D.
Prof. George Gabriel Stokes, M.A., D.C.L., LL.D.

Foreign Secretary.—Prof. William Hallowes Miller, M.A., LL.D.

Other Members of the Council.—George James Allman, M.D.; John Ball, M.A.; George Burrows, M.D.; Mr. George Busk, P.R.C.S.; Prof. Robert Bellamy Clifton, M.A.; Heinrich Debus, Ph.D.; Prof. Peter Martin Duncan, M.B.; Prof. George Carey Foster, B.A.; Mr. Francis Galton; Thomas Archer Hirst, Ph.D.; Sir John Lubbock, Bart.; Sir James Paget, Bart., D.C.L.; The Earl of Rosse, D.C.L.; General Sir E. Sabine, R.A., K.C.B.; Isaac Todhunter, M.A.; Sir Charles Wheatstone, D.C.L.

Mr. C. F. Varley was admitted into the Society.

A letter from Earl Stanhope, addressed to the Secretary, to be laid before the Council, was read, offering for their acceptance, on behalf of the Society, plaster busts of Martin Folkes (formerly P.R.S.) and Dr. Franklin, which have long been in the possession of his Lordship's family.

Pursuant to notice given at the last Meeting, the President proposed, and the Astronomer Royal seconded, the Emperor of Brazil for election and immediate ballot.

The ballot having been taken, His Majesty Pedro II., Emperor of Brazil, was declared duly elected.

The following communications were read:—

I. Second Paper "On the Numerical Values of e, $\log_e 2$, $\log_e 3$, $\log_e 5$, and $\log_e 10$; also on the Numerical Value of M, the Modulus of the Common System of Logarithms, all to 205 decimals." By WILLIAM SHANKS, Houghton-le-Spring, Durham. Communicated by Prof. G. G. STOKES, Sec. R.S. Received August 30, 1871.

In the author's former paper, inserted in vol. vi. p. 397 of the 'Proceedings of the Royal Society,' the values of e, and of the above mentioned logs, also

that of M, are given. Quite recently Mr. Glaisher has shown that the values of these logs are inaccurate from about the 59th decimal. This inaccuracy the author regrets, and has therefore recalculated them, and now gives their values entire and corrected. The values of $\log_e$ 2, 3, 5, and 10 agree with and verify Mr. Glaisher's, as will be seen, to about the 100th decimal; and it is thought (from this and the great care used) that the remaining decimals now given are free from error. The value of e, given in the former paper, has lately been verified by Mr. Glaisher to 137 decimals. The value of M has also been recalculated; and the accuracy of its value (as far as division is concerned) may be relied on, inasmuch as proofs were taken, by casting out the nines, at every interval of five quotient figures. This number, if 205 ciphers be prefixed to it and the decimal point be removed and placed on the left of them, may be regarded as the longest non-circulating reciprocal on record, consisting of 410 decimals. The remainder after the last division is given, so that the accuracy of the division may thereby be easily tested, by contraction or otherwise.

The corrected values are as follow:—

$\log_e 2 =$ ·69314 71805 59945 30941 72321 21458 17656 80755 00134 36025
52541 20680 00949 33936 21969 69471 56058 63326 99641 86875
42061 48102 05706 85733 68552 02357 58130 55703 26707 51635
07596 19307 27570 82837 14351 90307 03862 38916 73471 12335
01018 ± &c.

$\log_e 3 =$ 1·09861 22886 68109 69139 52452 36922 52570 46474 90557 82274
94517 34694 33363 74942 93218 60896 68736 15754 81373 20887
88070 02906 59578 65742 36800 42259 30519 82105 28018 70767
27741 06031 62769 18338 13671 79373 69884 43609 59903 74257
02948 ± &c.

$\log_e 5 =$ 1·60943 79124 34100 37460 07593 33226 18763 95256 01354 26851
77219 12647 89147 41789 87707 65776 46301 33878 09317 96108
00106 30302 17155 62899 72400 52293 24676 19963 36166 17463
70572 75521 79637 49718 32456 53492 85620 23415 25057 27015
51474 ± &c.

$\log_e 10 =$ 2·30258 50929 94045 68401 79914 54684 36420 76011 01488 62877
29760 33327 90096 75726 09677 35248 02359 97205 08959 82983
42167 78404 22862 48633 40952 54650 82806 75666 62873 69098
78168 94829 07208 32555 46808 43799 89482 62331 98528 39350
52492 ± &c.

M = ·43429 44819 03251 82765 11289 18916 60508 22943 97005 80366
65661 14453 78316 58646 49208 87077 47292 24949 33843 17483
18668 38440 53639 80947 79768 71211 65951 73183 60409 55627
56816 80637 45310 65045 32572 68778 26750 65871 94503 27872
73760 ± &c.

The remainder is as follows:—

17550 06500 84834 29272 80492 56652 30056 77179 51985 96380
06400 15769 36917 76044 90943 15598 00090 70477 96549 03362
30614 39569 71063 83855 24053 35869 04219 87709 24604 49236
79071 67965 18350 79865 61803 71534 89641 16619 31638 84825
79008 0

Aug. 28th, 1871.

II. Second Paper "On the Numerical Value of Euler's Constant, and on the Summation of the Harmonic Series employed in obtaining such Value." By WILLIAM SHANKS, Houghton-le-Spring, Durham. Communicated by Prof. G. G. STOKES, Sec. R.S. Received August 30, 1871.

Three cases and sources of inaccuracy in finding the value of E in the former paper (Proc. Roy. Soc. vol. xv. p. 429) having been pointed out by Mr. Glaisher, and some other minor errors not noticed by him having since been detected by the author, and these having vitiated the results, but only in a slight degree the inferences drawn from them (for in the former paper the *last and leading conclusion* as to the value of E, though *limited*, was certainly *correct*), the author has been led, from a deep sense of obligation to the Royal Society, to revise, correct, and extend what he had previously done. And it will be seen, from comparing Mr. Glaisher's remarks and results with what follows in this paper, that the supplementary matter herein given, including the extension of E &c. to 110 places of decimals, can scarcely be without interest to mathematicians, and especially as regards the summation of the harmonic series in the formula for finding the value of E.

Not having seen M. Oettinger's article in Crelle's 'Journal,' "On Computing the value of E," the author is unable to state what artifices he used in summing the harmonic series. Mr. Glaisher gives a very simple and obvious one from M. Oettinger, which the author could not but see and employ for calculating the values of the reciprocals of the even numbers.

In summing the harmonic series, the author found the reciprocals of all numbers up to 200, as far as 200 places of decimals; next the reciprocals from 200 to 500, to only 105 decimals; and afterwards the reciprocals of the odd composite numbers up to 5000, to the same extent. In passing from S_{2000} to S_{5000} some extra calculation was necessary, which need not be stated here. It is, however, necessary to calculate, *in extenso*, the reciprocals of the odd composite numbers only to half the number of terms which it is proposed to sum. The reciprocals of all the prime numbers must of course be calculated separately.

The leading artifices the author employed to shorten calculation may be best stated and explained by supposing that the reciprocals of all the odd numbers below 5000 have been computed and retained separately, also

that the sum of the series to 5000 terms has been found, and that the series is required to be summed to 10,000 terms.

To obtain the sum of the reciprocals of the even numbers from 5000 to 10,000, we have:—

$(\frac{1}{2501}+\frac{1}{2503}+\ldots\frac{1}{4999})\div 2.$ $(\frac{1}{41}+\ldots\ldots\frac{1}{77})\div 2^7.$

$(\frac{1}{1251}+\frac{1}{1253}+\ldots\frac{1}{2499})\div 2^2.$ $(\frac{1}{21}+\ldots\ldots\frac{1}{39})\div 2^8.$

$(\frac{1}{627}+\ldots\ldots\frac{1}{1249})\div 2^3.$ $(\frac{1}{11}+\ldots\ldots\frac{1}{19})\div 2^9.$

$(\frac{1}{313}+\ldots\ldots\frac{1}{625})\div 2^4.$ $(\frac{1}{5}+\ldots\ldots\frac{1}{9})\div 2^{10}.$

$(\frac{1}{157}+\ldots\ldots\frac{1}{311})\div 2^5.$ $\frac{1}{3}\div 2^{11}.$

$(\frac{1}{79}+\ldots\ldots\frac{1}{155})\div 2^6.$ $1\div 2^{13}.$

These twelve quotients, when added together, give the value of the reciprocals of the even numbers between 5000 and 10,000, including the latter number.

To obtain the sum of the reciprocals of the odd composite numbers between 5000 and 10,000, we have, using *prime* divisors,

$$(\frac{1}{1667}+\frac{1}{1669}+\ldots\frac{1}{3333})\div 3.$$
$$(\frac{1}{1001}+\frac{1}{1003}+\frac{1}{1007}+\ldots\frac{1}{1999})\div 5.$$

Here it must be observed that *all odd numbers which are multiples of previous prime divisors* must be excluded: *e. g.* $\frac{1}{1005}$ must be excluded from division by 5, because 1005 is a multiple of 3.

$(\frac{1}{719}+\ldots\ldots\frac{1}{1427})\div 7.$ $(\frac{1}{127}+\ldots\ldots\frac{1}{241})\div 41.$

$(\frac{1}{457}+\ldots\ldots\frac{1}{907})\div 11.$ $(\frac{1}{127}+\ldots\ldots\frac{1}{229})\div 43.$

$(\frac{1}{389}+\ldots\ldots\frac{1}{769})\div 13.$ $(\frac{1}{107}+\ldots\ldots\frac{1}{211})\div 47.$

$(\frac{1}{307}+\ldots\ldots\frac{1}{587})\div 17.$ $(\frac{1}{97}+\ldots\ldots\frac{1}{181})\div 53.$

$(\frac{1}{269}+\ldots\ldots\frac{1}{523})\div 19.$ $(\frac{1}{89}+\ldots\ldots\frac{1}{167})\div 59.$

$(\frac{1}{223}+\ldots\ldots\frac{1}{433})\div 23.$ $(\frac{1}{83}+\ldots\ldots\frac{1}{163})\div 61.$

$(\frac{1}{173}+\ldots\ldots\frac{1}{337})\div 29.$ $(\frac{1}{79}+\ldots\ldots\frac{1}{149})\div 67.$

$(\frac{1}{163}+\ldots\ldots\frac{1}{317})\div 31.$ $(\frac{1}{71}+\ldots\ldots\frac{1}{139})\div 71.$

$(\frac{1}{137}+\ldots\ldots\frac{1}{269})\div 37.$

Here it must be noted that all numbers *below* prime divisors must always be excluded.

$(\frac{1}{73}+\ldots\ldots\frac{1}{131})\div 73.$ $(\frac{1}{89}+\ldots\ldots\frac{1}{109})\div 89.$

$(\frac{1}{79}+\ldots\ldots\frac{1}{113})\div 79.$ $(\frac{1}{97}+\ldots\ldots\frac{1}{103})\div 97.$

$(\frac{1}{83}+\ldots\ldots\frac{1}{113})\div 83.$

These twenty-four quotients, when added together, give the sum of the reciprocals of the odd composite numbers between 5000 and 10,000. To this sum add the sum of the prime reciprocals between 5000 and 10,000; the result is the value of the reciprocals of all the odd numbers between 5000 and 10,000. It need scarcely be stated that the sum of these two

distinct sets of numbers, increased by the sum of the series to 5000 terms, will give the sum of the harmonic series to 10,000 terms.

Before proceeding further it should be stated that, having obtained the correct value of E, from S_{200} &c. to 110 decimals, verifying Mr. Glaisher's to 99 decimals, it was comparatively easy to extend S_{500} and S_{1000} to 110 decimals, and to correct and extend S_{2000}, S_{5000}, and $S_{10,000}$ to the same extent.

When we have S_{100}, the calculations from Bernoulli's 31 numbers will lead to obtaining E only to about 92 decimals. This value may no doubt be extended by finding the ratio between the last and each succeeding Bernoulli's number. Such ratio is, however, only approximative, and can yield correct results of only a limited number of decimals. The excess of the + Bernoulli terms over the — ones, to 110 decinals, when S_{100} is used, is readily obtained when E and $\log_e 100$ are known to the same extent. Such excess will be found below; also the separate sums of the + and — terms in which Bernoulli's numbers enter, both when $S=100$ and when $S=200$, to 205 decimals.

The values of S_{500}, S_{1000}, S_{2000}, S_{5000}, and $S_{10,000}$ to 110 decimals, also the corresponding + and — results of the Bernoulli terms to the same extent, are likewise given below, as they involve very considerable calculation, and may thus be tested and verified. The values of S_{100} and S_{200} may as well be also written anew, inasmuch as a few slight errors had crept into them before.

E =	·57721	56649	01532	86060	65120	90082	40243	10421	59335	93992
	35988	05767	23488	48677	26777	66467	09369	47063	29174	67495
	11141	14421......								
S_{100} =	5·18737	75176	39620	26080	51176	75658	25315	79089	72126	70845
	16531	76533	95658	72195	57532	55049	66056	87768	92312	04135
	49921	06986	97779	79182	73403	18717	00828	94825	42444	49096
	57618	56474	16326	13467	07313	21114	47132	49733	09103	51129
	......									
S_{200} =	5·87803	09481	21444	47605	73863	97130	86163	68374	00246	53024
	30844	64971	94472	28783	30029	84018	15499	64301	86679	89238
	37326	83211	85439	05911	76542	77755	27568	86559	30203	06046
	25715	75389	22254	75748	47845	75246	64079	54805	61627	08880
	......									
S_{500} =	6·79282	34299	90524	60298	92871	45367	97369	48198	13814	39677
	91166	43088	89685	43566	23790	55049	24576	49403	73586	56039
	14705	68279......								
S_{1000} =	7·48547	08605	50344	91265	65182	04333	90017	65216	79169	70880
	36657	73626	74995	76993	49165	20244	09599	34437	41184	50813
	93907	71134......								
S_{2000} =	8·17836	81036	10282	40957	76565	71641	69368	79354	66740	91248
	77402	20419	74812	15302	80688	34328	60377	35324	29687	02614
	20643	33506......								

S_{5000} = 9·09450 88529 84436 96726 12455 33393 43939 17819 87811 30381 14506 16283 85209 05328 30500 87619 93914 09299 23691 97409 31969 93538......

$S_{10,000}$ = 9·78760 60360 44382 26417 84779 04851 60533 48592 62945 57769 16183 89460 95668 16020 24943 15950 68001 25127 29008 08825 88669 45713......

When we have S_{100} we have excess of + Bernoulli terms over − Bernoulli terms as follows:—

+·00000 83332 50003 96783 73773 23792 87768 83353 90186 48901 78976 95889 08023 27933 88599 81913 48032 53704 54782 29326 45555 64243......

For S_{100} the sum of the + Bernoulli terms is

+·00000 83333 33337 30158 73773 44885 67821 37321 67823 08773 00082 30639 33761 49846 36254 14224 82920 15089 51978 53129 32622 35457 80834 33466 17877 22649 42919 19943 83311 44605 09910 37086 58144 47970 79756 13813 15081 18650 05862 58830 54265......

For S_{100} the sum of the − Bernoulli terms is

−·00000 00000 83333 33375 00000 21092 80052 53967 77636 59871 21105 34750 25738 21912 47654 32311 34887 61384 93958 76677 70933 50276 12351 60205 99358 40193 39578 24455 06318 69180 82191 70839 75485 79804 74621 35198 75347 46810 82718 00710 69953......

For S_{200} the sum of the + Bernoulli terms is

+·00000 20833 33333 39533 73016 61283 59538 59711 91078 12258 22890 92137 92329 17052 51095 23490 89894 78957 35141 06774 04433 65950 49748 89147 11592 75754 43919 38271 38143 50271 65525 60887 11534 99567 61791 58429 85003 06150 05862 58830 54265......

For S_{200} the sum of the − Bernoulli terms is

−·00000 00000 05208 33333 49609 37505 14960 84887 28877 09510 05685 64006 82170 11770 13023 01074 35246 38458 75084 75675 04222 29829 71026 50819 24197 13084 02015 36765 99447 44527 25466 44760 75767 81146 37524 18401 87847 46810 82718 00710 69953......

For S_{500} the sum of the + Bernoulli terms is

+·00000 03333 33333 33358 73015 87309 34487 73462 42678 21147 87864 83345 53789 61875 16264 59799 20761 22352 44796 75809 19590 05554......

For S_{500} the sum of the $-$ Bernoulli terms is

$-$·00000 00000 00133 33333 33344 00000 00008 63960 92825 14227 06303 41363 50645 63522 06215 12108 84946 96404 61971 02278 38712 72302......

For S_{1000} the sum of the $+$ Bernoulli terms is

$+$·00000 00833 33333 33333 73015 87301 59487 73448 77428 21067 82137 32165 00876 22580 99464 52882 37182 41954 63931 48991 92230 67302......

For S_{1000} the sum of the $-$ Bernoulli terms is

$-$·00000 00000 00008 33333 33333 37500 00000 00210 92796 09323 93526 00040 82093 23718 92820 00915 30332 37713 49061 83360 48493 88803......

For S_{2000} the sum of the $+$ Bernoulli terms is

$+$·00000 00208 33333 33333 33953 37301 58730 89855 02344 88243 11403 65711 84320 18015 80288 93393 93977 27188 21088 95868 23862 44594......

For S_{2000} the sum of the $-$ Bernoulli terms is

$-$·00000 00000 00000 52083 33333 33349 60937 50000 05149 60842 10995 59700 64404 23526 83197 86039 81846 60506 95079 95161 64799 80365......

For S_{5000} the sum of the $+$ Bernoulli terms is

$+$·00000 00033 33333 33333 33335 87301 58730 15880 77344 87734 48909 98210 67821 14787 86482 16512 65337 39125 53345 19304 37661 22499......

For S_{5000} the sum of the $-$ Bernoulli terms is

$-$·00000 00000 00000 01333 33333 33333 34400 00000 00000 08639 60927 96095 70104 02470 73465 66314 96500 75868 11665 04160 31880 36102......

For $S_{10,000}$ the sum of the $+$ Bernoulli terms is

$+$·00000 00008 33333 33333 33333 37301 58730 15873 02344 87734 48773 45710 67821 06782 13732 16500 84808 26100 49665 77626 52262 19358......

For $S_{10,000}$ the sum of the $-$ Bernoulli terms is

$-$·00000 00000 00000 00083 33333 33333 33337 50000 00000 00002 10927 96092 79613 71220 73188 24992 34000 15344 13659 87023 61119 37034......

Suppose n, in the harmonic series, $=1$ followed by 1000 ciphers; then,

since n is very large, we may disregard $-\frac{1}{2n}+\frac{B_1}{2n^2}$ &c. We thus have $E=Sn-\log_e n$, or $Sn=\log_e n+E$; but $\log_e n=\log_e 10\times 1$ followed by 100 ciphers, therefore

$S_n=2\cdot30258$ 50929 94045 68401 79914 54684 36420 76011 01488 62877 29760 33327 90096 75726 09677 35248 02359 97205 08959 82983 99889 35053 24395 34694 06073 44733 23049 86088 22209 63091 14157 00596 30696 81232 73586 10266 98852 09395 27703 06845 63633......

August 28th, 1871.

III. "An Experimental Determination of the Velocity of Sound." By E. J. Stone, M.A., F.R.S., Astronomer Royal at the Cape of Good Hope. Received August 21, 1871.

(Abstract.)

A galvanic current passes from the batteries at the Royal Observatory, Cape Town, at 1 o'clock, and discharges a gun at the Castle, and through relays drops a time-ball at Port Elizabeth. It appeared to the author that a valuable determination of the velocity of sound might be obtained by measuring upon the chronograph of the Observatory the interval between the time of the sound reaching some point near the gun and that of its arrival at the Observatory. As there is only a single wire between the Observatory and Cape Town, some little difficulty was experienced in making the necessary arrangements, without any interference with the 1 o'clock current to Port Elizabeth; but this difficulty was overcome by a plan which the author describes, and which was brought into successful operation on Feb. 27, 1871. The experiments could not have been carried out, on account of the encroachment they would have made on the time of the Observatory staff, had it not been for the assistance of J. Den, Esq., the acting manager of the Cape Telegraph Company, to whom the author is indebted for the preparation of a good earth-connexion near the gun, for permission to Mr. Kirby, a gentleman attached to the telegraph office, to assist in the experiments, and for a general superintendence of the arrangements at Cape Town.

The observed times of hearing the sound were recorded on the chronograph by two observers, situated one (Mr. Kirby) at a distance of 641 feet from the gun, the other (Mr. Mann) at the Observatory, at a distance of 15,449 feet from the gun. The former distance was sufficient to allow the connexion of the main wire to be broken at the telegraph office after the gun had been fired, but before the sound reached the first observer.

As there were no reciprocal signals, a correction was made by calculation for the effect of the wind, its velocity being measured by a set of Robin-

son's cups. The personal equation, under the circumstances of the observations, was found as follows:—A gun was fired at such a distance from the Observatory as to be heard with about the same degree of distinctness as the time-gun at the Castle. This distance was found to be 1483 feet. The registrations on the chronograph were made by Mr. Kirby at the distance of 162 feet from the gun, and Mr. Mann at the Observatory. For this comparatively small distance, the time of transit calculated from the velocity deduced from the time taken to travel over the larger distance may be deemed exact. The observed time for the smaller difference of distance was found to be too great by $0^s{\cdot}09$, which correction has been applied to all the observations. It depends more on want of sensibility in picking up and recognizing faint sounds than upon mere habit of making contacts. When the observers were interchanged, the observed interval of time appeared still too large, but in this case by $0^s{\cdot}02$. It is clear that such personal equations are not eliminated by an interchange of observers, nor by return signals.

In the reduction of the equations, the coefficient of elasticity of air under a constant volume (that is to say, the ratio of the increment of pressure for an increment 1° F. of temperature to the pressure at 32° F.) was regarded as an unknown quantity as well as V, the velocity of sound at 32° F. The reduction of the equations furnished by the observations, which were 38 in number, gave

$$V=1090{\cdot}6 \text{ feet per second,}$$
$$\alpha=0{\cdot}0019,$$

Regnault's value of α being $0{\cdot}0020$.

There appeared to be but little difference between the residual errors as dependent on the motion of the air. The author grouped the residuals into two classes, according to the dampness of the air; but there appeared to be no appreciable difference in the velocity as dependent upon dampness.

IV. "On a supposed alteration in the amount of Astronomical Aberration of Light, produced by the passage of the Light through a considerable thickness of Refracting Medium." By GEORGE BIDDELL AIRY, C.B., Astronomer Royal. Received November 17, 1871.

A discussion has taken place on the Continent, conducted partly in the 'Astronomische Nachrichten,' partly in independent pamphlets, on the change of direction which a ray of light will receive (as inferred from the Undulatory Theory of Light) when it traverses a refracting medium which has a motion of translation. The subject to which attention is particularly called is the effect that will be produced on the apparent amount of that

angular displacement of a star or planet which is caused by the Earth's motion of translation, and is known as the Aberration of Light. It has been conceived that there may be a difference in the amounts of this displacement, as seen with different telescopes, depending on the difference in the thicknesses of their object-glasses. The most important of the papers containing this discussion are:—that of Professor Klinkerfues, contained in a pamphlet published at Leipzig in 1867, August; and those of M. Hoek, one published 1867, October, in No. 1669 of the 'Astronomische Nachrichten,' and the other published in 1869 in a communication to the Netherlands Royal Academy of Sciences. Professor Klinkerfues maintained that, as a necessary result of the Undulatory Theory, the amount of Aberration would be increased, in accordance with a formula which he has given; and he supported it by the following experiment:—

In the telescope of a transit-instrument, whose focal length was about 18 inches, was inserted a column of water 8 inches in length, carried in a tube whose ends were closed with glass plates; and with this instrument he observed the transit of the Sun, and the transits of certain stars whose north-polar distances were nearly the same as that of the Sun, and which passed the meridian nearly at midnight. In these relative positions, the difference between the Apparent Right Ascension of the Sun and those of the stars is affected by double the coefficient of Aberration; and the merely astronomical circumstances are extremely favourable for the accurate testing of the theory. Professor Klinkerfues had computed that the effect of the 8-inch column of water and of a prism in the interior of the telescope would be to increase the coefficient of Aberration by eight seconds of arc. The observation appeared to show that the Aberration was really increased by $7''\cdot 1$. It does not appear that this observation was repeated.

A result of physical character so important, and resting on the respectable authority of Professor Klinkerfues, merited and indeed required further examination. Having carefully considered the astronomical means which would be most accurately employed for the experiment, I decided on adopting a vertical telescope, the subject of observation being the meridional zenith distance of γ Draconis, the same star by which the existence and laws of Aberration were first established. The position of this star is at present somewhat more favourable than it was in the time of Bradley, its mean zenith-distance north at the Royal Observatory being about $100''$ and still slowly diminishing. With the sanction of the Government, therefore, I planned an instrument, of which the essential part is, that the whole tube, from the lower surface of the object-glass to a plane glass closing the lower end of the tube, is filled with water, the length of the column of water being 35·3 inches. The curvatures of the surfaces of the two lenses constituting the object-glass, adapted, in conjunction with the water, to correct spherical and chromatic aberration, were investigated by myself and verified by my friend Mr. Stone (now Astronomer at the Cape Observatory). The

micrometer is constructed on a plan arranged by myself, by which the double observation in reversed positions of the instrument can be made with great ease. The reference to the vertical is given by two spirit-levels, both to be read at every single observation. The work of construction was intrusted to Mr. James Simms, who carried it out with great ability. Distilled water was supplied by H. W. Chisholm, Esq., Warden of Standards.

Had the result of the observations been confined to the determination of an astronomical constant, or the variation of its value for different telescopes, I should not have thought it worthy of communication to the Royal Society. But it is really a result of great physical importance, not only affecting the computation of the velocity of light, but also influencing the whole treatment of the Undulatory Theory of Light. In this view I have thought that an informal statement of the conclusions may be acceptable to the Society, reserving for publication in one of the annual Greenwich Volumes the details of the observations.

The instrument was mounted in a small Occasional Observatory first constructed for the transit-instrument of Mr. Struve when he was engaged in determining the longitude of Altona, and now planted on the "South Ground" of the Observatory. The seasons at which the meridional zenith-distance of γ Draconis is most affected by aberration in opposite directions are the Equinoxes.

For understanding the following Table, it is to be remarked that an apparent value of the Geographical Latitude of the Instrument is formed from every observation, by subtracting the Observed Instrumental Zenith-distance North of the Star from the Tabular Declination of the Star given in the 'Nautical Almanac.' The observed zenith-distance is affected with the True Aberration as seen in the instrument, the tabular declination is affected with the Received Aberration used in the computation of the 'Nautical Almanac,' and the apparent value of the geographical latitude is therefore affected by the difference between the True Aberration as seen in the instrument and the Received Aberration. If, therefore, under all circumstances, and especially in the comparison of days when the sign of aberration has changed, the apparent value of the geographical latitude is sensibly constant, it proves that the True Aberration is the same as the Received Aberration, or at least that one is not a multiple of the other.

The last column of the Table is given only to show to how large an extent Aberration enters into the star's Apparent Declination.

Every result for Observed Zenith-distance in the Table is the mean of observations in reversed positions of the instrument.

Day of observation.	Star's Observed Zenith-distance North.	Star's Declination from 'Nautical Almanac.'	Difference for Geographical Latitude of Instrument.	Correction for Aberration adopted in 'Nautical Almanac.'
1871.	"	° ′ "	° ′ "	"
Feb. 28	85·30	51 29 59·3	51 28 34·0	−18·71
March 1	85·71	59·1	33·4	18·82
3	84·19	58·9	34·7	19·02
4	82·18	58·8	36·6	19·11
16	83·63	58·0	34·4	19·73
17	84·58	58·0	33·4	19·74
21	83·87	57·9	34·0	19·73
23	82·73	57·9	35·2	19·69
24	84·18	58·0	33·8	19·66
26	84·04	58·1	34·1	19·59
27	83·48	51 29 58·2	51 28 34·7	−19·54
Mean Latitude of Instrument from Spring Observations			51 28 34·4	
Aug. 29	122·10	51 30 34·4	51 28 32·3	+18·25
Sept. 5	121·84	35·0	33·2	19·01
7	121·62	35·1	33·5	19·18
9	120·27	35·2	34·9	19·33
11	122·98	35·3	32·3	19·45
15	122·20	35·4	33·2	19·64
17	121·53	35·5	34·0	19·70
22	121·38	35·5	34·1	19·74
24	120·01	35·4	35·4	19·72
Oct. 1	120·62	35·1	34·5	19·46
2	120·29	35·1	34·8	19·40
3	121·31	35·0	33·7	19·33
4	124·41	34·9	30·5	19·26
6	120·60	51 30 34·8	51 28 34·2	+19·10
Mean Latitude of Instrument from Autumn Observations			51 28 33·6	

Remarking that the mean results for Geographical Latitude of the Instrument (determined from observations made when the Aberration of the star had respectively its largest + value and its largest — value) agree within a fraction of a second, I think myself justified in concluding that the hypothesis of Professor Klinkerfues is untenable. Had it been retained, the Aberrations to be employed in the corrections would have been increased by + 15″ and — 15″ respectively, and the two mean results would have disagreed by 30″.

The latitude of the instrument from these observations is about 51° 28′ 34″·0. The position of the instrument, as measured on the Observatory Map, is 340 feet south of the Transit-circle, a spatial distance corresponding to about 3″·35. The latitude of the Transit-circle being taken at 51° 28′ 38″·4, the geodetic latitude of the instrument is 51° 28′ 35″·05, an agreement closer than I expected, consideration being given to the form of the ground. It appears very probable that at the place of the Transit-

circle, on the north brow of the hill, the zenithal direction is disturbed towards the north and the astronomical latitude is too great.

There is only one point in this investigation upon which a doubt can be suggested as possible, namely the evaluation of the micrometer-scale. It was thus conducted:—The micrometer-plate contains 26 wires, and the fixed part of the instrument contains 25 crosses, each interval being nearly 256″. With this arrangement every wire-interval is measured with great ease, and the whole series of 25 intervals is accurately obtained in terms of the micrometer. By placing the instrument in a proper position, the same intervals are obtained in time of the star's transit, which is easily converted into arc. The comparison of these gives the value of micrometer-divisions which has been employed.

The following verification, of somewhat inferior accuracy, has been made by measures of the instrument. It appears that the ray of light passes through 0·9 inch of glass, 35·3 inches of water, and 0·8 inch of air, nearly (the measure of the last being slightly uncertain). Remarking that the dividing surfaces are horizontal and plane, it is easily seen that the micrometer-scale ought to be such as is due to an air-telescope whose length in inches $=\frac{0\cdot9}{1\cdot6}+\frac{35\cdot3}{1\cdot336}+0\cdot8=27\cdot8$ inches. And from this, with observation of transit of the star, it was found that the measure of 25 intervals of wires ought to be 0·8693 inch: as measured with a pair of compasses, it was found sometimes 0·871, sometimes 0·875. The agreement is fully as close as can be expected from the rudeness of the operation, and shows distinctly that there can be no error of principle in the method of evaluing the micrometer-scale.

V. "Magnetic Survey of the East of France in 1869." By the Rev. S. J. Perry and the Rev. W. Sidgreaves. Communicated by the President. Received July 13, 1871.

(Abstract.)

This paper contains the results of a series of magnetic observations taken in the east of France during the months of August and September 1869, and is a continuation of the paper on the survey of the west of France, published in the Philosophical Transactions for 1870, p. 33.

No change was made in the observers, nor in the methods of observation, during the two surveys; and the only alteration in the instruments was the substitution in 1869 of a Jones theodolite in lieu of the small altazimuth by Cook used in 1868.

Observations were made at twenty-one stations in the following order:—Paris, Rheims, Metz, Strasbourg, Issenheim, Dôle, Mont Rolland, Dijon, Lyons, Avignon, Marseilles, Monaco, Montpellier, Grenoble, N. D. de

Myans (near Chambéry), Villefranche, St. Etienne, Clermont, Moulins, Paris, Douay, and Boulogne.

The magnetic elements are reduced to the epoch January 1st, 1869, the same as that adopted for the western stations.

Station.	Dip.	Declination.	Intensity.	Horizontal Force.
Avignon	61°·841	16°·046	9·7927	4·6224
Boulogne	67·126	18·227	10·1511	3·9458
Clermont	63·607	16·460	9·9010	4·4013
Dijon	64·409	16·612	9·9418	4·2943
Dôle	64·213	16·084	9·9307	4·3201
Douay	66·785	17·991	10·1301	3·9931
Grenoble	62·903	15·822	9·7293	4·4317
Issenheim	64·601	15·794	9·9585	4·2714
Lyons	63·268	....	9·8826	4·4454
Marseilles	60·576	15·691	9·6092	4·7207
Metz	65·458	15·976	10·0012	4·1541
Monaco	61·368	14·524	9·7189	4·6571
Montpellier	61·614	16·545	9·7512	4·6358
Mont Rolland	64·260	....	9·9692	4·3295
Moulins	64·081	16·487	9·9190	4·3356
N. D. de Myans	62·875	15·182	9·8293	4·4815
Paris	65·859	17·260	10·0618	4·1151
Rheims	65·936	16·722	10·0967	4·1170
St. Etienne	63·063	14·910	9·8472	4·4609
Strasbourg	64·687	15·578	9·9405	4·2502
Villefranche	63·498	16·942	9·8853	4·4111

The secular variations of these several elements are $-0°·054$, $-0°·1696$, $+0·0047$, and $-0·0195$. The yearly acceleration will therefore be $-0°·00082$ for the dip in 1863·5, the value found by General Sabine for the epoch 1780 to 1830 being $-0°·00085$.

As regards the horizontal force, the secular variation in the east of France is considerably less than in the west; but the change for the whole of France is almost identical with that deduced by Dr. Lamont for 1858, the yearly acceleration being less than 0·000007.

A comparison of the lines of equal dip, declination, intensity, and horizontal force, deduced from the observations taken in the east and west of France, leads to the following conclusions.

The isoclinals are found to be rather further apart and more inclined to the geographic meridian, but receding less quickly from it, in the east than in the west; the isogonics are more distant from each other, but make a less angle with the meridian, in the east than in the west; but the results obtained from the declination observations of 1869 are less trustworthy than those for 1868, on account of the unsteadiness of the new theodolite.

The distance between the isodynamics does not sensibly vary for east

and west, but the angle for the east is less; whilst for the horizontal-force lines both the angle and the distance are somewhat in excess in the east, though the difference is diminishing.

VI. "On the Behaviour of Supersaturated Saline Solutions when exposed to the Open Air." By Charles Tomlinson, F.R.S. Received July 26, 1871.

It is a remarkable proof of the difference between the air of a room and that of a field or garden in the country, that supersaturated saline solutions, which crystallize the moment they are uncovered in a room, may be kept uncovered in an open space during many hours without crystallizing.

During the last three years I have made many experiments to confirm this conclusion in the little garden at the back of my house at Highgate. I have no doubt that in a more open space further in the country the results would have been more perfect; but still I venture to think they are sufficiently striking to merit a place in the 'Proceedings.' The following were conducted during the spring of the present year.

A solution of two parts of sodic sulphate and one part of water was boiled and filtered into 3- and 4-ounce flasks, of which the opening of the short cylindrical neck is just three-quarters of an inch. The filtered solutions were reboiled and the flasks covered with watch-glasses. One flask was placed on a stool in the middle of a gravel-walk and the watch-glass removed. I now proceed to quote from my note-book some results in the order of time, and then to summarize them.

1871, March 17. Put out flask containing cold solution at 12.30 P.M.; temp. 56° Fahr.; clouds, and afterwards sun, 60° and upwards. At 2.50, therm. 50°. At 4.30 found the solution solid, and upon it a speck of soot which had evidently acted as a nucleus.

At 5.30 put out flask; temp. 43°, with slight wind; 6.30, 41°; 7.30, 42°, with a good deposit of the seven-atom salt. At 9, temp. 40°, and deposit much increased. At 11, solution solid, and the seven-atom deposit chalky white.

March 18. 2.15, put out flask and uncovered it under clear sky. At 4, temp. 57°, fine crop of crystals, evidently due to evaporation, which was so powerful that a round patch immediately below the opening of the neck was white and pulverulent from the formation of anhydrous salt. In this case the crystallization took place, as in the case of a saturated solution, in an open dish, only much more quickly, on account of the much larger quantity of salt in the supersaturated solution. There was no formation of the seven-watered salt *.

* With reference to some of the cases of crystallization given in my paper "On Supersaturated Saline Solutions, Part II." (Phil. Trans. 1871, p. 51), Professor Stokes

The same flask was reboiled and left to cool during an hour. It was put out at 5.15; temp. 55°, with clouds; at 6, temp. 47½°; at 6.35, temp. 45°. At 7.15 the seven-watered salt was forming and heat-currents ascending; cloudy. At 9, the seven-atom salt was much increased in quantity; temp. 45°. At 9.45, the solution solid, the nuclear action directly under the mouth of the flask.

March 19. Added a small quantity of water to supply waste from evaporation and reboiled the same flask. When cold, put the flask out at 5 P.M. At 6.30 brought it back into room, and it crystallized within two or three minutes.

Reboiled this flask and put it uncovered in the garden to cool at 6.35. At 8.45 foggy, and temp. 43°. Shook the solution; it was very viscous, but did not crystallize. At 9.30 abundant deposit of moisture: at 10.30 thick mist, temp. 41½°; no crystallization.

March 20. At 7 A.M. found a considerable deposit of the seven-watered salt, much of it finely crystallized; flask and stool very wet; temp. 40°. Took the flask indoors, and after a few minutes the solution crystallized and the seven-atom salt became opaque. There was a speck of soot on the solution, but it lay so loosely that it had evidently not disturbed the surface-tension of the solution.

Three flasks were put out in the garden and uncovered. One crystallized from the action of a speck; a second during the day deposited some seven-watered salt and then crystallized by evaporation, producing some very fine crystals of the ten-watered salt with dihedral summits. These crystals, although so fine, were produced rapidly (as under the action of films), as was evident from phenomena which may often be seen in the sudden crystallization of supersaturated solutions, namely, the heat liberated in the passage of the liquid to the solid state volatilizes a portion of the water of the solution, and the vapour rising upwards towards the neck condenses on the inner surface of the flask; but the ring of glass just above the solidified solution is rendered too warm to condense the vapour, so that the flask presents the curious appearance of salt at the bottom in a chalky white deposit covered by a layer of crystals or by an opalescent layer; then comes a zone of clear glass surmounted by a wide deposit of condensed vapour. The heat thus given out during the solidification of supersaturated saline solutions may vary from 10° or 20° to 100° F.

The third flask put out under this date crystallized some hours later by evaporation.

April 25. Flask out all night. This morning found it covered with dew, some of which trickled down into the solution without acting as a nucleus.

April 27. Two flasks put out, and after a few hours rain came on and

was kind enough to suggest that the large crystals might be due to evaporation. Such was certainly the case in some of these garden experiments.

both the solutions crystallized. Heated the flasks over a spirit-lamp, and passed the solutions through a filter, when in each case a minute black speck, which had acted as a nucleus, was left on the filter.

Two flasks that had been standing some time on the window-ledge, each containing a deposit of the seven-watered salt, were put out during the heavy rain of a thunder-storm and uncovered. The drops repeatedly entered the flasks, splashing up the solution; but they did not act as nuclei. The flasks were thus exposed during six hours, when one of them was brought in and placed on the table; it immediately became solid.

After the storm, and while the sun was shining, snipped off some pieces from some young leaves of the currant- and gooseberry-bush; they did not act as nuclei, even when shaken up in the solution. The upper surfaces of the leaves had been washed in the rain, but the under surfaces were dry. The scissors used were washed in spirit, and several cuttings made in order still further to clean the blades, before any of the pieces were allowed to fall into the flask.

The conclusions which I think myself justified in drawing from these and similar experiments are the following:—

1. That a highly supersaturated solution of sodic sulphate may be exposed to the open air of the country in an uncovered flask and in cloudy weather for from twelve to twenty hours without any formation of the ordinary ten-watered crystals.

2. That if the temperature fall to about 40° Fahr. and under, the modified seven-watered salt is formed at the bottom of the solution just as in covered vessels.

3. That if the exposed solution suddenly crystallize into a closely packed mass of needles, a nucleus may always be found in the form of an insect, a speck of soot, a black point of carbon, &c.

4. That if during the exposure rain comes on, the solution generally crystallizes suddenly, in consequence of an active nucleus being brought down; but if the flask be put out during heavy rain, when we may suppose all the solid nuclei to have been brought down, the rain-drops, now quite clean, fall into the solution without any nuclear action.

5. That the young and newly sprouted leaves of trees, such as those of gooseberry- and currant-bushes, have no nuclear action.

6. That in clear cloudless weather, when the force of evaporation is strong, the solutions, after some hours' exposure, produce fine groups of crystals of the ten-atom salt, just as a saturated solution would do if left to evaporate slowly in an open dish.

7. That if the solution, after being exposed to the open air, be brought into a room, it crystallizes immediately under the action of aërial nuclei.

Supersaturated solutions of Epsom salts and of alum were similarly exposed, and the results were in harmony with the above conclusions. The following are a few cases.

March 20, 1871. Magnesic sulphate four parts, water two and a half parts; boiled and filtered into three small flasks, and at 7.10 P.M. exposed two of them in the garden; placed the third (covered) in the room on the window-ledge in contact with the glass pane. At 8.10, temp. 40°, no change: 9.15, temp. 40°, with much dew; one solution solid, the other unchanged: 10, temp. 39°, the flask very wet with dew; no change. 21st, 8 A.M., temp. 35°, wet fog, no change; brought the flask indoors and uncovered the one in window; they became solid in less than half an hour. Two of the flasks were reboiled and united, so that the globe of the flask was quite filled. Put this into the garden at 4 P.M., temp. 55°. At 6, temp. 43½°; at 8, temp. 39½°; at 10.30, temp. 35°, with clear sky. 22nd, at 8 A.M., temp. 31°, no change; at 10 the contents of flask were found in a solid state with a speck of soot on the surface.

Potash-alum three parts, water two parts; boiled and filtered into three flasks. When cold, put one flask in the garden and took off watch-glass. Time 5.30, temp. 46°. At 6, temp. 43½°, and the sky very clear. At 6.35, no change; temp. 40½°; removed the watch-glass from the flask indoors, and the solution crystallized immediately. At 8 the solution in the garden crystallized, apparently from evaporation; temp. 39½°.

The results obtained by exposure to the open air of highly supersaturated solutions of sodic acetate are not in harmony with the above, on account of the tendency of such solutions, when left to repose, to arrange themselves into layers of different density; so that while the lower part of the solution becomes more supersaturated, the upper part becomes less so. By keeping such a solution during some weeks in a long test-tube plugged with cotton-wool, the upper part of the solution is scarcely more than saturated, so that it may be touched with an unclean wire without crystallizing, and the wire may be passed about one-third down the tube before the solution becomes sensitive to the action of a solid nucleus. But when once the crystallization is started, the whole contents of the tube become solid, and the tube may be inverted without the escape of any liquid.

For these garden experiments a solution of four parts sodic acetate to one part of water was boiled and filtered into two small flasks, which had been previously washed with spirits of wine, rinsed with water, and then with a little acetic acid.

March 18. At 8 P.M. the flasks were exposed in the garden; at 11.15 the temp. was 35°. On the 19th, at 8.30 A.M., after being left out all night, both the solutions were liquid, but very viscous. At 10.50 the flasks were brought into a room and placed on the mantle-shelf, where they remained all that day as well as the next. On the 21st they were still liquid; one of them was touched with the end of a wooden penholder, and it at once became solid; the other flask was left uncovered on the shelf until the 24th before it became solid. On the 21st the solution that was touched with the nucleus was reboiled with the addition of half an ounce

of water and filtered into two flasks. When cold, one of these was put out at 8.35. At 10.30 the temperature was 35°, and next morning at 8 A.M. 31°. The flask was now brought in and left on the mantle-shelf, where it remained some days exposed to dust, and it crystallized from loss of water by evaporation.

P.S. August 17, 1871.—I may mention that the suggestion made above, that in an open space far from houses the results would be still more perfect, has been realized by exposing in such a space supersaturated solutions of sodic sulphate and of alum in small shallow vessels, quite full, to the action of a strong wind under a cloudy sky, and they did not crystallize during half an hour's exposure; but the moment they were touched with the finger they became solid. Similar solutions were exposed in small open beakers in an open space during many hours, and at a temperature of about 40° F., without crystallizing.

VII. "Note on the Spectrum of Encke's Comet." By William Huggins, D.C.L., LL.D., V.P.R.S. Received November 16, 1871.

I give the following observations of Encke's comet, and of the spectrum of its light, in the order of the dates of the evenings on which they were made.

Oct 17. The comet presented the appearance of a nearly circular faint nebulosity, in which no condensation could be certainly distinguished.

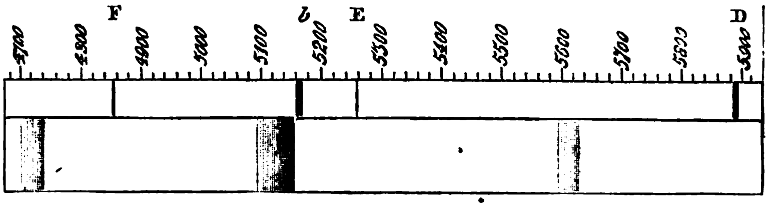
Nov. 7. By this time an important change had taken place in the appearance of the comet. There was now a strong condensation of light towards the east. The more condensed part of the comet, which was fan-shaped, was bounded on the eastern side by a tolerably defined contour, which approached in form to a parabolic curve. Surrounding this brighter portion of the comet was a much fainter nebulosity, of which the boundary on the eastern side appeared to form a line at right angles to the axis of the comet.

I suspected a very minute stellar nucleus just within the eastern extremity of the brighter condensed part, and to a small extent north of the comet's axis.

Nov. 8. The description given yesterday is applicable to the comet tonight. The brighter part appears more defined and in stronger contrast to the fainter outlying nebulosity. The nucleus is now visible with certainty. On the western preceding side of the comet, the side towards the sun, the cometary light becomes gradually fainter and more extended until it is lost to view.

On this evening the light of the comet was examined by the spectroscope. The larger part of the light was resolved by the prism into a bright band in the green part of the spectrum. The band was defined at

its less refrangible limit, and gradually faded towards the blue. The micrometer gave 5160 millionths of a millimetre as the wave-length of the less refrangible boundary of the band. Two other bright bands were occasionally suspected; one of them appeared to be about two thirds of the distance from the bright band towards D, the other a little distance beyond F. No continuous spectrum could be detected. The nucleus was



probably much too minute and faint to give a continuous spectrum that could be seen.

No difference in the spectrum was seen when the slit was moved over the comet in different directions, as far as its feeble light permitted.

The spectrum of a hydrocarbon, giving the bands which appear to be due to carbon, was then reflected into the instrument, and observed simultaneously with that of the comet. The band in the green was found to be identical in position with the brightest of the bands of carbon, and to be similar in gradation of brightness from its less refrangible limit.

Nov. 9. The observations of yesterday were confirmed. The second more refrangible band, which was then caught only by glimpses, was found to be coincident with the third band in the carbon spectrum. The wave-length of the less refrangible limit was about 4735^{mm}. The least refrangible of the three cometary bands could be seen only occasionally.

Nov. 12. The observations on this evening contain no new facts.

Nov. 13. To-night the nucleus appears as a minute, well-defined stellar point.

In the spectroscope the three bands are distinctly seen. The position in the spectrum of the least refrangible band corresponds with the first band of the carbon spectrum; it commences from the red, with a wave-length of about 5632^{mm}.

Attempts were made with a double-image prism, a Nicol's prism, and a Nicol's prism combined with a Savart's system of plates to detect polarized light in the comet, but without success.

Nov. 14. The form of the comet remains nearly the same. The outlying nebulosity is now chiefly on the south of the axis of the comet. The nucleus appears to be precisely at the extreme eastern limit of the brighter, more condensed part of the comet.

The same spectrum was seen, but fog coming on interrupted the observations.

On this evening an attempt was made again to detect polarized light. A

double-image prism was placed between the eyepiece and the eye. The prism was brought into four different positions 90° apart. At each position of the prism an attempt was made to estimate the relative brightness of the two images. The power of the prism was just sufficient to give two images of the comet without their overlapping. The difference in brightness of the images was exceedingly small; I could not be certain that any appreciable difference really existed. However, I attempted in each case to select one of the two images as the brighter one. Afterwards I determined the position of the prism at the four different estimations, and I then found that three of the estimations were in accordance with a portion of the comet's light being polarized in a plane passing through the sun, and one in opposition to that supposition. I hesitate to attach any positive value to these observations; but they may perhaps be taken as showing that no considerable part of the comet's light is polarized.

The foregoing observations appear to show that the spectrum of this comet is identical with that of Comet II. 1868, a description of which I had the honour to present to the Royal Society*.

It is worthy of notice that the cometary matter appears drawn out and diffused towards the sun, and that it has not yet come under the influence of the force, or been subjected to the conditions, whatever they may be, by which in most cases cometary matter appears to be powerfully repelled from the sun.

The observations were made with the telescope belonging to the Royal Society, of 15 inches aperture. The spectroscope contained one prism with a refracting angle of 60, and the small observing telescope magnified six times.

November 30, 1871.

ANNIVERSARY MEETING.

General Sir EDWARD SABINE, K.C.B., President, in the Chair.

Dr. Blakiston, for the Auditors of the Treasurer's Accounts on the part of the Society, reported that the total receipts during the past year, including a balance of £127 9*s*. 3*d*. carried from the preceding year, and £706 17*s*. 2*d*. balance of the Oliveira bequest, amount to £5095 15*s*. 7*d*.; and that the total expenditure in the same period, including £518 2*s*. 10*d*. from the Society's funds to complete the payment for the Equatorial Telescope, amounts to £5169 13*s*. 2*d*., leaving a balance of £28 2*s*. 2*d*. in the hands of the Treasurer, and of £101 19*s*. 9*d*. due to the Bankers.

The thanks of the Society were voted to the Treasurer and Auditors.

* Phil. Trans. 1868, p. 555 and plate xxxiii.

The Secretary read the following Lists:—

Fellows deceased since the last Anniversary.

On the Home List.

Sir Thomas Dyke Acland, Bart., M.A., D.C.L.
Charles Babbage, M.A.
Field-Marshal Sir John Fox Burgoyne, Bart., G.C.B., D.C.L.
Colonel Sir Proby T. Cautley, K.C.B.
Major-General Sir William Thomas Denison, R.E., K.C.B.
Edwin Richard Wyndham-Quin, Earl of Dunraven and Mount-Earl.
Commander Matthew Curling Friend, R.N.
George Grote, LL.D.
Thomas Henry Hall, M.A.
The Rev. William Vernon Harcourt, M.A.
Philip Hardwick, R.A.
Sir John Frederick William Herschel, Bart., K.H., D.C.L.
Thomas Mayo, M.D.
Sir Roderick Impey Murchison, Bart., K.C.B.
The Rev. Joseph Bancroft Reade, M.A.
Henry Hyde Salter, M.D.
Samuel Solly, F.R.C.S.
James Yates, M.A.

On the Foreign List.

Wilhelm Karl Haidinger.

Change of Title.

Earl De Grey and Ripon *to* Marquis of Ripon.

Fellows elected since the last Anniversary.

William Henry Besant, M.A.
William Budd, M.D.
George William Callender, F.R.C.S.
William Carruthers, F.L.S.
Robert Etheridge, F.R.S.E.
Frederick Guthrie, B.A.
John Herschel, Capt. R.E.
Right Hon. Robert Lowe, LL.D., Chancellor of the Exchequer.
Alexander Moncrieff, Capt. M.A.
Richard Quain, M.D.
Carl Schorlemmer.
Edward Thomas, Treas. R.A.S.
Edward Burnet Tylor.
Cromwell Fleetwood Varley, C.E.
Arthur, Viscount Walden, P.Z.S.
John Wood, F.R.C.S.

His Majesty Pedro II., Emperor of Brazil.

The President then addressed the Society as follows :—

GENTLEMEN,

BY the publication of Volume V. of the Catalogue of Scientific Papers, the alphabetical list of titles according to authors' names is brought down to the letters T I Z. The remainder of the series, with the supplementary matter, will fill one more volume, thus making six volumes for the completion of this part of the work. The preparation of Volume VI. is in a forward state, some sheets are printed, and its publication may be confidently looked for before our next Anniversary.

We have to regret that, in consequence of Dr. Carus having been for some time incapacitated by illness, but little progress has been made this year with the 'Index Rerum.'

We have lost this year three distinguished Fellows of our Society,—Sir John Herschel, Mr. Babbage, and Sir Roderick Murchison. Of the first-named, and most illustrious, Sir John Herschel, you might well expect from me a much more than ordinary notice; but, happily, such a notice (at once far more worthy of the occasion than any thing which I could have been competent to furnish, and more full than could have fallen within the limits of an Anniversary Address) will be in your hands within a few days in our Obituary Notices. I would only add my own most warm and cordial assent to every part thereof which falls within my capability of judgment. The deaths of Mr. Babbage and of Sir Roderick Murchison are more recent; but we may be sure that the care of our excellent senior Secretary will, at the proper time, supply suitable obituary notices of their scientific achievements,—perhaps, in the case of Mr. Babbage, obtaining such a notice from some eminently qualified member of the great University to which he belonged. All these three gentlemen were of advanced age, and the health of all three had been failing for a more or less considerable interval; yet it is comparatively but a short time since the last departed (Sir Roderick Murchison) still displayed such an energetic and vigorous activity as will make his loss severely felt, most especially by the Royal Geographical Society, from whom and from the Geological Society large and worthy notices may doubtless be expected in their respective obituaries; while our own obituary may contain such a brief but valuable statement of the wider bearings of his geological labours as may still come appropriately from the Royal Society. It might be an indulgence to myself, but perhaps scarcely appropriate from this Chair, if, in speaking of Sir Roderick Murchison, I were to permit myself to advert to our long, joint, and friendly labours together at the British Association for the Advancement of Science.

By the munificence of one of our Fellows, Mr. John Peter Gassiot, the Kew Observatory has taken its place amongst the permanent in-

stitutions of our country. Mr. Gassiot has transferred to the Royal Society, in Trust, certain securities, producing a net income of £500 per annum, towards the cost of carrying on and continuing magnetical and meteorological observations with self-recording instruments, and any other physical investigations that may from time to time be found practicable and desirable, in the present building at Kew belonging to Her Majesty's Government; or in the event of the Government at any time declining to continue to place that building at the disposition of the Royal Society, then in any other suitable building which the Council of the Royal Society may determine. It is further provided that, "in the event of the Royal Society at any future time declining to continue such an observatory, either, as at present, at Kew or elsewhere, the securities producing the income shall be transferred to another corporation for educational purposes." The cost of the *meteorological* observations by self-recording instruments at Kew is at present defrayed from public funds placed at the disposal of a Committee nominated, at the request of Her Majesty's Government, by our President and Council, and consisting of Fellows of the Society serving gratuitously. Carrying out Mr. Gassiot's views, the Council of the Royal Society has formed the same individuals into a "Kew Committee," also serving gratuitously, and having the proceeds of the Gassiot Fund at their disposal, and applying them to the maintenance of *magnetical* observations by self-recording instruments *primarily*; and *secondarily*, as far as may be practicable, to aiding any other suitable physical investigations for which it may be possible to find space and adequate supervision.

Among the first duties which have required the attention of the Kew Committee has been the agreeable one of responding to an application made to them by Dr. Jelinek, Director of the Central-Anstalt für Meteorologie und Erdmagnetismus, to procure for that establishment a set of self-recording magnetographs similar to those at Kew. The request has been of course complied with; and it is hoped that the apparatus will be ready for transmission to Vienna in March next, being the time named by Dr. Jelinek as that at which the new building in course of erection in that city is expected to be completed. The Committee has also been apprised by a letter (dated in June last) from Mr. Stone, Astronomer Royal at the Cape of Good Hope, that he had at that date applied to the Admiralty (being the Department of Her Majesty's Government under which the Cape Observatory is placed) for a set of magnetographs, similar to those at Kew, to be employed at the Cape. The Kew Committee hold themselves in readiness to supply the desired apparatus when they may receive directions to that effect from the Admiralty; such directions, however, have not yet been received. If Mr. Stone's request is granted, the Cape Observatory will be the third in the British Colonial Dominions employing such instruments, the other two being the Colaba Observatory, under Mr. Chambers, at Bombay (for which our thanks are due to Sir Stafford North-

cote, when Secretary of State for India), and the Mauritius Observatory, under Mr. Meldrum (that establishment itself, as well as its magnetographs, being mainly due to the action of Sir Henry Barkly, F.R.S., when Governor of that colony).

The care of our Foreign Secretary in watching scientific proceedings in other countries has recently called our attention to the investigation of a new feature in the cosmical connexions of terrestrial magnetism, in a paper by Professor Hornstein, of Prague, entitled, "On the dependence of the Earth's Magnetism on the Rotation of the Sun"*. From an examination of the magnetic records at Prague and Vienna, Professor Hornstein infers the existence of a periodic magnetic disturbance in very close accord with the synodic period of the rotation of the sun-spots in the sun's equator.

As the existence of the Great Melbourne Telescope is in no small degree owing to the exertions of Members of this Society, and to their influence, you will be glad to hear that it is now in regular and successful work, after several difficulties and misadventures, such as often occur in the early trials of uncommon instruments or new sorts of observations. Some of these will be found in the volume of correspondence which your Council has thought it desirable to publish, both as containing a complete history of this great work from its first conception to its complete execution, and also as presenting a collection of the opinions of persons whose knowledge of large telescopes must make these opinions specially valuable to any who may hereafter be engaged in similar undertakings. In particular one matter, about which considerable apprehension was entertained, the repolishing the specula at Melbourne, has been well performed. The speculum, described as A in the correspondence, which had become tarnished (under circumstances described there), has been repolished, and Mr. Ellery reports that "its performance is highly satisfactory." In a subsequent part of his last Report, he thus summarizes his estimate of the instrument:—"Further experience with the Great Telescope, and of "the conditions which affect its performance, have very much enhanced "our opinion of it; and the drawings and other results obtained unmis-"takably show that excellent work can be done with it. On really "favourable occasions the performance, even with very high powers, is "exceedingly good. It is evident, however, that a telescope of such "large dimensions requires a very long practice before it can be fairly or "successfully used. The mechanical arrangements are all in perfect order; "and the moving and setting the telescope from one object to another is "performed almost as quickly and easily as with a five-feet equatorial. "The clock has worked well and with great regularity from the first." This is in full accordance with the Reports of the Telescope Committee and of Mr. Lassell: the chief remaining difficulty is how the results

* *Vide suprà*, p. 31.

may best be published, especially the drawings of nebulæ; but there is no doubt that this will soon be overcome.

In my Address of 1869 I had the pleasure of announcing to the Society the advanced state of the fine equatorially-mounted telescope, fitted more especially for spectroscopic research (therefore rather for celestial physics than for more strictly astronomical work), which, while it was to remain the property of the Royal Society, was intended by the President and Council to be placed in the able and zealous hands of Mr. Huggins, who was preparing at his own expense a suitable building for its reception. The necessary spectroscopic apparatus for use with it has not been long completed; but some of our Fellows are aware that Mr. Huggins has been already doing work with this fine instrument in his observations of the spectrum of Uranus, and in his quite recent examination of the spectrum of Encke's Comet, which he finds to agree with that of carbon, and to be apparently identical with that of Comet II. 1868, of which a description appears in the Phil. Trans. for 1868, p. 555. His other observations have been hitherto mainly confirmatory of those he had previously obtained of the spectra of the nebulæ and stars; but he hopes for more definite results regarding the approach or recession of stars, and in particular the rate of the recession of Sirius.

In my last Address I spoke with pleasure of the approaching completion of the fine series of Pendulum Experiments extending throughout the continent of India, from Cape Comorin to the high tablelands of the Himalayas (to which it was further proposed to add two stations on the homeward route, *i. e.* Aden, and a station near the "Bitter Lakes"). Of this magnificent series, designed to comprise thirty Indian stations, twenty-five Indian stations had then been completed by the skilled and indefatigable exertions of Captain Basevi of the Royal Engineers. Only five more remained, one on the Indus, and four in the higher Himalayas; but, alas! while engaged on these, Captain Basevi's health yielded to the combined effects of arduous exertion, of climate, and finally of mountain exposure. His last completed station (to the south of Leh, in lat. 33°) was at an altitude of 15,500 feet. He then proceeded to one still higher, closely approaching 17,000 feet, which was to be the last of all, and there died. His character, services, and the sacrifice of his valuable life have received a fitting tribute from his immediate chief, Colonel Walker, head of the Trigonometrical Survey of India. We may securely anticipate that, under the superintending care of Colonel Walker, not only will the programme of operations in India be carried out with entire satisfaction, but also that effectual provision will be made for that repetition of the determinations in England which is essential to the completion and full assurance of the scientific results of Captain Basevi's labours in this great undertaking.

The existence of a Lunar Atmospheric Tide, as indicated by differences of barometric pressure corresponding systematically to differences of the moon's position relatively to the meridian, has received a further confirmation by the discussion by Mr. Bergsma, of the Royal Netherlands Observatory at Batavia, of a series of hourly observations of the barometric pressure at that Observatory, extending (Sundays excepted) from January 1866 to January 1869, made specially with a view to this investigation. The discussion is on the same plan (slightly modified and thereby improved) as that by myself of the observations at St. Helena, from October 1843 to September 1845 inclusive, in the Phil. Trans. for 1847; and that by Captain Elliot of the observations at Singapore, from 1841 to 1845, in the Phil. Trans. for 1852. The results, as regards the existence of the periodic tide, are substantially the same in the three cases, but may probably be more approximatively exact in value in the more recent investigation. From the observations of the three years at Batavia, Mr. Bergsma finds that the Lunar Tide has at that station two maxima and two minima. The two highest barometric pressures are those for the lunar hours 1 and 13, being the hours following the two passages of the moon through the meridian. The two lowest pressures are those for the lunar hours 7 and 19, being those following the two passages of the moon through the horizon. The means found for each of the years 1866, 1867, and 1868 show nearly the same features as the means for the three years. The difference between the mean of the two maxima and the mean of the two minima is 0·107 millim. at Batavia: this difference was found for St. Helena 0·094 millim., and for Singapore 0·145 millim.

The Fellows will remember that in my two last Addresses (for 1869 and 1870) I referred to the valuable memoirs of Professor Heer, of Zurich, on the fossil plants brought of late, at various times, from the Arctic regions. Professor Nordenskiöld revisited the shores of West Greenland in 1870; and on learning that he had given the principal geological facts and fossils collected in that expedition to Professor Heer, I lost no time in communicating with the latter gentleman, and received from him (through Mr. Robert Scott, F.R.S., Director of the Meteorological Office) a reply, which appeared to me so interesting that I placed it in the hands of Sir Charles Lyell, asking him, if he would be so kind, to furnish me with a notice sufficiently brief to be included in this Address. He has done so, and I subjoin it in his own words:—" The first voyage of Parry in 1819–20 made " the naturalist acquainted with the marvellous fact that plants similar to " those of our ancient coal once flourished in Melville Island, lat. 75° N.; " and now we learn more exactly, from the fruits of Nordenskiöld's late " Expedition, that the arctic plants of that early Palæozoic period are not " only traceable over a wide area, but that not a few of them are identical " with European species; and it is remarkable that the specimens which " have been brought from these high latitudes are equal in size and show

"as vigorous a growth as those found fossil in Europe. Plants that are "comparatively modern, of Miocene date, discovered by various explorers "in Greenland and Spitzbergen, had already testified to the existence of a "mild climate in Tertiary times, such as formerly prevailed in Central "Europe, and which, according to Heer, had probably reached to the pole "itself. Now we learn, from the same eminent botanist, that the fossils "brought in 1870 from Atarne and Atarnekerdluk, in Greenland, by Nor-"denskiöld and his companions, throw light on the flora of a geological "period, the Cretaceous, intermediate between the Carboniferous and Mio-"cene; for among these newly found fossils are plants referable both "to the Lower and Upper Cretaceous formation, which prove, says Pro-"fessor Heer, that in these high latitudes, as in Central Europe, the "Lower Cretaceous flora consisted principally of Ferns, Conifers, and "Cycads; while in the Upper Cretaceous, Dicotyledones appear; and both "these great divisions of the Upper Mesozoic flora show a warmer climate "than the Miocene."

It is satisfactory to learn that there is a probability that these researches will be resumed in 1872 by a second North-German Expedition to East Greenland.

The Scientific Institutions and establishments of our country have been honoured this summer by long and careful visits from His Majesty Pedro II., Emperor of Brazil, whose remarkable competence to appreciate such Institutions may be said to be universally recognized. At a recent Meeting of the Royal Society, we have had the pleasure of enrolling His Majesty's name in the List of our Fellows. We have only to regret the Emperor's unavoidable absence from our Meetings; unavoidable because the Society having been in recess during his visit to England, the election could not have taken place until the new Session, when he had quitted our shores. It was pleasing to hear on this occasion, from Fellows distinguished in different branches of science, the warm and competent testimony borne to his Majesty's acquirements and to his rare scientific intelligence.

As your President, I have been called upon, by the Royal Commission on Scientific Instruction and the Advancement of Science, to give evidence regarding the assistance which, in the course of the last fifty years, has been rendered from time to time by Her Majesty's Government in the advancement of science, on the recommendation of the Scientific Institutions of the country, and especially on the recommendation of the Royal Society. The evidence thus given has been printed by the direction of the Commission, and is procurable in the usual manner.

I proceed to the award of the Medals.

The Copley Medal has been awarded to Dr. Julius Robert Mayer, of Heilbronn, for his researches on the Mechanics of Heat, including essays on:— 1. The Forces of Inorganic Nature; 2. Organic Motion in connexion with

Nutrition; 3. Fever; 4. Celestial Dynamics; 5. The Mechanical Equivalent of Heat.

I may presume that the last named, viz. the Mechanical Equivalent of Heat, though not the sole, is yet the principal ground on which the award of the Copley Medal was made; and, so far as this may have been the case, the award may perhaps be considered to require some explanation on the part of the President and Council, inasmuch as this is the second of two Copley Medals awarded (and, I believe, in each case rightly awarded) for what may, perhaps, be mainly regarded as one and the same discovery,—the later (which is the present award to Dr. Mayer) being for investigations earlier in date than those of our countryman Mr. Joule, to whom the first Medal of the two was awarded. This seeming inconsistency can, I believe, be fully justified. Seeing also the scientific interest of the history of the double investigation, I have thought it desirable to obtain a short but comprehensive notice on the subject from the able hands of our junior Secretary, Professor Stokes; it is as follows:—

"In a paper published in 1842, Mayer showed that he clearly conceived "the convertibility of falling force, or of the *vis viva*, which is its equiva- "lent or representative in visible motion, into heat, which again can dis- "appear as heat by reconversion into work or *vis viva*, as the case may be. "He pointed out the mechanical equivalent of heat as a fundamental "datum, like the space through which a body falls in one second, to be "obtained from experiment. He went further. When air is condensed "by the application of pressure, heat, as is well known, is produced. "Taking the heat so produced as the equivalent of the work done in com- "pressing the air, Mayer obtained a numerical value of the mechanical "equivalent of heat which, when corrected by employing a more precise "value of the specific heat of air than that accessible to Mayer, does not "much differ from Joule's result. This was undoubtedly a bold idea, "and the numerical value obtained by Mayer's method is, as we now "know, very nearly correct. Nevertheless it must be observed that an "essential condition in a trustworthy determination is wanting in Mayer's "method; *the portion of matter operated on does not go through a cycle of* "*changes*. Mayer reasons as if the production of heat were the sole effect "of the work done in compressing air. But the volume of the air is "changed at the same time, and it is quite impossible to say *à priori* "whether this change may not involve what is analogous to the statical "compression of a spring, in which a portion, or even a large portion of the "work done in compression may have been expended. In that case the "numerical result given by Mayer's method would have been erroneous, "and *might* have been even widely erroneous. Hence the practical cor- "rectness of the equivalent obtained by Mayer's method must not lead us "to shut our eyes to the merit of our own countryman Joule, in being the "first to determine the mechanical equivalent of heat by methods which "are unexceptionable, as fulfilling the essential condition that no ultimate "change of state is produced in the matter operated upon."

Professor Miller,

As Dr. Mayer's health and the inclemency of the weather, combined with the length of the journey, deprive us of the pleasure of his presence at our Meeting, I will request you to transmit this Medal to him, and with it the assurance of our cordial respect and regard.

The Council has awarded a Royal Medal to Dr. John Stenhouse, F.R.S., for his researches on the Lichens and their proximate constituents and derivatives, including erythrite, and for his researches on the action of charcoal in purifying air.

Dr. Stenhouse,

I have great pleasure in delivering to you this Medal, awarded to you by the Royal Society for the long, laborious, and valuable researches with which from time to time you have enriched our Transactions.

The Council has awarded a Royal Medal to Mr. George Busk, F.R.S., for his researches in Zoology, Physiology, and Comparative Anatomy.

Mr. Busk,

I present you with this Medal in testimony of the appreciation by the Royal Society of the results of your researches in Zoology, Physiology, and Comparative Anatomy.

In conclusion, I desire to avail myself of this occasion to express my warmest acknowledgments to the Fellows of the Society for the kind consideration which I have received from them at all times and in all circumstances, and, in particular, for their attendance at, and thereby their support of, the President's soirées, in which I have attempted to follow in the footsteps of my predecessors. Since the date of Sir Joseph Banks's Presidency, at the close of the last century, it has been regarded as the privilege of the President to receive the Fellows, either at his own house or at that of the Society, together with others whom he might think it would be agreeable to them to meet. There has thus been afforded to persons engaged in mechanical or other inventions, auxiliary to science or otherwise connected therewith, a convenient opportunity for the exhibition and discussion of their various apparatus. The soirées have also been described, and I believe justly so, as affording suitable opportunities for the interchange of kindly feeling and good companionship between the President and the Fellows. I may venture to say that I have found them thoroughly so in the ten years during which I have had the honour of the Presidency; and I beg to express my grateful acknowledgments accordingly to the Fellows, collectively and individually.

On the motion of Mr. De La Rue, seconded by Colonel Walker, and supported by the Astronomer Royal, it was resolved,—"That the thanks of the Society be returned to the President for his Address, and for the admirable and dignified manner in which he has presided over the Society; and that he be requested to allow his Address to be printed."

The Statutes relating to the election of the Council and Officers having been read, and Mr. Cæsar Hawkins and Admiral Ommaney having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were collected, and the following were declared duly elected as Council and Officers for the ensuing year:—

President.—George Biddell Airy, C.B., M.A., D.C.L., LL.D., Astronomer Royal.

Treasurer.—William Spottiswoode, M.A.

Secretaries.— { William Sharpey, M.D., LL.D.
George Gabriel Stokes, M.A., D.C.L., LL.D.

Foreign Secretary.—Professor William Hallowes Miller, M.A., LL.D.

Other Members of the Council.—George James Allman, M.D.; John Ball, M.A.; George Burrows, M.D.; Mr. George Busk, P.R.C.S.; Professor Robert Bellamy Clifton, M.A.; Heinrich Debus, Ph.D.; Professor Peter Martin Duncan, M.B.; Professor George Carey Foster, B.A.; Mr. Francis Galton; Thomas Archer Hirst, Ph.D.; Sir John Lubbock, Bart.; Sir James Paget, Bart., D.C.L.; The Earl of Rosse, D.C.L.; General Sir E. Sabine, R.A., K.C.B.; Isaac Todhunter, M.A.; Sir Charles Wheatstone, D.C.L.

The thanks of the Society were voted to the Scrutators.

The following Table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Royal.	Foreign.	Compounders.	£4 yearly.	Total.
November 30, 1870.	3	50	281	263	597
Since elected	+1		+7	+9	+17
Since compounded..			+1	−1	
Since deceased		−1	−11	−7	−19
November 30, 1871.	4	49	278	264	595

Receipts and Payments of the Royal Society between December 1, 1870, *and November* 30, 1871.

Receipts	£	s.	d.
Balance at Bank and on hand	127	9	3
Annual Subscriptions, Admission Fees, and Compositions	1706	0	0
Rents	257	4	4
Divideds	1476	3	4
Ditto, Trust Funds	314	2	2
Oliveira Bequest (balance of Deposit)	706	17	2
„ „ (Interest)	14	14	3
Sale of Transactions, Proceedings, &c.	490	7	4
Petty Repayments	2	17	9
	5095	15	7
Balance due to Bankers	101	19	9
	£5197	15	4

Payments	£	s.	d.	£	s.	d.
Salaries, Wages, and Pension				1057	5	4
The Scientific Catalogue				266	19	0
Equatoreal Telescope and Spectroscope				1302	14	5
Books for the Library and Binding				211	11	3
Printing Transactions, Proceedings, and Catalogue, Paper, Binding, Engraving, and Lithography				1634	19	3
General Expenses (as per Table subjoined)				349	15	4
Donation Fund	160	0	0			
Rumford Fund	136	9	4			
Wintringham Fund	35	6	6			
Copley Medal Fund	4	15	2	346	8	7
C. W. Siemens, Bakerian Lecture	4	0	0			
Rev. T. S. Evans, Fairchild Lecture	2	18	10			
Croonian Lecture, Poor of St. James' Parish	2	18	9			
				5169	13	2
Balance of Catalogue Account				25	13	9
„ Petty Cash Account				2	8	5
				£5197	15	4

W. SPOTTISWOODE,
Treasurer.

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.), £136 per annum.
Estate at Acton, Middlesex (34 A. 2 R. 27½ P.), £109 10s. per annum.
Fee Farm near Lewes, Sussex, rent £19 4s. per annum.
One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.
£14,000 Reduced 3 per Cent. Annuities.
£29,569 15s. 7d. Consolidated Bank Annuities.
£513 9s. 8d. New 2½ per Cent. Stock—Bakerian and Copley Medal Fund.
£660 Madras Guaranteed 5 per Cent. Railway Stock—Davy Medal Fund.
£10,000 Italian Irrigation Bonds—The Gassiot Trust.

Scientific Relief Fund.

	£	s.	d.
Investments up to July 1870, New 3 per Cent. Annuities	£6202	15	6
" " Metropolitan $3\frac{1}{2}$ Consols	£ 100	0	0
	£6302	15	6

Dr.	£	s.	d.	*Cr.*	£	s.	d.
Balance	174	3	6	By Grants	175	0	0
Dividends	182	4	0	Balance	281	7	6
Donation	100	0	0				
	£456	7	6		£456	7	6

Statement of Income and Expenditure (apart from Trust Funds) during the Year ending November 30, 1871.

	£	s.	d.
Annual Subscriptions	1076	0	0
Admission Fees	170	0	0
Compositions	460	0	0
Rents	257	4	4
Dividends on Stock (exclusive of Trust Funds)	1010	12	6
" on Stevenson Bequest	465	10	10
Sale of Transactions, Proceedings, &c.	490	7	4
Oliveira Bequest (Interest)	14	14	3
Petty Repayments	2	17	9
Income available for the Year ending Nov. 30, 1871	3947	7	0
Expenditure in the Year ending Nov. 30, 1871	4116	7	5
Excess of Expenditure over Income in the Year ending Nov. 30, 1871	£169	0	5

	£	s.	d.	£	s.	d.
Salaries, Wages, and Pension				1057	5	4
Equatoreal Telescope (balance)				518	2	10
Spectroscope for India				77	14	5
The Scientific Catalogue				266	19	0
Books for the Library				128	9	11
Binding ditto				83	1	4
Printing Transactions, Part II. 1870 and Part I. 1871	384	1	3			
Ditto Proceedings, Nos. 123-129	354	11	4			
Ditto Miscellaneous and Library Catalogue	167	14	11	1634	19	3
Paper for Transactions and Proceedings	246	10	6			
Binding and Stitching ditto	79	9	6			
Engraving and Lithography	402	11	9			
Fittings, Cleaning, and Repairs	87	12	10			
Miscellaneous Expenses	44	3	1			
Coal, Lighting, and Gas Repairs	100	4	6			
Tea Expenses	24	3	6			
Fire Insurance	20	1	6	349	15	4
Taxes	13	17	6			
Advertising	15	16	6			
Postage, Parcels, and Petty Charges	41	13	11			
Mablethorpe Schools, Donation	2	2	0			
				£4116	7	5

W. SPOTTISWOODE, *Treasurer.*

Account of the appropriation of the sum of £1000 annually voted by Parliament to the Royal Society (the Government Grant), to be employed in aiding the advancement of Science (continued from Vol. XIX. p. 144).

1871.

1. To E. T. Chapman, for continuing his Researches on the Physical Properties of Organic Bodies	£50
2. To R. H. Scott and Dr. J. Rae, to defray expenses in obtaining Specimens of Fossil Plants from N.W. America	50
3. To E. J. Mills, for a Research on Chemical Activity	50
4. To Dr. Stenhouse, for continuation of Researches on Orcin and its Homologues, Furfurol and its Isomers, and on Bromanil	100
5. To W. De La Rue, for completing the ten years' Series of Observations with the Kew Photo-heliograph	200
6. To Dr. C. R. A. Wright, for continuation of Researches on the Chemistry of the Opium Alkaloids	20
7. To A. Dupré, for continuing Investigations on the Specific Heat and other Physical Characters of Aqueous Mixtures and Solutions	80
8. To Prof. Guthrie, for continuation of Experiments on Approach caused by Vibration	100
9. To the Eclipse Committee, towards the expenses of the Expedition, December 1870	250
10. To R. Etheridge, for assisting in the publication of a complete Catalogue of British Fossils, Stratigraphically and Palæontologically arranged	150
11. To J. N. Lockyer, for mounting equatorially a Reflecting Telescope, and for procuring another Spectroscope	150
12. To Dr. Andrews, for a Research on the Behaviour of Gases under great Pressure	100
13. To Prof. W. C. Williamson, for Researches into the Organization of the Fossil Plants of the Coal-measures	25
14. To Dr. Pettigrew, for Experiments on Artificial Flight	100
15. To Dr. Wright, for continuation of Researches on the Chemistry of the Opium Alkaloids	40
16. To W. N. Hartley, for a Microscope for Researches in Spontaneous Generation	15
17. To Dr. Carpenter, for further Deep-Sea Researches in the Mediterranean	100
18. To Dr. Divers, for Chemicals and Apparatus required for Researches into the Salts of Nitrous Oxide	25
	£1605

Dr.	£	s.	d.		£	s.	d. *Cr.*
To balance on hand, Nov. 30, 1870....	1627	4	2	By appropriations as above	1605	0	0
To Grant from the Treasury (1871)..	1000	0	0	Balance on hand, Nov. 30, 1871	1031	8	8
To interest	9	4	6				
	£2636	8	8		£2636	8	8

Account of Sums granted from the Donation Fund in 1871.

1. Mr. J. P. Gassiot, to defray expense of making prints from Negatives of Sun-Pictures taken at Kew Observatory (second instalment. See Vol. XIX. p. 145)........................ £60

2. Dr. Carpenter, towards the expenses of his return journey from Alexandria 50

3. Mr. E. Whymper, for operations to investigate the Structure of Glacier Ice 50

£160

Presents received November 16, 1871.

Transactions.

Bologna :—Accademia delle Scienze dell' Istituto. Memorie. Serie Seconda. Tomo X. 4to. *Bologna* 1870–71. Rendiconto delle Sessioni. Anno Accademico 1870–71. 8vo. *Bologna* 1871. The Academy.

Calcutta :—Geological Survey of India. Memoirs. Vol. VII. Part 1–3. roy. 8vo. *Calcutta* 1869–71. Palæontologia Indica (Cretaceous Fauna of Southern India). Vol. III. No. 1–8. 4to. *Calcutta* 1870–71. Records. Vol. II. Part 2–4; III., IV. Part 1, 2. Title-pages. roy. 8vo. *Calcutta* 1869–71. The Survey.

Cherbourg :—Société des Sciences Naturelles. Mémoires. Tome XV. 8vo. *Cherbourg* 1870. Catalogue de la Bibliothèque. Partie 1. Publications des Académies. 8vo. *Cherbourg* 1870. Complément à l'étude sur la Houle et le Roulis par E. Bertin. 8vo. *Cherbourg* 1870. The Society.

Danzig :—Naturforschende Gesellschaft. Schriften, Neue Folge. Band II. Heft 3, 4. roy. 8vo. *Danzig* 1871 (duplicate with photographs). The Society.

Dresden :—Kais. Leopoldino-Carolinische deutsche Akademie der Naturforscher. Verhandlungen (Nova Acta). Band XXXV. 4to. *Dresden* 1870. The Academy.

Transactions (*continued*).

Göttingen:—K. Gesellschaft der Wissenschaften. Nachrichten, aus dem Jahre 1870. 8vo. *Göttingen* 1870. The Society.

Helsingfors:—Finska Vetenskaps Societeten. Acta. Tom. IX. 4to. *Helsingforsiæ* 1871. Öfversigt af... Förhandlingar. XIII. 1870–71. 8vo. Bidrag till Kännedom af Finlands Natur och Folk. Häftet 17. 8vo. *Helsingfors* 1871. Bidrag till Finlands Officiela Statistik V. 4to. *Helsingfors* 1869. The Society.

London:—International Congress of Prehistoric Archæology. Transactions of the Third Session, Aug. 1868. 8vo. *London* 1869. Sir John Lubbock, Bart., F.R.S.

Royal Agricultural Society. Journal. Second Series. Vol. VII. Part 2. 8vo. *London* 1871. The Society.

Royal Geographical Society. Journal. Vol. XL. 8vo. *London* 1870. General Index to the Contents of the first ten volumes of the London Geographical Journal, compiled by J. R. Jackson. 8vo. *London* 1844. Proceedings. Vol. I. No. 1, 5–11; Vol. XV. No. 2–4. 8vo. *London* 1855–71.

Royal Medical and Chirurgical Society. General Index to the first fifty-three volumes of the Medico-Chirurgical Transactions. 8vo. *London* 1871. Proceedings. Vol. VI. No. 8. 8vo. *London* 1871. The Society.

Saint Bartholomew's Hospital. Reports. Vol. VII. 8vo. *London* 1871. The Hospital.

Lyon:—Académie des Sciences, Belles Lettres et Arts. Mémoires. Classe des Lettres. Tome XI., XIV. roy. 8vo. *Lyon* 1862–69. The Academy.

Société d'Agriculture, Histoire Naturelle et Arts Utiles. Annales. 4e série. Tome I. 1868. roy. 8vo. *Lyon* 1869. The Society.

Munich:—Königl. bayerische Akademie der Wissenschaften. Abhandlungen. Historische Classe. Band XI. Abth. 2, 3. Philos.-philol. Classe, Band XII. Abth. 2. 4to. *München* 1869–71. Sitzungsberichte, 1870, II. Heft 3, 4. Math.-phys. Classe, 1871, Heft 1. Philos.-philol. Classe, 1871, Heft 1–3. 8vo. Almanach für des Jahr 1871. 12mo. *München*. Brahma und die Brahmanen von Martin Haug. 4to. 1871. The Academy.

Prag:—Königl. böhmische Gesellschaft der Wissenschaften. Abhandlungen vom Jahre 1870. Sechste Folge. Band IV. 4to. *Prag* 1871. Sitzungsberichte, Jahrgang 1870. 8vo. *Prag* 1870–71. The Society.

St. Petersburg:—Académie Impériale des Sciences. Mémoires. Tome XVI. No. 1–8. 4to. *St. Pétersbourg* 1870. Bulletin. Tome XV. No. 3–5; Tome XVI. No. 1. 4to. *St. Pétersbourg* 1871. The Academy.

Stockholm:—Kongl. Svenska Vetenskaps-Akademie. Handlingar. Ny

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Följd. Bandet VII. Haftet 2; B. VIII. 4to. *Stockholm* 1868–69. Öfversigt af . . . Förhandlingar. Årgången XXVI. 1869. 8vo. 1870. The Academy.

Vienna:—Geographische Gesellschaft. Mittheilungen. Neue Folge, III. Nr. 1–14. 8vo. *Wien* 1870. Wilhelm Haidinger, von M. A. Becker. 8vo. 1871. The Society.

Kais. Akademie der Wissenschaften. Denkschriften. Math.-naturw. Classe, Band XXX. Phil.-hist. Classe, Band XIX. 4to. *Wien* 1870. Sitzungsberichte. Math.-nat. Classe. Erste Abth. Band LX. Heft 3–5, B. LXI. H. 1–5, B. LXII. Heft 1, 2. Zweite Abth. B. LX. H. 3–5, B. LXI. H. 1–5, B. LXII. H. 1–3. Register zu den Bänden 51 bis 60. Phil.-hist. Classe, B. LXIII. H. 1–3, B. LXIV. H. 1–3, B. LXV. H. 1–4, B. LXVI. H. 1. 8vo. *Wien* 1870. Almanach. Jahrg. 20, 1870. Phänologische Beobachtungen aus dem Pflanzen- und Thierreiche von K. Fritsch. Heft 8. Jahrgang 1857. 4to. *Wien* 1869. Tabulæ Codicum Manuscriptorum in Bibliotheca Palatina Vindobonensi. Vol. IV. 8vo. *Vindobonæ* 1870. The Academy.

K. K. Geologische Reichsanstalt. Jahrbuch. Band XXI. Nr. 1, 2. 8vo. *Wien* 1871. Verhandlungen. 1871, Nr. 1–5, 7–10. 8vo. Abhandlungen. Band V. Heft 1, 2. 4to. *Wien* 1871. Zur Erinnerung an Wilhelm Haidinger von F. v. Hauer. 8vo. *Wien* 1871. The Institute.

Washington:—Smithsonian Institution. Smithsonian Contributions to Knowledge. Vol. XVII. 4to. *Washington* 1871. Annual Report of the Board of Regents. 8vo. *Washington* 1871. The Institution.

Würzburg:—Philosophisch-medicinische Gesellschaft. Jahrbücher. Band I. Heft 1–3. 8vo. *Würzburg* 1828. Dr. Sharpey, Sec. R. S.

Acland (H. W.), F.R.S. National Health. Second edition. 8vo. *Oxford* 1871. The Author.

Carneri (B.) Sittlichkeit und Darwinismus. 8vo. *Wien* 1871. C. Darwin, F.R.S.

Dircks (H.) Perpetuum Mobile. Second Series. 8vo. *London* 1870. The Author.

Fraas (O.) Die Fauna von Steinheim. 4to. *Stuttgart* 1870. C. Darwin, F.R.S.

Huygens (C.) Traité de la Lumière. 4to. *Leide* 1690. Dr. Sharpey, Sec. R.S.

Morfit (C.) A Practical Treatise on the Manufacture of Soaps. 8vo. *London* 1871. The Author.

Pidiret (T.) Wissenschaftliches System der Mimik und Physiognomik. 8vo. *Detmold* 1867. C. Darwin, F.R.S.

Taylor (A. S.), F.R.S. The Principles and Practice of Medical Jurisprudence. 8vo. *London* 1865. The Author.

Thomas (Edward), F.R.S. The Chronicles of the Pathán Kings of Dehli. 8vo. *London* 1871. The Author.

November 23, 1870.

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Amsterdam:—Koninklijke Akademie van Wetenschappen. Verhandelingen. Deel 12. 4to. 1870. Afdeeling Letterkunde. Deel 6. 4to. 1871. Verslagen en Mededeelingen. Afd. Natuurkunde. Tweede Reeks. Deel 5. 8vo. 1871. Processen-Verbaal 1879–71. Jahrboek voor 1870. 8vo. The Academy.

Batavia:—Bataviaasch Genootschap van Kunsten en Wetenschappen. Tijdschrift voor Indische Taal-, Land- en Volkenkunde. Deel 19. Afl. 1–6. 8vo. 1869–70. Notulen van de Algemeene en Bestuurs-Vergaderingen. Deel 7. No. 2–4; Deel 8. No. 1, 2. 8vo. 1869–70. The Society.

Buenos Aires:—Museo Público. Anales, por German Burmeister. Entrega 8. 4to. *Buenos Aires* 1871. The Museum.

Haarlem:—Hollandsche Maatschappij der Wetenschappen. Natuurkundige Verhandelingen. 3[e] série. Tom. I. 3. 4to. 1870. Archives Néerlandaises des Sciences Exactes et Naturelles, redigées par E. H. von Baumhauer. Tome V. liv. 4, 5; T. VI. liv. 1–3. 8vo. *La Haye* 1870–71. The Society.

Musée Teyler. Archives. Vol. III. fasc. 2. roy. 8vo. *Harlem* 1871. The Museum.

London:—British Association. Report of the Fortieth Meeting held at Liverpool in September 1870. 8vo. *London* 1871. The Association.

British Museum. Catalogue of Syriac Manuscripts, by W. Wright. Part 2. 4to. *London* 1871. Catalogus Codicum Manuscriptorum Orientalium. Partis 2 Suppl. fol. 1871. Catalogue of the Specimens of *Dermaptera Saltatoria*, by F. Walker. Part 5. 8vo. 1871. Hand-List of Genera and Species of Birds, by G. R. Gray. Part 3. 8vo. 1871. Catalogue of the Collection of Meteorites. 8vo. 1871. Guide to the First Vase Room. 8vo. 1871. Guide to the Bronze Room. 8vo. 1871. The Museum.

Clinical Society. Transactions. Vol. IV. 8vo. *London* 1871.

Institution of Civil Engineers. Minutes of Proceedings. Vol. XXXI., XXXII. 8vo. *London* 1871. General Index from Vols. XXI. to XXX. 8vo. 1871. The Institution.

Transactions (*continued*).

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Pathological Society. Transactions. Vol. XXII. 8vo. *London* 1871. The Society.

Royal Society. Philosophical Transactions. Vol. LXX., LXXI. Part 2; LXXII.-LXXV. 4to. *London* 1780-85. The Royal Institution.

Observations, &c.

Cape of Good Hope:—Royal Observatory. Results of Astronomical Observations made in the year 1856, reduced and printed under the superintendence of E. J. Stone, F.R.S. 8vo. *Cape Town* 1871. (2 copies.) The Observatory.

Greenwich:—Royal Observatory. Astronomical and Magnetical and Meteorological Observations made in the year 1869, under the direction of G. B. Airy, C.B. 4to. *London* 1871. The Admiralty.

Lisbon:—Observatorio do Infante D. Luiz. Annaes. Vol. III. No. 4, Vol. IV.-VIII. folio. *Lisboa* 1866-70. The Observatory.

London:—Meteorological Office. Weather Reports, Jan. 1 to June 30, 1871. folio. Quarterly Weather Report, 1870. Part 1, 2. 4to. 1871. Contributions to our knowledge of the Meteorology of Cape Horn and the West Coast of South America. 4to. 1871. Report of the Meteorological Committee of the Royal Society for the year ending 31st December, 1870. 8vo. 1871. The Office.

Madrid:—Real Observatorio. Anuario, 1869, 1870. 12mo. Observaciones Meteorológicas, 1865-68. 3 vols. 8vo. Resumen de las Observaciones Meteorológicas efectuadas en la Peninsula. 1865-68. 2 vols. 8vo. The Observatory.

New South Wales:—Results of Meteorological Observations made during 1870, under the direction of H. C. Russell. 8vo. *Sydney* 1871. The Government of New South Wales.

Oxford:—Radcliffe Observatory. Results of Astronomical and Meteorological Observations made in the year 1868, under the superintendence of the Rev. Robert Main. Vol. XXVIII. 8vo. *Oxford* 1871. The Radcliffe Trustees.

Paris:—l'Observatoire. Annales, publiées par le Directeur. Observations. Tome XXIII. 1867. 4to. 1871. Atlas des Mouvements généraux de l'Atmosphère. Année 1865. Juillet-Décembre. Oblong. *Paris* 1869. The Observatory.

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Utrecht:—Koninklijk Nederlandsch Meteorologisch Instituut. Nederlandsch Meteorologisch Jaarboek voor 1869, Deel 2; 1870, Deel 1. 4to. The Institute.

Vienna:—K. K. Central-Anstalt für Meteorologie und Erdmagnetismus. Jahrbücher von Carl Jelinek und Carl Fritsch. Neue Folge. Band V. 4to. *Wien* 1870. The Institution.

Plaster Bust of Martin Folkes, P.R.S.; also Plaster Bust of Benjamin Franklin, F.R.S. Presented by Earl Stanhope, F.R.S.

December 7, 1871.

GEORGE BIDDELL AIRY, C.B., President, in the Chair.

The President announced that he had appointed the following Members of the Council to be Vice-Presidents:—

The Treasurer.
Mr. Francis Galton.
Sir John Lubbock.
Sir James Paget.
The Earl of Rosse.
Sir Edward Sabine.

The following communications were read:—

I. "On the Fossil Mammals of Australia.—Part VI. Genus *Phascolomys*, Geoffr." By Prof. Owen, F.R.S. Received September 14, 1871.

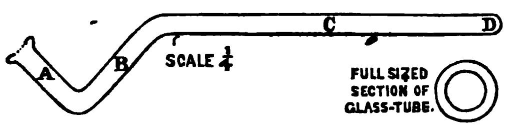
(Abstract.)

In this paper the author premises a reference to former ones on the Osteology of existing *Marsupialia*, in the 'Transactions of the Zoological Society,' and to his 'Catalogue of the Osteological Series in the Museum of the Royal College of Surgeons,' in which are defined cranial characters serving to distinguish existing species of the genus *Phascalomys*, Geoffr.; and after showing, in subsequently received materials, the kind and extent of variety of such characters in the same species, he proceeds to apply the knowledge so gained to the determination of some fossil remains of species of Wombat, similar in size to the known existing kinds.

The extinct *Phascolomys Mitchelli*, indicated by remains brought to England in 1835 by Sir Thomas Mitchell, C.B., the discoverer of the bone-caves of Wellington Valley, Australia, is determined by specimens subsequently obtained by Prof. Alex. M. Thomson and Gerard Krefft, Esq., from the same caves. A second species, distinguished by characters of the nasal bones, is called, after its discoverer, *Phascolomys Krefftii*. Modifications of the lacrymal, maxillary, and palatal bones in the existing kinds of Wombat are also applied to the determination of the fossils: specimens from the freshwater deposits of Queensland are thus shown to belong to the species, *Phascolomys Mitchelli*, originally founded on fossils from the breccia-caves of New South Wales. The author next proceeds to point out the characters of the mandible in existing Wombats available in the determination of extinct species of *Phascolomys*. On this basis he defines specimens which he provisionally refers to his *Phascolomys Krefftii*. He then points out the mandibular characters of *Phascolomys Mitchelli*, and shows that the existing *Phascolomys latifrons* was represented by mandibular fossils from the breccia-caves of Wellington Valley. Proceeding next to the description of fossil mandibular remains of the genus *Phascolomys*, from the freshwater deposits of Queensland, the author defines *Phascolomys Thomsoni*, *Phasc. platyrhinus*, and *Phasc. parvus*. The latter, seemingly extinct, species is markedly inferior in size to any of the known existing species. An account of the extinct kinds of Wombat, exceeding in size the existing species, will be the subject of a succeeding communication. The present is illustrated by subjects occupying seven plates and eight woodcuts, all the figures being from nature and of the natural size.

II. "On the Solvent Power of Liquid Cyanogen." By G. Gore, F.R.S. Received September 1, 1871.

In the following experiments a number of stout tubes of refractory glass of the annexed shape were employed.



Each tube was closed at one end, and had a flanged mouth at the other. The limb A was 1½ inch long, the limb B 2 inches, and C 7 inches.

In making the experiments, each tube was first filled, to an extent of 5½ inches of its length, with highly dried crystals of mercuric cyanide; a small fragment of asbestus* was then pushed tightly against the end of the cyanide by means of a thin rod of gutta percha, and the bend of the tube cleaned by means of a slender brush. A number of taper plugs of gutta

* This may be dispensed with if the mercuric cyanide is perfectly dry.

percha were previously made by softening the end of a rod of that substance in boiling water and then chilling it; and loops of thin copper wire were also prepared for the purpose of securing the plugs.

Having placed in the bend of the tube the substance to be operated on, a plug was wetted with chloroform and at once firmly inserted in the mouth of the tube and secured by means of the wire. A wet rag was wrapped round the limb B, and the portion of the limb C containing the cyanide was inserted in a loosely fitting tube of copper foil within a tube of wrought iron about two feet long, supported in a nearly horizontal position, with the end D the lowest.

A vessel having been previously arranged to drop cold water continually upon the wet rag, a glass screen was placed by the side of the exposed part of the glass tube, and heat was gradually applied, by means of a row of Bunsen's burners, to the part of the iron tube containing the limb C. In about four minutes liquid cyanogen began to form, and in about five minutes longer about two inches in length of the bend was filled with that liquid, and the heat was then discontinued. The temperature of the iron tube was below that of visible redness; if the heat applied was not too great the glass tube retained its original form, and might afterwards be easily removed from its casing and set aside for future examination. A white fog, and signs of a heavy vapour in the bend of the tube, always immediately preceded the formation of the liquid. In many instances some of the cyanogen retained the liquid state for several days, and even weeks, especially if the bend of the tube was kept cool by means of a wet sponge.

The cyanide employed in the experiments should be crushed to a coarse powder, and highly heated a long time, with constant stirring, until it is of a slightly brown colour; otherwise, immediately previous to the appearance of the liquid cyanogen, a small quantity of a brownish treacley liquid distils over and affects the results.

More than 130 substances, including solids and liquids, and a great variety of simple and compound bodies, were subjected to the action of the liquid; the substances were, in nearly all cases, as free from moisture as it was possible to obtain them. The following are the results obtained:—

Substances soluble in liquid Cyanogen at 60° *Fahr.*—Camphor dissolved rapidly and copiously. Hydrate of chloral quickly and freely. Solid C_2Cl_6 was freely soluble. Iodine was moderately soluble, and formed a deep red liquid. Picric acid moderately soluble; the solution was yellow. White phosphorus dissolved very slowly, and to but a small extent*. Gum benzoin, also white cane-sugar, dissolved slowly and sparingly. Gum assafœtida and gamboge dissolved slightly, and formed yellowish liquids. Bisulphide of carbon, C_2Cl_2 and C_2Cl_4, mixed perfectly with the liquid; CCl_4 mixed less perfectly. Water was only slightly absorbed.

* See also Gmelin's Handbook, vol. viii. p. 147, article "Cyanide of Phosphorus."

Substances insoluble in liquid Cyanogen at 60° Fahr.—Water (nearly insoluble, would not mix with the liquid*, and produced no chemical change). Iodic acid. Fine lampblack. Well-burned wood-charcoal (two kinds). Crystals of boron. Boracic acid. Crystals of silicon. Precipitated silica. Titanic acid. A fragment of sulphur. Selenium. Selenious acid. Tellurium. Arsenic. Arsenious and arsenic acids. Realgar. Orpiment. Antimony. Antimonic oxide. Antimonic acid. Fused fluoride of antimony. Red sesquisulphide of antimony. Bismuth. Fluoride of bismuth. The oxides of iridium and silver. Argentic nitrate, fluoride, chloride, iodide, and iodate. Mercury. Mercuric chloride. Copper. Cuprous iodide. Chloride of Nickel. Iron. Mundic. Peroxide of manganese. Fluoride of manganese. Chromic oxide. Violet chloride of chromium. Oxide and fluoride of uranium. Red oxide and peroxide of lead. Plumbic chromate. Tin. Cadmium. Sulphide of cadmium. Zinc. Tungstic and niobic acids. Oxide of cerium. Carbonate of glucinum. Aluminium. Magnesium. Magnesia. Dry lime. Anhydrous carbonate of sodium. Potassic nitrate, bromide, iodate, and fluozirconate. Bichromate and bitartrate of rubidium. Bitartrate of cæsium. Chloride and carbonate of ammonium. Oxalate of cerium. Paracyanogen. Cyanide of mercury. Nitrocyanide of titanium. Pyrogallic, gallic, tannic, and succinic acids. Sulphate of quinine. Alloxan. Starch. Milk-sugar. Egg-albumen. Gun-cotton. The gums ammoniac, animi, copal, dammar, elemi, galbanum, guaiacum, juniper, kowrie, mastic, myrhha, olibanum, opoponax, sagapenum, seed-lac, shellac, thus, and tragacanth. Yellow resin. Egyptian asphalte. Gutta percha. India-rubber. Paraffin. Anthracene. Stearine. Bee's-wax. Spermaceti. Aloes—Barbadoes, Cape, hepatic, and Socotrine. Brazil-wood. Red sandal-wood. Cuba yellow fustic. Terra-japonica.

The following substances produced some changes, but did not dissolve:—Perfectly white and dry iodic acid made the liquid of a pink colour. Dry selenious acid and iodate of potassium became slightly pink. Yellow chloride of nickel (nearly anhydrous) became at once green†. Cuprous iodide acquired the colour of vermilion. Sesquicarbonate of ammonium became brown. The gums myrrha and guaiacum coloured the liquid yellowish. Bisulphide of carbon, saturated with sulphur, rapidly set free its sulphur in the form of crystals, the bisulphide being absorbed and dissolved by the cyanogen. A saturated solution of phosphorus in bisulphide of carbon quickly liberated its phosphorus in a similar manner, but not in a crystalline form. The contact of moisture and an alkaline substance with the liquified gas caused the formation of a brown solid body, apparently paracyanogen.

From these results it is evident that liquid cyanogen, at 60° Fahr., is a remarkably inert body, and possesses very little solvent power for sub-

* See Gmelin's Handbook of Chemistry, vol. vi. p. 388.

† Probably in consequence of the presence of a trace of moisture.

stances in general; out of 132 substances only 14 were dissolved, and not a single one exhibited signs of strong chemical action.

III. "On Fluoride of Silver.—Part III." By G. Gore, F.R.S. Received October 6, 1871.

(Abstract.)

In this communication the author has finally shown that the action of iodine, under the influence of heat (including the process described by Kämmerer, Phil. Mag. 1863, vol. xxv. p. 213, for the isolation of fluorine), does not liberate uncombined fluorine, but produces fluoride of iodine and iodide of silver, a double salt, composed of iodide of silver and fluoride of platinum, being produced at the same time by corrosion of the platinum vessels, if the temperature approaches a red heat.

The fluoride of iodine produced is a highly volatile and colourless liquid, does not corrode mercury or red-hot platinum, corrodes glass at 60° Fahr., and crystals of silicon at a red heat, also platinum in contact with argentic fluoride in a state of fusion; it instantly turns a deal splint black, fumes powerfully in the air, and is decomposed with violence by water into hydrofluoric and iodic acids, in accordance with the following equation:— $IF_5+3H_2O=5HF+HIO_3$. It dissolves iodine, and is absorbed by that substance; it is also absorbed either by argentic fluoride or iodide when those substances are cooled in its vapour, and may be expelled from them at a red heat. Its vapour quickly darkens the colour of a deal splint, and very gradually turns paraffin brown.

The platinum vessels in which the reaction with iodine was effected were considerably corroded (but less so than when bromine or chlorine were employed), and many expensive vessels were rendered useless by this cause during the experiments.

No chemical change occurred on heating argentic fluoride to redness with pure carbon.

By heating this fluoride to redness in a current of dried coal-gas, it was wholly reduced to metallic silver, hydrofluoric acid and tetrafluoride of carbon being evolved.

In liquid cyanogen, argentic fluoride neither dissolved nor suffered chemical change; but at a low red heat, in a current of dry cyanogen gas, it was entirely reduced to metal, either nitrogen and tetrafluoride of carbon, or fluoride of cyanogen being liberated. An aqueous solution of silver fluoride was precipitated by passing a prolonged current of cyanogen gas through it. Fluoride of silver was also decomposed by fusion with paracyanogen.

Argentic fluoride was not dissolved or chemically changed by immersion in anhydrous liquified hydrocyanic acid; but by passing the dry acid in vapour over the red-hot salt, the latter was decomposed and metallic

silver liberated. Aqueous hydrocyanic acid readily precipitated a solution of argentic fluoride.

Fluoride of silver was not decomposed by heating it to redness in an atmosphere of carbonic oxide or carbonic acid gases; but by prolonged passage of the mixed gases through an aqueous solution of the salt, a brown precipitate, soluble in aqueous hydrofluoric acid, was produced.

By fusing the fluoride in a current of vaporous terchloride of carbon, it was wholly converted into argentic chloride, the vessels being much corroded, and an insoluble double salt of platinum and silver formed. Similar results took place on using tetrachloride of carbon. Silver fluoride was insoluble, and remained unchanged in liquid tetrachloride of carbon at 60° Fahr.; and terchloride or tetrachloride of carbon had no chemical effect upon an aqueous solution of the silver-salt. A solution of bromine or iodine in tetrachloride of carbon was quickly decolorized by agitation with small particles of argentic fluoride.

Crystals of boron did not decompose fluoride of silver at a low red heat, nor chemically change at 60° Fahr. an aqueous solution of the salt containing either free hydrofluoric or nitric acids.

Vitrified boracic acid violently decomposed fluoride of silver in a state of fusion, emitting copious white acid fumes; but it had no chemical effect upon an aqueous solution of the salt at 60° Fahr.

By placing crystals of silicon upon argentic fluoride in a state of fusion, they become at once red-hot, undergoing rapid combustion, and evolving fluoride of silicon. A lump of fused silicon slowly decomposed an aqueous solution of fluoride of silver, setting free metallic silver in crystals. Crystals of silver behaved similarly, but much more rapidly, and evolved abundance of gas if the solution contained free hydrofluoric acid; on adding nitric acid to this mixture, bubbles of spontaneously inflammable silicide of hydrogen gas were evolved and ignited.

Pure and dry precipitated silica added to fluoride of silver, at a temperature of low redness, evolved much heat, with violent action, and set free metallic silver.

No chemical change took place on passing fluoride of silicon over red-hot fluoride of silver.

Argentic fluoride in a state of fusion is rapidly decomposed by sulphur with evolution of heat; fluoride of sulphur is at the same time produced, and argentic sulphide formed. To ascertain whether fluoride of sulphur is a gas or a volatile liquid, an apparatus called a "gas-collector" was devised and employed, and a full description of its construction is given. By using this apparatus, substances may be heated without contact with the external air, and without subjecting the joints of the apparatus in which they are heated to leakage by expansion or contraction of the gaseous contents.

Fluoride of sulphur was found to be a heavy colourless vapour, uncondensable at the temperature of melting ice and at the ordinary atmo-

spheric pressure. It corrodes glass, fumes strongly in the air, and has a characteristic and very powerful dusty odour, not very unlike that of a mixture of chloride of sulphur and sulphurous anhydride. Sulphur rapidly decomposed an aqueous solution of argentic fluoride.

Sulphurous anhydride passed over fluoride of silver at an incipient red heat, produced little or no decomposition of the silver-salt. Vaporous fluoride of sulphur also produced no visible effect.

By passing the vapour of liquid chloride of sulphur over the fluoride in a state of fusion, chemical action occurred, a vapour was evolved which corroded glass and possessed a dusty odour, but did not condense to a liquid; it was probably fluoride of sulphur. The saline residue consisted of argentic chloride and sulphide. A solution of argentic fluoride was decomposed by agitation with liquid chloride of sulphur, hydrofluoric acid being evolved, and argentic chloride and sulphide produced.

Argentic fluoride did not dissolve in bisulphide of carbon. By passing the vapour of the latter substance over the silver-salt at a red heat, a chemical change took place, and a fuming acid vapour was evolved, in accordance with the following equation :—$4AgF+CS_2=2Ag_2S+CF_4$. A solution of bromine or iodine in bisulphide of carbon was rapidly decolorized by agitation with particles of argentic fluoride, and the liquid acquired the odour of tetrafluoride of carbon.

December 21, 1871.

GEORGE BIDDELL AIRY, C.B., President, in the Chair.

The following communications were read :—

I. "Contributions to the History of Orcin.—No. II. Chlorine- and Bromine-substitution Compounds of the Orcins." By JOHN STENHOUSE, LL.D., F.R.S., &c.* Received October 11, 1871.

Schunck†, and subsequently the author of this paper‡, many years ago studied the action of chlorine upon orcin, and obtained more or less crystalline products, contaminated with a brown resinous matter, from which however, they did not succeed in separating the crystals in a state of purity. In the year 1864 De Luynes § obtained a crystalline substance by acting on orcin with a mixture of potassium chlorate and hydrochloric acid. De Luynes states it to be trichlororcine, $C_7\,H_5\,Cl_3\,O_2$, and in Kekulé's 'Benzolderivate' the melting-point is given at 159°.

* A preliminary notice of several of the compounds described in this paper appeared in the Chem. News, vol. xxiii. p. 230, and Zeits. Chem. vol. vii. p. 229.

† Ann. Chem. Pharm. vol. liv. p. 271.

‡ Phil. Trans. 1848, p. 88, and Ann. Chem. Pharm. vol. lxvii. p. 97.

§ Ann. Chem. Pharm. vol. cxxx. p. 34; Kekulé's 'Benzolderivate,' vol. i. p. 388.

Chlororcin.

Pentachlororcin, $C_7H_3Cl_5O_2$.—This compound was obtained by the action of chlorine upon orcin when the former was kept in excess. Two methods of effecting this object were employed,—the first by adding the orcin to chlorine hydrate, the other by the action of potassium chlorate and hydrochloric acid.

A pulpy mixture of the crystalline hydrate of chlorine and water was first prepared by passing a current of chlorine, with occasional agitation, through water to which about one third of crushed ice had been added, until a sufficient quantity of the hydrate was formed. On gradually adding a moderately strong aqueous solution of orcin, a purple colour was produced when the solution first came in contact with the chlorine hydrate, but disappeared immediately on agitation. The addition of the orcin was continued until nearly all the chlorine hydrate was decomposed. It was always advisable to leave a slight excess of the latter, as a purer product was then obtained, and also to have some crushed ice present in the mixture, to avoid decomposition and consequent loss of the hydrate by the heat produced during the reaction. The colourless or pale yellow solution, on standing, yielded a crystalline deposit of crude pentachlororcin.

After many trials, however, it was found that pentachlororcin could generally be more conveniently prepared in quantity by submitting orcin to the chlorinating action of a mixture of hydrochloric acid and potassium chlorate in the following manner:—Four parts of powdered potassium chlorate and a solution of two parts of orcin in seven parts of hydrochloric acid were added to 35 parts of hydrochloric acid, sp. gr. 1·17, placed in a large beaker, and kept cool by immersion in water; a small portion of the potassium chlorate was first added to the hydrochloric acid in the beaker; on pouring in some of the solution of orcin, the same evanescent purple colour made its appearance as when chlorine hydrate was used; the alternate addition of the chlorate and the orcin solution was continued in such a manner that there might always be an excess of chlorate, and that the contents of the beaker never became very hot. It was found necessary to use concentrated hydrochloric acid in this experiment, as otherwise the product was largely contaminated with a viscid oily compound. After twelve to twenty hours the crystalline chlororcin was collected and washed with a small quantity of water. Orcin treated with chlorine hydrate yields nearly twice its weight of crude chlororcin, but with potassium chlorate and hydrochloric acid about 150 per cent. of its weight.

In order to purify the crude chlororcin obtained by either of the above-described methods, it was dissolved when dry in a considerable quantity of carbon disulphide (eight measures), filtered and concentrated by distillation (to one half). On being set aside for some time it usually crystallized out; but as it exhibits strongly the phenomenon of supersaturation, especially when impure, it was sometimes necessary to agitate or add a crystal

of the substance. The solution then immediately deposited the chlororcin, and occasionally became hot enough to cause ebullition of the carbon disulphide. Two or three crystallizations rendered it quite pure. Pentachlororcin crystallizes from carbon disulphide in large colourless prisms, melting at 120°·5. It is moderately soluble in bisulphide of carbon and benzol, and readily in ether. It dissolves somewhat in cold alcohol, and when boiled with it for some time undergoes a change which I have not as yet investigated,—water throwing down an oil which only solidifies after having been exposed to the air for some days in a shallow vessel. The pentachlororcin is but very slightly soluble in water, to which, however, it communicates an exceedingly disagreeable and persistent bitter metallic flavour. When boiled with water it is decomposed, an oil and a crystalline solid passing over with the vapour. The oil, which has a peculiar odour, recalling that of chloropicrin, is heavier than water; the solid compound crystallizes in needles, and is identical with the trichlororcin described below. When heated with concentrated sulphuric acid it blackens and decomposes, giving off hydrochloric acid. It dissolves in nitric acid by the aid of heat, and crystallizes out unchanged on cooling. The addition of water to the solution precipitates the pentachlororcin. By long boiling with the acid it is decomposed with evolution of nitrous fumes.

Analysis of Pentachlororcin.

I. ·901 grm. substance gave ·937 grm. carbonic anhydride and ·096 grm. of water.

II. ·196 grm. substance gave ·473 grm. argentic chloride.

III. ·360 grm. substance gave ·871 grm. argentic chloride.

IV. ·229 grm. substance gave ·555 grm. argentic chloride.

V. ·202 grm. substance gave ·488 grm. argentic chloride.

Theory.			I.	II.	III.	IV.	V.	Mean.
C_7 =	84	= 28·33	28·36	..	..	..	..	28·36
H_3 =	3	= 1·01	1·18	..	..	..	..	1·18
Cl_5 =	177·5	= 59·87	..	59·70	59·85	59·96	59·75	59·82
O_2 =	32	= 10·79	..	..	..	..	..	..
	296·5							

V. was prepared by the action of potassium chlorate and hydrochloric acid, I., II., III., and IV. by means of chlorine hydrate. The substance was dried *in vacuo*.

Trichlororcin, $C_7H_5Cl_3O_2$.—When pentachlororcin was heated with hydriodic acid to 100° it was decomposed, trichlororcin being formed and iodine set free. The best method of conducting the operation, so as to obtain a pure product, was to add the pentachlororcin in small portions to a mixture of amorphous phosphorus and hydriodic acid, containing eight or ten per cent. of iodine, and to digest between each addition until

the liberated iodine had been reconverted into hydriodic acid. When all the pentachlororcin had been introduced, the digestion was continued until the trichlororcin appeared as a colourless oily layer at the bottom of the flask. On cooling, a considerable portion more crystallized out of the hydriodic acid in colourless needles. The oily trichlororcin, which solidified on cooling, was dissolved in a small quantity of spirit, filtered to separate it from the excess of amorphous phosphorus, and the alcoholic solution precipitated by water. The amount of crude product thus obtained was about 75 per cent. of the weight of the pentachlororcin originally employed. One or two crystallizations from boiling water slightly acidified with acetic acid rendered it quite pure. If very pure pentachlororcin had not been employed in the preparation the crude product was more or less coloured, and could only be purified with considerable difficulty, as the colouring-matter adheres to the trichlororcin with great obstinacy. It was found, however, that several alternate crystallizations from benzol and petroleum-oil, and a final crystallization from water, almost entirely removed the brown colouring-matter. It fuses to an oil under boiling water, in which it is moderately soluble, crystallizing out almost completely on cooling in long colourless transparent needles, which become white and opaque when exposed to the air. It is but sparingly soluble in carbon disulphide, moderately in petroleum-oil, rather more so in benzol, and excessively soluble in ether and alcohol. It is soluble in boiling glacial acetic acid, and crystallizes out on cooling in thin transparent plates, which become white and opaque on the addition of water. Trichlororcin melts at 123°, and when heated to a very much higher temperature it blackens and gives off hydrochloric acid, even under diminished pressure. It cannot, therefore, be distilled *in vacuo*, but it passes over readily with the vapour of water. When heated for several hours to 180° with moderately strong hydriodic acid and phosphorus, it was found to be reconverted into orcin, which could be extracted from the solution by agitating it with ether. The addition of alcoholic ammonia to a solution of the trichlororcin in alcohol threw down a white crystalline precipitate, but slightly soluble in water or alcohol. This compound, when dissolved in a large excess of dilute aqueous ammonia, and submitted to the action of metallic zinc in a close vessel at the ordinary temperature, yielded a colourless solution. On exposing this to the air, however, it acquired a fine blue colour, which was changed to red by the action of acids. Further investigation will no doubt show the nature of this compound.

Analysis of Trichlororcin.

I. ·257 grm. substance gave ·486 grm. argentic chloride.

II. ·302 grm. substance gave ·572 grm. argentic chloride.

III. ·216 grm. substance gave ·293 grm. carbonic anhydride and ·046 grm. water.

	Theory.		I.	II.	III.	Mean.
C_7 =	84	=36·93	..	..	37·00	37·00
H_5 =	5	= 2·20	..	..	2·37	2·37
Cl_3 =	106·5	=46·81	46·78	46·86	..	46·82
O_2 =	32	=14·06	..	..	..	..
	227·5	100·00				

I. was from the pentachlororcin prepared by the action of chlorine hydrate on orcin; II. and III. from that prepared by hydrochloric acid and potassium chlorate. The substance was dried *in vacuo.*

This trichlororcin differs considerably in its melting-point and other properties from that described by De Luynes * as formed by the action of hydrochloric acid and potassium chlorate on orcin. Moreover, although many attempts were made to prepare trichlororcin by his method with varying proportions of the materials, pentachlororcin was invariably obtained, contaminated, however, in some instances, with a large amount of viscid oily impurities. I conclude, therefore, that De Luynes's trichlororcin was either isomeric with that above described, or that it was an impure substance.

Bromorcin.

Pentabromorcin, $C_7 H_3 Br_5 O_2$.—This compound was readily formed by the action of an excess of bromine on orcin. Seven parts of bromine and about 200 of water were placed in a stoppered bottle, and a moderately strong aqueous solution of one part of orcin added by small portions, with constant agitation. The yellow crystalline product thus obtained, amounting to 370 per cent. of the weight of the orcin, was then collected, and purified by repeated crystallization from carbon disulphide, in a manner similar to the corresponding chlorine compound. It is almost insoluble in water, very soluble in alcohol and ether, and moderately so in benzol and carbon disulphide, from the latter of which it may be obtained in very large and almost colourless transparent crystals. It melts at 126°, and when boiled with water appears to undergo a decomposition similar to the pentachlororcin. Heated with concentrated sulphuric acid it gradually dissolves, and on continuing the heat hydrobromic acid and bromine are evolved in abundance. Like the corresponding chlorine compound, it dissolves in hot nitric acid, crystallizing out again on cooling, but is at the same time far more readily decomposed. When boiled with amorphous phosphorus and moderately strong hydriodic acid it is rapidly decomposed, and passes into solution probably as orcin. With very weak hydriodic acid, however, containing about one per cent. of iodine and excess of amorphous phosphorus, the decomposition takes place more gradually, and a heavy oily layer is obtained, which solidifies on cooling. This, after purification, was

* Ann. Chem. Pharm. vol. cxxx. p. 34; Kekulé's 'Benzolderivate,' vol. i. p. 388.

found to be identical with the tribromorcin described by Stenhouse *, and subsequently examined by Laurent and Gerhardt †, and by Lamparter ‡.

Analysis of Pentabromorcin.

I. ·578 substance gave 1·046 argentic bromide, which is equivalent to 77·00 per cent. bromine, the formula $C_7H_3Br_5O_2$ requiring 77·07 per cent.

Pentachlororcin hypochlorite, $C_7H_4Cl_6O_3 = C_7H_3Cl_5O_2, HClO$.—A white crystalline substance was obtained in endeavouring to prepare pentachlororcin by the action of calcium hypochlorite and hydrochloric acid on orcin; but as it appeared to differ somewhat from that compound it was carefully examined. After some trials the following was found to be the best method of preparation:—Five parts of orcin were dissolved in a mixture of four measures of hydrochloric acid, sp. gr. 1·17, with four measures of water; and this solution was then gradually added to a moderately strong clear solution of calcium hypochlorite, taking care to leave the latter in excess. The liquid, which has a milky appearance, was then strongly acidulated with hydrochloric acid, and allowed to stand 24 hours, when it deposited a considerable quantity of white crystals. These were collected, dried, and purified by crystallization from benzol, as, unlike pentachlororcin, they are but sparingly soluble in carbon disulphide. When pure it crystallizes in transparent prisms of great dispersive power. It is very soluble in ether, moderately so in light petroleum-oil, and almost insoluble in water. When heated with hydriodic acid and phosphorus it is decomposed, but apparently without the formation of trichlororcin. It is very soluble in alcohol, and after boiling the solution for some time the addition of water causes the precipitation of an oil, which solidifies if exposed to the air for a few days. When the substance is boiled with water the latter becomes milky, and gives off pungent vapours. The hypochlorite dissolves in hot nitric acid, and crystallizes out on cooling; hot concentrated sulphuric acid decomposes it. It has a considerably higher melting-point than pentachlororcin, namely 140°·5. The following results were obtained by analysis:—

I. ·272 grm. substance gave ·240 grm. carbonic anhydride and ·033 grm. water.

II. ·274 grm. substance gave ·242 grm. carbonic anhydride and ·032 grm. water.

III. ·257 grm. substance gave ·633 grm. argentic bromide.

IV. ·147 grm. substance gave ·361 grm. argentic bromide.

* Phil. Trans. 1848, p. 87.

† Ann. Chim. Phys. [3] vol. xxiv. p. 317.

‡ Ann. Chem. Pharm. vol. cxxxiv. p. 257.

	Theory.		I.	II.	III.	IV.	Mean.
C_7 =	84 =	24·07	24·06	24·08	..	..	24·07
H_4 =	4 =	1·15	1·31	1·34	..	..	1·32
Cl_6 =	213 =	61·03	..	..	60·94	60·75	60·85
O_3 =	48 =	13·75	..	..	..	..	..
	349	100·00					

The rational formula deduced from the percentage composition of the substance is $C_7 H_4 Cl_6 O_3$; and I propose to give it the provisional name of *pentachlororcin hypochlorite*, $C_7 H_3 Cl_5 O_2$, $H Cl O$, until its constitution is more satisfactorily made out by an examination of the products of its decomposition.

Chlorresorcin.

Pentachlorresorcin, $C_6 H Cl_5 O_2$.—An attempt was made to obtain a chlorresorcin by submitting resorcin to the action of chlorine hydrate in a manner similar to that which had been so successfully employed in preparing pentachlororcin; but the results obtained were unsatisfactory, the product being very small in comparison to the resorcin taken, and even that so contaminated with oily impurities that further examination was deemed inexpedient. The action of potassium chlorate and hydrochloric acid, however, gave a much more favourable result. Five parts of potassium chlorate and a solution of two parts of resorcin in eight of hydrochloric acid were gradually added to forty parts of hydrochloric acid, which was prevented from becoming very hot by immersion in cold water. The operation was conducted in a manner similar to that previously described in the preparation of pentachlororcin; but more care was required to obtain a successful result, and the amount of product was comparatively small, about 70 per cent. of the resorcin. The crystalline compound which is deposited on standing was collected, and, after being simply pressed to remove some of the mother-liquor, boiled with a considerable quantity of carbon disulphide. The supernatant aqueous layer was then separated by means of a separating-funnel, and the greater portion of the carbon disulphide removed by distillation. When sufficiently concentrated the anhydrous chlorresorcin crystallized out in brilliant colourless plates or flattened prisms. One or two recrystallizations rendered it pure. The crude product of the action of the chlorate and hydrochloric acid on resorcin appeared to consist principally of a hydrate of the chlorresorcin, as when it was rapidly heated for a short time with a comparatively small proportion of carbon disulphide, and the solution filtered, it deposited a white crystalline compound in minute scales, sparingly soluble in the disulphide. On submitting the solution to distillation, however, to remove the excess of carbon disulphide, water passed over with the latter, and the solution, when sufficiently concentrated, deposited large crystals of the anhydrous substance. Pure pentachlorresorcin is colourless, and melts at 92°·5. It is moderately soluble in warm water,

from which it separates, on cooling, in a white opaque mass of indistinct crystals, apparently the hydrate. It is readily soluble in carbon disulphide, benzol, and petroleum-oil, and very soluble in alcohol and ether.

Analysis of the Chlorresorcin.

I. ·476 grm. substance gave ·447 grm. carbonic anhydride and ·020 grm. water.

II. ·308 grm. substance gave ·289 grm. carbonic anhydride and ·013 grm. water.

III. ·193 grm. substance gave ·490 grm. argentic chloride.

IV. ·259 grm. substance gave ·659 grm. argentic chloride.

	Theory.		I.	II.	III.	IV.	Mean.
C_6	= 72	= 25·49	25·56	25·60	..	..	25·58
H	= 1	= 0·35	0·47	0·47	..	..	0·47
Cl_5	= 177·5	= 62·84	..	..	62·81	62·95	62·88
O_2	= 32	= 11·32	..	..	..	..	..
	282·5	100·00					

The results of the analysis correspond very nearly to the formula $C_6HCl_5O_2$, that of pentachlorresorcin. This somewhat anomalous composition was, however, confirmed by the analyses of the corresponding bromine compound.

Bromresorcin.

Pentabromresorcin, $C_6HBr_5O_2$.—This compound was prepared by adding resorcin solution to a mixture of bromine and water, in a manner similar to that employed in the preparation of pentabromorcin. It is, however, advantageous to use considerably less water (about one fifth), and to moderate the heat produced during the reaction by occasionally immersing the bottle in cold water. Two or three crystallizations from carbon disulphide serve to purify the product, which then forms large colourless or faintly yellow prismatic crystals. The pure pentabromresorcin melts at 113°·5. It is almost insoluble in water, but readily soluble in ether and alcohol, from the latter of which it is precipitated on the addition of water. It is also moderately soluble in cold benzol and in hot petroleum-oil, from which it crystallizes out in great part on cooling. Treated with hydriodic acid it yields a colourless compound, crystallizing in needles, probably tribromresorcin.

Analysis of Pentabromresorcin.

I. ·382 grm. substance gave ·712 grm. argentic bromide.

II. ·292 grm. substance gave ·544 grm. argentic bromide.

Theory.			I.	II.	Mean.
C_6 =	72 =	14·25	..	..	..
H =	1 =	0·20	..	..	..
Br_5 =	400 =	79·21	79·30	79·27	79·28
O_2 =	32 =	6·34	..	..	..
	505	100·00			

The analyses of those chlorine and bromine derivatives of resorcin that have just been described thoroughly establish the existence of the compounds $C_6 H Cl_5 O_2$ and $C_6 H Br_5 O_2$, which closely resemble in their properties the corresponding pentachlororcin, $C_7 H_3 Cl_5 O_2$, and pentabromorcin, $C_7 H_3 Br_5 O_2$, obtained from ordinary orcin. The view of the constitution of the orcins put forth by Kekulé, who regards them as dihydroxyl derivatives of the benzols, is scarcely in accordance with the method of formation and composition of these compounds, as in the case of the pentabromresorcin five hydrogen atoms in the resorcin are undoubtedly directly replaced by bromine, although one of them, according to Kekulé's view, exists as hydroxyl.

An attempt was made to prepare the resorcin body corresponding to that obtained from orcin by the action of calcium hypochlorite and hydrochloric acid, and which I have designated pentachlororcin hypochlorite, but without success. The product was a very viscid oil, which showed no signs of solidification even after standing for some weeks, and from which I was unable to obtain any crystalline compound.

II. "Note on Fucusol." By John Stenhouse, LL.D., F.R.S. Received October 11, 1871.

In a paper which I communicated to the Royal Society of London in 1850 *, "On the Oils produced by the Action of Sulphuric Acid upon various Classes of Vegetables," after describing the sources, method of preparation, and characteristic properties of furfurol and its educts, I described another isomeric substance closely resembling furfurol, both in its physical and chemical properties, and which I named fucusol from the source whence it had been obtained, namely, *Fucus nodosus, F. vesiculosus, F. serratus*, &c.

The nature of the substance which yielded furfurol was involved in considerable obscurity until the publication of Gudkow's paper "On the Furfurol-yielding Substance in Bran" †, which he found to be present in it to the amount of from 15 to 20 per cent., and which, when boiled with dilute sulphuric acid, was converted into a brownish sweet syrup. This is the substance from which furfurol is obtained by distillation with sulphuric acid or hydrochloric acid.

* Phil. Trans. 1850, p. 467.

† Zeits. Chem. 1870, p. 360.

Fucusol.

I have found that when seaweeds were boiled with very dilute sulphuric acid, containing about 3 per cent. of acid, that a substance corresponding to that described by Gudkow was obtained, which, when distilled with sulphuric or hydrochloric acid, yielded fucusol. In my paper of the year 1850 I have described the difference in the physical properties of furfurol and fucusol, and also the difference between the products obtained by the action of ammonia upon them and the bases derived from these. I have again repeated the examination of these substances with great care, and find my former statements to be correct. In addition to these, I have recently prepared the aniline compound analogous to furfuraniline *. *Fucusaniline* hydrochlorate crystallizes in needles of a magnificent purple colour, and closely resembles the corresponding furfurol compound.

As is well known †, when furfurol is boiled with water and silver oxide, metallic silver is deposited and silver pyromucate formed, the latter remaining in solution. In a similar manner I found that when fucusol was digested at 100° C. for five or six hours with an excess of recently precipitated silver oxide and a considerable quantity of water, its odour gradually disappeared, and at the completion of the reaction a silver-salt was found in solution, whilst metallic silver was deposited, partly in a granular state and partly as a mirror, on the bottom of the flask. Sufficient hydrochloric acid was then added to the hot filtered solution to precipitate the silver. After the removal of the argentic chloride it was carefully evaporated at a temperature below 100° and the brown semicrystalline mass thus obtained boiled with light petroleum-oil, which dissolved the acid and left the colouring-matter. One or two crystallizations from boiling water rendered it quite pure.

Analysis.

·188 grm. substance gave ·370 grm. carbonic anhydride and ·080 grm. water.

		Theory.	Found.
C_5	60	53·56	53·68
H_4	4	3·57	4·25
O_3	48	42·87	..
	112	100·00	

From this analysis, it will be seen that this acid, which I propose to call *β pyromucic acid*, is isomeric with ordinary pyromucic acid, from which, however, it slightly differs in its physical properties. The melting-point of pure *β* pyromucic acid is 130°, being nearly the same as that of the acid obtained from furfurol, which I found to be 133°, and which

* Proc. Roy. Soc. vol. xviii. p. 537. † Ann. Chem. Pharm. vol. cxvi. p. 259.

Schwanert * gives as 134°·3. The β pyromucic acid from fucusol crystallizes from its aqueous solution in small rhomboidal plates, whilst the acid which I had prepared from furfurol crystallized in flat needles.

Silver β pyromucate.—This compound was prepared from the pure β acid by boiling it for a short time with silver oxide and a sufficient quantity of water, filtering, and setting aside to crystallize. A single recrystallization from boiling water, in which it is only moderately soluble, rendered it quite pure. On cooling the hot aqueous solution, the silver β pyromucate is obtained in long flat needles, whilst the corresponding salt of the ordinary acid forms small crystalline scales: ·505 grm. of silver-salt gave ·330 grm. argentic chloride, which corresponds to 49·18 per cent. of metallic silver; the formula $C_5 H_3 AgO_3$ requires 49·32 per cent.

From this silver determination it will be seen that this compound is isomeric with the ordinary silver α pyromucate, $C_5 H_3 AgO_3$.

III. "On some recent Researches in Solar Physics, and a Law regulating the time of duration of the Sun-spot Period." By Warren De La Rue, D.C.L., F.R.S., Balfour Stewart, F.R.S., and Benjamin Loewy, F.R.A.S. Received October 12, 1871.

1. In the short account of some recent investigations by Professor Wolf and M. Fritz on sun-spot phenomena, which has been published lately in the 'Proceedings of the Royal Society' (1871, vol. xix. p. 392), it was pointed out that some of Wolf's conclusions were not quite borne out by the results which we have given in our last paper on Solar Physics in the Philosophical Transactions for 1870, pp. 389–496. A closer inquiry into the cause of this discrepancy has led us to what appears a definite law, connecting numerically the two branches of the periodic sun-spot curve, viz. the time during which there is a regular diminution of spot-production, and the time during which there is a constant increase.

It will be well, for the sake of clearness, to allude here again, as briefly as possible, to Professor Wolf's results before stating those at which we have arrived.

2. Professor Wolf had previously devoted the greater part of his laborious researches to a precise determination of the mean *length* of the whole sun-spot period, but latterly he has justly recognized the importance of obtaining some knowledge of the average character of the periodic increase and decrease. Hence he has, as far as he has been able to do so by existing series of observations, and his peculiar and ingenious method of rendering observations made at different times and by different observers comparable with each other, endeavoured to investigate more closely the nature of the periodic sun-spot curve by tabulating and graphically representing the monthly means taken during two and a half years before and after the

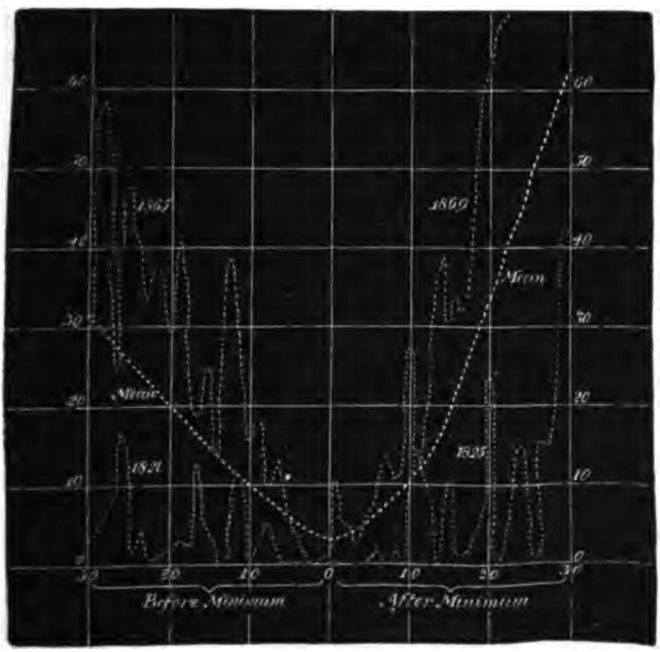
* Ann. Chem. Pharm. vol. cxvi. p. 257.

minimum, and applying this method to five distinct minimum epochs, which he has fixed for the following years:—

1823·2
1833·8
1844·0
1856·2
1867·2

3. In a Table he gives their mean numbers, expressing the solar activity, arranged in various columns, and arrives at the following results:—

(1) It is shown now, with greater precision than was previously possible, that the curve of sun-spots ascends with greater rapidity than it descends. This fact is shown in the subjoined diagram, which it may be of interest to compare with the curves given previously by ourselves in the above-mentioned place. The zero-point in this diagram corresponds to the minimum of each period; the abscissæ give the time before and after it, viz. two and a half years, or thirty months; the ordinates express the amount of spot-production in numbers of an arbitrary scale. The two finely dotted curves are intended to show the actual character of a portion of two periods only, viz. those which had their minima in 1823·2 and 1867·2; the strongly dotted curve, however, gives the mean of all periods (five) over which the investigation extends.



(2) Denoting by x the number of years during which the curve ascends

and presuming that the behaviour is approximately the same throughout the whole period of 11·1 years as during the five years investigated, we have the proportion

$$x : 11{\cdot}1 - x :: 1 : 2,$$

whence $$x = 3{\cdot}7,$$

or the average duration of an ascent is 3·7 years, that of a descent 7·4 years.

(3) The character of a single period may essentially differ from the mean; but on the whole it appears that a $\left\{\begin{matrix}\text{retarded}\\\text{accelerated}\end{matrix}\right\}$ descent corresponds to a $\left\{\begin{matrix}\text{retarded}\\\text{accelerated}\end{matrix}\right\}$ ascent. Thus the minimum of 1844·0 behaved very normally, but that of 1856·2, and still more that of 1823·2, shown in the above diagram, presents a retarded ascent and descent; on the other hand, in the minimum of 1833·8, and still more in that of 1867·2, also shown in the diagram, both ascent and descent are accelerated.

4. Finally, Professor Wolf arranged in the manner shown in the following Table the successive minima and maxima, in order to arrive at some generalization which might enable him to foretell the general character and length of a future period. Taking the absolute differences in time of every two successive maxima, and the mean differences of every two alternating minima, he shows that the greatest acceleration of both maximum and minimum happens together. This result strengthens our own conclusions, to be immediately stated, by new evidence, as it is derived from observations antecedent to the time over which our researches extend.

Minima.	Differences of alternating Minima.	Means.	Maxima.	Differences of successive Maxima.
1810·5				
			1816·8	
1823·2	23·3	11·65		12·7
			1829·5	
1833·8	20·8	10·4		7·7
			1837·2	
1844·0	22·4	11·2		11·4
			1846·6	
1856·2	23·2	11·6		11·6
			1860·2	
1867·2				

From this Professor Wolf predicts for the present period a very accelerated maximum—a prediction which seems likely to be fulfilled.

5. Comparing, now, M. Wolf's results with our own, it must not be overlooked, in judging of the agreement or discrepancy of these two independently obtained sets, that our facts have been derived from the actual measurement and subsequent calculation of the spotted area from day to day since 1833 recorded by Schwabe, Carrington, and the Kew solar photograms, which measurements are expressed as millionths of the sun's

visible hemisphere, while the conclusions of M. Wolf are founded on certain "relative numbers," which give the amount of observed spots on an arbitrary scale, chiefly designed to make observations made at different times and by various observers comparable with each other. This will obviously, in addition to the sources of error to which our own method is liable, introduce an amount of uncertainty arising from errors of estimation and the possibility of using for a whole series an erroneous factor of reduction. Nevertheless we shall find a very close agreement in various important results; and this seems a sufficient proof of the great value and reliability of M. Wolf's "relative numbers," especially for times previous to the commencement of regular sun observations.

6. The following is a comparison of the data of periodic epochs, as fixed by ourselves and M. Wolf:—

		I.	II.	III.	IV.
Minima epochs.	De La Rue, Stewart, and Loewy . .	1833·92	1843·75	1856·31	1867·12
	Rudolf Wolf . .	1833·8	1844·0	1856·2	1867·2

		I.	II.	III.
Maxima epochs.	De La Rue, Stewart, and Loewy	1836·98	1847·87	1859·69
	Rudolf Wolf	1837·2	1846·6	1860·2

It will be seen from this comparison that only one appreciable difference occurs, viz. in the maximum of 1847, which M. Wolf fixes nearly one and a quarter years before our date.

The mean length of a period is found by us to be 11·07 years, which agrees very well with M. Wolf's value, viz. 11·1 years.

7. We found the following times for the duration of increase of spots during the three periods, and for the corresponding decrease, or for ascent and descent of the graphic curve, beginning with the minimum of 1833:—

	Time of ascent.	Time of descent.
I.	3·06 years.	6·77 years.
II.	4·12 "	8·44 "
III.	3·37 "	7·43 "
Mean. . . .	3·52 "	7·55 "

Professor Wolf gives 3·7 years and 7·4 years for the ascent and descent respectively; and considering that he derived these numbers only from an investigation of a portion of each period, the agreement is indeed surprising, and would by itself suggest that the times of ascent and descent are connected by a definite law.

8. M. Wolf has expressed in general terms the following law with reference to this relation of increase and decrease of spots:—

"The character of a single period may essentially differ from the mean behaviour; but on the whole it appears that a $\left\{\begin{array}{c}\text{retarded}\\\text{accelerated}\end{array}\right\}$ descent corresponds to a $\left\{\begin{array}{c}\text{retarded}\\\text{accelerated}\end{array}\right\}$ ascent."

We, on the other hand, have, by an inspection of our curves (*vide* Phil. Trans. 1870, p. 393), been induced to make the following remark on the same question:—

"We see that the second curve, which was longer in period as a whole than either of the other two, manifests this excess in each of its branches, that is to say, its left or ascending branch is larger as a whole than the same branch of the other two curves, and the same takes place for the second or descending branch. On the other hand, the maximum of this curve is not so high as that of either of the other two; in fact the curve has the appearance as if it were pressed down from above, and pressed out laterally so as to lose in elevation what it gains in time."

Although both statements appear to lead up to the same conclusion, viz. that ascent and descent are connected by a law, still they differ essentially in this respect, that if A, B, C represent the three following consecutive events, descent, ascent, descent, Professor Wolf's law refers to the connexion between A and B, while our remark refers to B and C. We consider two successive minima as the beginning and end of a single period, while M. Wolf, at least in this particular research, places the minimum within the period, and compares the descent from the preceding maximum with the ascent to the next one.

9. We have considered the connexion thus indicated of sufficient importance to apply to it the following test. If, using the previous notation, a definite relation exists between A and B, the *ratio* of the times which the events occupy in every epoch ought to be approximately constant; similarly with respect to B and C; and this ratio should not be influenced by the *absolute* duration of the two successive events. It is clear that the greater uniformity of these ratios will be a test for their interdependence. The following is the result of the comparison:—

a. Professor Wolf's law: comparison of A and B.

	Periods.	Duration of descent (A).	Periods.	Duration of ascent (B).
I.	1829·5 to 1833·8	4·3 years	1833·8 to 1837·2	3·4 years.
II.	1837·2 to 1844·0	6·8 „	1844·0 to 1846·6	2·6 „
III.	1846·6 to 1856·2	9·6 „	1856·2 to 1860·2	4·0 „

	Ratio $\frac{A}{B}$.		Difference from mean.
I.......	1·265	Mean 2·093	−0·728.
II.......	2·615		+0·522.
III.......	2·400		+0·307.

These differences from the mean are so considerable, that in the present state of the inquiry a connexion between any descent and the immediately *succeeding* ascent appears highly improbable. A very new and apparently important relation seems, however, to result from a similar comparison of any ascent and the immediately succeeding descent, or between B and C.

b. Comparison of B and C.

	Periods.	Duration of ascent (B).	Periods.	Duration of descent (C).
I.	1833·92 to 1836·98	3·06 years	1836·98 to 1843·75	6·77 years.
II.	1843·75 to 1847·87	4·12 „	1847·87 to 1856·31	8·44 „
III.	1856·31 to 1859·69	3·38 „	1859·69 to 1867·12	7·43 „

	Ratio $\frac{C}{B}$.		Difference from mean.
I.......	2·212	Mean 2·151	+0·061.
II.......	2·044		−0·107.
III.......	2·198		+0·047.

The agreement of these ratios with each other, and the small differences from the mean of the single ratios, justify us in the mean time, until a greater number of periods are before us, to state the connexion between the two branches of the periodic curve from one minimum to another in the following more precise terms:—

If T *be the time of duration of sun-spot increase from a minimum to a maximum, then* 2·15 × T (*with a probable error of less than* ±0·05) *will be the duration of the sun-spot decrease until the next minimum.*

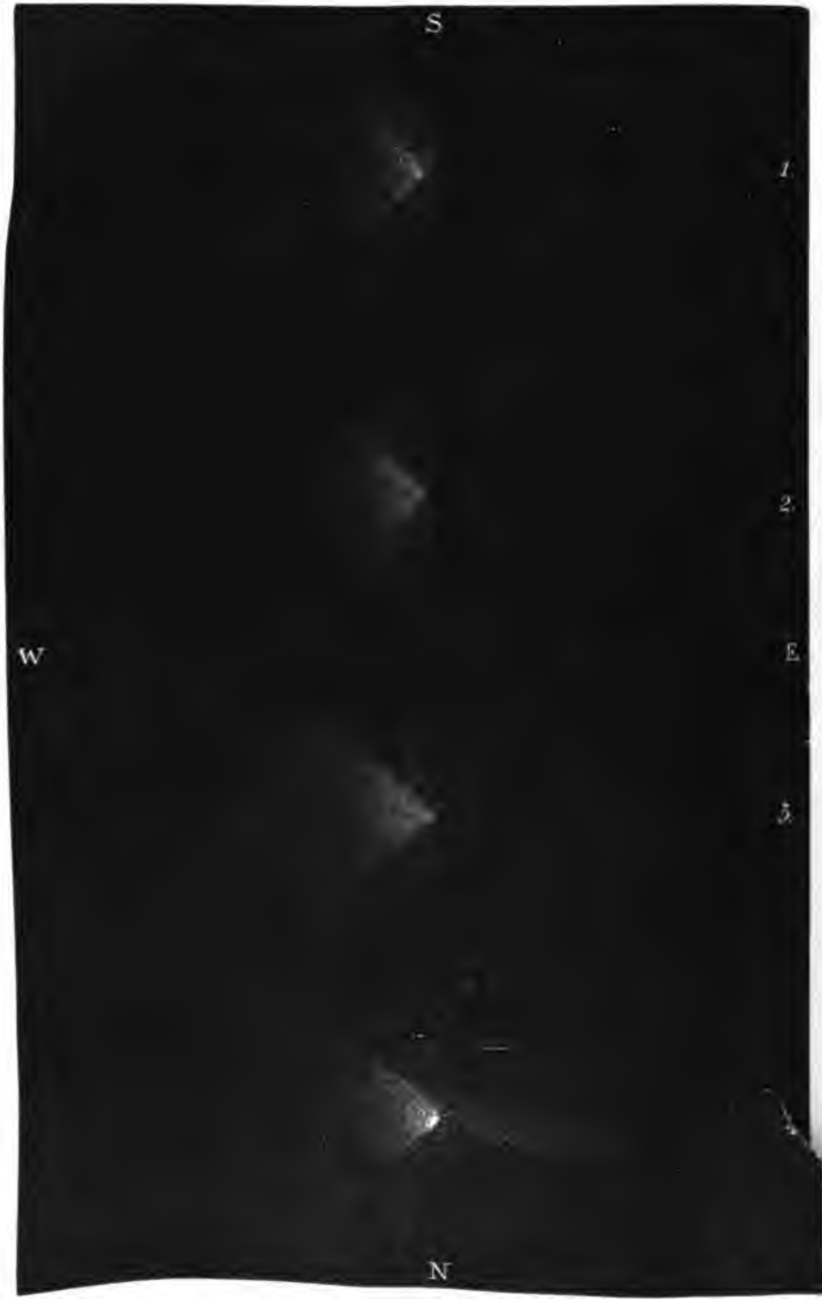
This law, together with the fact which we have previously established, that a longer period shows generally a depressed curve, while a shorter is characterized by great peaks, points strongly to the conclusion that *the energy of the ultimate causes of sun-spot production, whether these causes be intrasolar or extrasolar, is for every period constant.*

IV. "Note on the Telescopic Appearance of Encke's Comet." By William Huggins, D.C.L., LL.D., F.R.S. Received December 16, 1871.

The first three figures which accompany this note represent the comet on evenings on which its appearance was described in a note on the spectrum of the comet which I had the honour to present to the Royal Society*. A continuance of bad weather prevented me from making later observations of the comet, with the exception of one evening, December 5, when figure 4 was obtained under unfavourable circumstances.

Fig. 1. November 7, 7.30 P.M.—From Oct. 17, when the comet consisted of a nearly round nebulosity without condensation in any part, to Nov. 7 no observations could be obtained. At the latter date, the remarkable fan-form which distinguishes this appearance of the comet was already distinctly presented. The faint light by which the comet was surrounded terminated on the side from the sun, that from which the tail is usually projected, in a straight boundary at right angles to the longer axis of the comet. At the opposite side, that towards the sun, the faint nebulosity expanded and became fainter until it could be no longer traced. The

* *Suprà*, p. 45.


S
1.
2.
W
E
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N

minute stellar nucleus which was suspected at the eastern extremity of the fan is not marked in the figure.

Fig. 2. November 8, 7 P.M.—The fan was now brighter and more defined in form. The nucleus, as a minute bright point, appeared to be situated not at the extreme western point, but a little within it, towards the north.

The sides of the fan were slightly curved, suggesting an approach to a parabolic form.

The fan was brighter on the southern side. The eastern edge of the faint light by which the comet was surrounded still preserved a right line from north to south.

Fig. 3. November 14, 6.40 P.M.—The appearance of the comet was essentially the same as on Nov. 8.

The bounding lines of the fan were perhaps less curved; they enclosed an angle of from 85° to 90°.

The nucleus had become brighter, and now appeared to form the extreme eastern point of the fan.

No prolongation of the eastern boundary, where the tail is usually formed, was seen.

Fig. 4. December 5, 5.30 P.M.—Thin mist in the atmosphere allowed the brighter parts only of the comet to be satisfactorily observed.

The condensation of light was now much stronger at the eastern end, but a defined nucleus was not detected.

The fan form was less marked; the brighter part of the comet more resembled a brush-like flame.

The atmospheric haze nearly concealed the faint light surrounding the comet, but, by glimpses, a tail was now seen to project towards the east; it was traced to a distance of about twice the length of the bright brush.

The tail appeared to come from the northern side of the longer axis of the comet, and to consist of a faint ray with sides nearly parallel.

As I am at present without a suitable micrometer, I was not able to take measures of the comet.

The Society then adjourned over the Christmas Recess to Thursday, January 11, 1872.

January 11, 1872.

GEORGE BIDDELL AIRY, C.B., President, in the Chair.

The following communications were read:—

I. "Experiments made to determine Surface-conductivity for Heat in Absolute Measure." By Donald M'Farlane. Communicated by Sir William Thomson, F.R.S. Received August 8, 1871.

The experiments described in this paper were made in the Physical Laboratory of the University of Glasgow, under the direction of Sir William Thomson, during the summer of 1871. A set of similar experiments were made in 1865; but being merely preliminary, carried on by different individuals, and embracing only a limited range of temperatures, it is thought unnecessary to allude further to them here*.

A copper ball, 2 centimetres radius, having a thermo-electric junction at its centre, was suspended in the interior of a double-walled tin-plate vessel which had the space between the double sides filled with water at the atmospheric temperature, and the interior coated with lamp-black. The other junction was in metallic contact with the outside of the vessel, and the circuit was completed through the coil of a mirror galvanometer. One junction was thus kept at a nearly constant temperature of about 14° Cent., while the other had the gradually diminishing temperature of the ball.

Having adjusted the galvanometer to the degree of sensitiveness desired, the copper ball was heated in the flame of a spirit-lamp till its temperature was considerably above that required to throw the spot of light off the scale; it was then put into position in the interior of the tin-plate vessel, and as soon as the spot of light came within range, the deflections from the zero position were noted at intervals of one minute exactly till the change of deflection was reduced to about two scale-divisions per minute.

Two series of experiments were made in this way, each consisting of several sets of readings. In the first the ball had a bright surface, and in the second it was coated with a thin covering of soot from the flame of a lamp, and in both the air was kept moist by a saucer containing a quantity of water placed in the interior of the tin-plate vessel.

As the range of differences of temperatures of the junctions extended over 50° Cent., the change in the difference of thermo-electric qualities of the copper and iron wires forming the junctions was very considerable,

* These experiments consisted of two series, one with the air moist by a little water placed in the interior of the vessel, the other having the air dried by substituting sulphuric acid for the water in the first; and the results in the two cases were so nearly alike, that any effect due to the moisture or dryness of the air could not be distinguished from errors of observation. From this circumstance, as well as the limited range of temperatures, these results are not given here.

and it was necessary to make a careful thermometric comparison of the temperatures of the junctions and galvanometer deflections. For this purpose the junctions were tied to the bulbs of two previously compared thermometers, having their stems divided to tenths of a degree Cent.; these were then placed in two vessels of water, one at the temperature of the air, and the other heated by small additions of hot water, and kept well stirred; simultaneous readings of the thermometers and galvanometer deflections were then taken at various points of the scale*, from which the formula

$$y=0^{\circ}{\cdot}0924+0^{\circ}{\cdot}0000227x$$

was obtained, where y is the value of a scale-division in terms of a degree Centigrade, and x the galvanometer deflection; and the difference of temperature of the junctions is therefore

$$xy=0^{\circ}{\cdot}0924x+0^{\circ}{\cdot}0000227x^2,$$

from which the numbers in col. II. of the following Tables were calculated.

The method adopted in reducing the observations was this:—Each single set of readings was arranged in a vertical column, and the whole series placed side by side with corresponding numbers in the same horizontal line; the means of the horizontal lines were formed into a similar column, and divided into groups, each consisting of four consecutive numbers, and the means of these groups form the numbers in col. I. of the Tables.

Col. II. contains the differences of the temperatures of the junctions at intervals of four minutes, corresponding to the mean deflections in col. I.

Col. III. contains the common logarithms of the numbers in col. II.

Col. IV. contains the differences of the successive numbers in col. III. divided by 4.

Col. V. is formed from col. IV., by multiplying by the Napierian logarithm of 10, and is the rate at which the difference of temperature varies per minute.

Col. VI. shows the quantity of heat emitted from the ball in gramme-water units per square centimetre per second per degree of difference of temperatures, and is formed by multiplying the numbers in col. V. by ·009385†, a constant depending on the surface of the ball and its capacity for heat.

* These readings were plotted, and the curve drawn through the points agreed very closely with a portion of a parabolic curve whose equation is

$$y=2{\cdot}4+10{\cdot}6x-{\cdot}019x^2,$$

y denoting the deflections of the galvanometer, and x the difference of temperature; y is a maximum when $x=\frac{10{\cdot}6}{{\cdot}038}=279^{\circ}$, and, the colder junction having been at 16° Cent., we get 295° as the neutral point of the specimens of copper and iron wires used—a very close agreement with former observations, considering the great distance of the neutral point from the temperature of the observations.

† The surface of the ball was 50·26 sq. centimetres, and its capacity for heat 28·31

The numbers found in cols. VI. and VII. were plotted on squared paper, and a mean curve drawn through the points; and, assuming the quantity of heat emitted to be represented by the formula $x=a+bt+ct^2$, where t is the difference of temperature, the coordinates of the curve were employed to determine a, b, and c; and col. VIII., calculated by the formula, is added to show the degree of approximation to which the results of the experiment are represented by it.

First Series.

Atmosphere moist. Copper Ball polished bright.
Means of nine sets of Observations.

I. Mean deflections of nine sets of observations.	II. Difference of temperature, D.	III. $\log_{10}$ D.	IV. $\frac{\log_{10} D' - \log_{10} D''}{4}$	V. $\frac{\log_e D' - \log_e D''}{4}$ C.	VI. Heat emitted per second by observation.	VII. Mean difference of temperature, $\frac{1}{2}[D'-D'']$, (t).	VIII. Heat emitt per second calculated formula, (x).
627·19	66°·88	·82527	·01037	·02387	·000223	63°·83	·000230
576·28	60·79	·78380	·01045	·02406	·000226	58·00	·000227
528·82	55·21	·74200	·01055	·02429	·000227	52·65	·000226
484·50	50·10	·69981	·01041	·02396	·000225	47·81	·000224
444·14	45·51	·65815	·01033	·02378	·000223	43·45	·000222
407·15	41·38	·61681	·01028	·02366	·000222	39·51	·000220
373·37	37·64	·57570	·01015	·02337	·000219	35·96	·000217
342·26	34·28	·53509	·01020	·02348	·000220	32·75	·000215
313·64	31·21	·49429	·00992	·02284	·000214	29·85	·000213
287·89	28·48	·45459	·00982	·02261	·000212	27·25	·000210
264·42	26·02	·41529	·00943	·02171	·000204	24·94	·000208
243·58	23·85	·37756	·00962	·02215	·000208	22·84	·000205
223·96	21·83	·33907	·00945	·02175	·000204	20·92	·000202
206·15	20·01	·30127	·00915	·02106	·000198	19·20	·000200
190·20	18·39	·26467	·00905	·02083	·000195	17·62	·000198
175·57	16·92	·22845	·00909	·02093	·000196	16·24	·000196
161·99	15·56	·19207	·00901	·02074	·000195	14·94	·000194
149·51	14·32	·15603					

Formula for calculating column VIII.:—

$$x = \cdot 000168 + \cdot 00000198t - \cdot 000000017t^2.$$

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gramme-water units. Let x denote the heat emitted per second, per sq. centimetre per degree of difference of temperature, and C the rate at which the difference of temperature varies per minute; then

$$\frac{x \times 60 \times 50\cdot 26}{28\cdot 31} = C,$$

and therefore

$$x = \cdot 009385\ C.$$

Second Series.

Atmosphere moist. Copper Ball blackened.
Ten sets of Observations.

I. Mean deflections of ten sets of observations.	II. Difference of temperature, D.	III. $\log_{10}$ D.	IV. $\frac{\log_{10} D' - \log_{10} D''}{4}$.	V. $\frac{\log_e D' - \log_e D''}{4}$, C.	VI. Heat emitted per second by observation.	VII. Mean difference of temperature, $\frac{1}{2}$[D'−D''], (t).	VIII. Heat emitted per second, calculated by formula, (x).
631·85	67°·44	·82893	·01511	·03478	·000326	63°·06	·000328
558·46	58·68	·76849	·01510	·03477	·000326	54·87	·000328
492·92	51·06	·70808	·01488	·03426	·000322	47·79	·000325
435·27	44·52	·64856	·01476	·03399	·000319	41·69	·000321
384·29	38·86	·58950	·01460	·03362	·000315	36·42	·000315
339·49	33·98	·53122	·01433	·03300	·000310	31·88	·000311
300·20	29·78	·47392	·01391	·03202	·000301	27·99	·000303
266·11	26·20	·41830	·01377	·03170	·000297	24·64	·000298
236·12	23·08	·36324	·01358	·03126	·000293	21·72	·000292
209·65	20·37	·30899	·01325	·03050	·000286	19·20	·000287
186·59	18·03	·25600	·01311	·03018	·000283	17·00	·000283
166·14	15·98	·20358	·01290	·02970	·000279	15·09	·000279
148·16	14·19	·15198	·01258	·02896	·000272	13·41	·000274
132·46	12·64	·10165	·01265	·02902	·000272	11·94	·000271
118·29	11·25	·05104	·01230	·02832	·000266	10·70	·000268
105·97	10·04	·00186	·01236	·02846	·000267	9·50	·000265
94·79	8·96	·95240	·01261	·02903	·000272	8·47	·000262
84·59	7·98	·90195					

Formula for calculating column VIII.:—

$$x = \cdot000238 + \cdot00000306t - \cdot000000026t^2.$$

The following Table gives the results calculated by the formula for every fifth degree within the limits of the experiments :—

Difference of temperature.	Heat emitted.		Ratio of emissive power of polished to that of blackened surface.
	Polished surface.	Blackened surface.	
5°	·000178	·000252	·707
10	·000186	·000266	·699
15	·000193	·000279	·692
20	·000201	·000289	·695
25	·000207	·000298	·694
30	·000212	·000306	·693
35	·000217	·000313	·693
40	·000220	·000319	·693
45	·000223	·000323	·690
50	·000225	·000326	·690
55	·000226	·000328	·690
60	·000226	·000328	·690

II. "The Myology of the Cheiroptera." By A. Macalister, A.B., M.B.T.C.D., Professor of Zoology, University of Dublin. Communicated by Dr. Sharpey, Sec. R.S. Received October 19, 1871.

(Abstract.)

This paper is a record of the structural details of nineteen species of Bats, and for purposes of comparison the author has appended a description of the muscles of the Flying Squirrel (*Pteromys*) and of the Flying Lemur (*Galeopithecus*). The species of Bats examined were the following:—*Pteropus edulis, medius, Edwardsii, Macroglossus minimus, Cephalotes Pallasii, Cynonycteris amplexicaudalis, Eleutherura marginata, Rhinolophus ferrum-equinum, speoris*, and *diadema, Megaderma lyra, Artibeus jamaicensis, Vampyrops vittatus, Vespertilio murinus, Vesperugo pipistrellus, Synotus barbastellus, Plecotus auritus, Noctulina altivolans*, and *Scotophilus hesperus*.

As the habits of the Bats are singularly different from those of the other mammals, the study of their myology becomes a matter of great interest; the special features displayed by their muscles are very numerous, but the principal of these may be tabulated as follows:—

1st. The singularly modified occipital trapezius.

2nd. The enormously developed and subdivided great pectoral.

3rd. The digastric being intersected by a linear inscription, forming a connecting link between the mammals with a single-bellied depressor of the mandible and those with a biventral muscle.

4th. The separate and displaced scapular deltoid.

5th. The palmaris longus acting as a superficial flexor.

6th. The displacements of the lower-extremity muscles consequent on the rotation of the lower limbs backward—such as the everted iliacus, the diminished glutei, and the weakness of the extensors of the knee.

7th. The increased size of the gracilis.

8th. The absence in general of the sartorius, tensor vaginæ femoris, biceps, plantaris, popliteus, and soleus.

It is interesting, in connexion with this last peculiarity, to notice the occurrence of a rudimental sartorius in one species and of a rudimental popliteus in another.

The cutaneous muscles are of very great interest, and this is increased by the comparison with those of the other flying mammals.

The author regards it as a point of very great importance that he has been able to apply the test of nerve-supply in the identification of some disputed muscles. Thus he has shown that the upper part at least of the occipito-pollicalis is of the nature of the trapezius, although its continuation is a cutaneous muscle; and this is interesting, as in the other flying mammals the entire of this muscle is cutaneous and springs from

the upper part of the platysma: he has also been able to show that the abdominal pectoral is not part of the pectoralis minor.

By dissecting a large number of species, the author has been able to correct a number of errors in the hitherto-published records of the myology of the Cheiroptera—such as the origin of the fourth pectoral, the insertion of the latissimus dorsi, the arrangement of the forearm-muscles, &c.

Although the general plan of the muscular system is the same in all the species, yet there are very many suggestive varieties; and from a comparison of their muscles, it would seem that each of the four great groups of Bats is characterized by a slightly different arrangement of muscles.

The author has, for purposes of brevity, carefully abstained from adding any thing of theoretical deduction to this paper, which he has endeavoured to confine to a simple statement of anatomical facts.

III. "Notice of further Researches on the Fossil Plants of the Coal-measures." By Dr. W. C. Williamson, F.R.S., in a Letter to Dr. Sharpey, Sec. R.S. Received November 17, 1871.

Owens College, Manchester, Nov. 16, 1871.

My dear Dr. Sharpey,—Since I read my last communication to the Royal Society on the organization of the Fossil Plants of the Coal-measures I have done a large amount of work, having cut between two and three hundred new sections and with most satisfactory results. I have obtained a series of specimens almost completing the life-history of one plant from Burntisland, beginning with the tips of the smallest twigs and ending with the large stems. The former are mere aggregations of parenchyma with a central bundle of barred vessels mixed with a small amount of primitive cell-tissue. As the twig grew the leaves assumed definite form, and the central vascular bundle opened out at its central part, so as to form a cylinder, the interior of which was occupied by parenchyma. This cylinder grew rapidly, the number of its vessels steadily increasing; but they were all equally arranged as in, what I have termed, the medullary vascular cylinder, i. e. *not* in radiating series. We thus obtain the origin of that remarkable cylinder, and see that it is the expanded homologue of the central vascular bundles of the living Lycopods. Whilst these processes were in progress the cortical portion became differentiated into layers, and the parenchymatous cells of the pith continued to multiply, so as to occupy the expanding interior of the vascular cylinder. After attaining a certain size, through the above processes, a new element of growth appeared; an *exogenous* addition was made to the exterior of the cylinder, also consisting of barred vessels, but these are arranged in the radiating series described in my last memoir. This series continued to grow until it attained to considerable dimensions; but the entire vascular system always remains small, compared with the diameter of the stem, the chief bulk of which consists of

an enormously thick bark. The structure just descibed is that of a true example of the genus *Diploxylon* of Corda. But I have got abundance of specimens with leaves on the exterior of the bark, demonstrating that the plant is a true *Lomatophloios*, thus indicating the correctness of my supposition, advanced in my last memoir, that sooner or later the genus *Diploxylon* would have to be abandoned.

As if to place beyond doubt the accuracy of these interpretations, I have now got magnificent specimens, apparently representative of a *cambium layer*, in which the half-grown vessels and the imperfectly formed medullary rays are exquisitely clear. In addition to these discoveries I have obtained a *Lepidostrobus*, which I have no doubt is the fruit of the above plant. It is provided with both microspores and macrospores, the exteriors of the latter being curiously furnished with numerous caudate prolongations, causing them to resemble some of the fossil *Xanthidia* of the chalk.

I have further obtained, both from Lancashire and Burntisland, beautiful stems of another type, and which I have no doubt belong to *Asterophyllites*. These began to grow, as before, with a central vascular bundle surrounded by a cylinder of parenchyma, but the transverse section of the bundle soon became *triquetrous* instead of circular. This, it may be remembered, is the characteristic of the corresponding bundle of the strobilus which I have just described in the 'Transactions of the Literary and Philosophical Society of Manchester,' under the name *Volkmannia Dawsoni*, and which I referred to *Asterophyllites*. This central triangular axis does not expand or become converted into a hollow cylinder; but vessels are at once added to each of its three sides, exogenously, and in radiating series, until it becomes converted into a cylindrical woody axis. I have specimens showing the nodes and internodes, leaving little, if any, room to doubt the close affinity between the plant in question and the verticillate-leaved *Asterophyllites*.

The details of these discoveries, along with those respecting a most remarkable series of Lycopodiaceous plants, to which I have given the name of *Dictyoxylon*, but which name will have to be abandoned for the late Mr. Gourlie's name of *Lyginodendron*, will be laid before the Royal Society with as little delay as possible. I may observe that the plants last referred to have developed, so far as type is concerned, in a way very similar to that of the *Lomatophloios*, allowance being made for generic and specific peculiarities.

I am, my dear Sir,
Very sincerely yours,
W. C. Williamson.

I ought not to close this letter without acknowledging the indefatigable energy of G. Grieve, Esq., of Burntisland, who has supplied me with a constant stream of specimens, upon which I have been able to operate, thus rendering an admirable service to the cause of palæophytology.

January 18, 1872.

GEORGE BIDDELL AIRY, C.B., President, in the Chair.

Pursuant to notice given at the last Meeting, Mr. Spottiswoode proposed and Admiral Richards seconded the Right Hon. George Joachim Goschen, First Lord of the Admiralty, for election and immediate ballot.

The ballot having been taken, Mr. Goschen was declared duly elected.

The following communication was read:—

'Investigations of the Currents in the Strait of Gibraltar, made in August 1871, by Captain G. S. Nares, R.N., of H.M.S. 'Shearwater,' under Instructions from Admiral Richards, F.R.S., Hydrographer of the Admiralty." Communicated by Admiral Richards. Received December 28, 1871.

In fulfilment of instructions to afford to Dr. Carpenter, F.R.S., who was attached to the 'Shearwater,' every opportunity to investigate the Gibraltar current, and to "use every effort to set the question at rest," Captain Nares spent six days, from August 22nd to the 30th, in examining the currents and taking soundings and observations of temperature in the Strait of Gibraltar. The details and results, embodied in a report, were delivered to the Admiralty, and, by favour of Admiral Richards, have furnished material for the following summary.

While the observations were being made, east winds, force 0 to 6, were experienced: they were reported as having also prevailed during the previous fortnight.

The movement of water in the Strait was found to be tidal, affected by a surface-current running into the Mediterranean from the Atlantic. The ebb-tide, which is a combination of the two streams running in the same direction, sets to the eastward, and at a rate considerably faster than the flood-tide, which is caused by the two streams meeting, and which runs to the westward.

In the Narrows, in the middle of the Strait, the surface-water is affected by tidal influence, but not to so great an extent as the water inshore; for the east-running current from the Atlantic being here collected into an extremely narrow and strong-running stream, it is only during easterly winds and calms that a decided set to the westward is enabled to manifest itself; during westerly winds the tidal set to the westward is doubtless frequently overpowered by the extra strength of the in-running Atlantic current, and no set to the westward can be expected; at the same time there must be a considerable diminution of the east-running current during the flood-tide. In the Narrows the actual distance run by the surface-water towards the

Atlantic during one flood-tide under the most favourable circumstances, viz. strong continuous east winds, was only two miles, while the easterly set was found to be at least ten miles during the ebb.

This, however, is the case with the surface-water only; the bottom stratum is unaffected by the in-running current from the Atlantic and sets east or west according to the tides for equal periods; and there is no reason to suppose that it is interfered with by westerly winds as the upper water undoubtedly is.

On the shallow ridge at the west entrance of the Strait the in-running current from the Atlantic is insufficient to counteract the effect of the west-running flood-tide, and a tidal influence was shown on both the surface- and bottom-water.

It was found that the surface-water moved towards the east during the ebb-tide faster than the bottom-water; with the flood it set towards the west slower than the bottom-water; and it ran towards the east faster than it ran towards the west. The bottom-water ran to the westward with the flood faster than it ran to the eastward with the ebb. The power exerting itself to produce this excess of west-running water at the bottom is therefore sufficient to annihilate the in-running current which makes itself so easily felt in the water above.

The prevailing set of the surface-current running from the westward into the Mediterranean being met and checked by an advancing tidal wave coming from the eastward, the water is raised and causes the flood-tide (*i. e.* on the shore); after the wave has passed the water falls, running to the eastward with the prevailing current; thus the flood or rising tide by the shore in the Strait is produced by the west-running current. The change of the tidal stream in the bottom stratum corresponds with the time of high and low water at Gibraltar. With east winds, the surface-current stops running to the east from one to two hours after low water; it turns to run to the east again at high water.

These observations may be of great practical use to the large number of sailing-vessels engaged in the Mediterranean trade. Vessels intending to beat out of the Strait to the westward should get under weigh, or sail out from under shelter, at low water; at half flood long tacks may be made from shore to shore, but shelter must be gained before high water.

The method and apparatus employed in observation of the currents were similar to those with which we have become familiar through the reports of recent deep-sea researches. The current-drags were made of canvas, hanging 4 feet below two light wooden crossed yards, each 4 feet long, secured in the middle and weighted to 75 lbs., the weights being attached to the bottom of the canvas. This was suspended at the requisite depth by a fine line, 0·2 inch in diameter, hanging from one end of an iron buoy, 5½ feet long, 1 foot in diameter in the middle, and pointed at each end, each buoy being capable of floating 100 lbs. weight. Close to the current-drag was a stouter line by which to lift it out of the water, which

the light line was incapable of doing. The line was made very fine, to eliminate as much as possible the error which must always creep in in consequence of the suspending-line passing through currents running in different directions and at different speeds.

In practice it was found that the surface- and bottom-water were rarely moving at the same speed, which was indicated by the buoy from which the current-drag was suspended being pulled through the surface-water. The movement of the current-drag itself floating in the lower stratum must therefore have been affected to a certain extent corresponding to the force of the upper current pressing against the buoy. In making a correction for this movement, Captain Nares estimates that the current-drag was itself retarded to an extent equal to half the difference of their speed. Although the pressure on the buoy carried it away from a position immediately over the current-drag, yet the suspending-line was never more than 5° out of the vertical, showing that the current-drag did not vary its depth of flotation to any appreciable extent.

To obtain the surface-drift, a current-drag, lowered 2 feet below the surface, was suspended from a cork sphere, 1 foot in diameter, which, floating with the top just above water, presented as little surface as possible to be acted upon by the wind, and kept the current-drag itself below the wash of the sea.

As these observations were made in August, when the evaporation of the Mediterranean is at its maximum, and the freshwater supply derived from rain and rivers at a minimum, no great increase in the in-running superficial current can be expected during the winter months when these conditions are reversed.

From these remarks we pass to the practical work of observation; and it may suffice to give in full detail the proceedings of one day only, which will serve to show how the conclusions were arrived at.

On August 22nd, in mid-channel 4 miles S.S.W. of Tarifa, wind east, force 3, with a smooth sea, about one hour after high water, after sounding in 325 fathoms, a current-drag was lowered 2 feet below the surface, and allowed to drift, the ship keeping station close to it to enable its position to be fixed occasionally by angles between shore objects.

It drifted in 28 minutes E.N.E. 0·8 mile = 1·7 mile an hour. By observations immediately afterwards the temperature of the water at the surface was found to be 63°, and at 200 fathoms below 57°.

At 4½ hours after high water, a boat was anchored in 330 fathoms. The surface-current was then running towards the east at an estimated speed of 1·5 knot an hour. A current-drag was lowered 150 fathoms, but soon after settling down, and before any measurement of the distance run could be obtained, its suspending-line fouled the anchor-line and was carried away, showing that a strong under-current was running to the eastward, which subsequent observations proved was its natural course at this time of tide. The anchor was then picked up to change its position rather to the westward, where the strait was broader.

At 2 hours after low water, the density of the bottom-water in 170 fathoms having been ascertained, a boat was anchored midway between Tangier and Cape Plata. At 3 hours after low water the surface-current was found to be running to the westward one knot an hour by log-line. A current-drag was then lowered 125 fathoms. By mast-head angles from the boat at anchor it drifted in ten minutes N. 87° W. 450 yards $=1{\cdot}35$ mile an hour. The drag pulling its buoy through the surface-water to the westward indicated that the bottom-water was really moving at an estimated rate of 1·5 mile an hour. At 4 and 5 hours after low water, shore positions showed that the surface-water was running S.S.W. 0·4 mile an hour; and at 5½ hours it was stationary.

The observations on this day, although not so continuous or complete as those taken subsequently, proved that the bottom-water was affected by tidal influence, running to the westward as the water was rising, and to the eastward as the water fell.

August 23rd.—One hour and a half after high water, the weather being calm, with smooth sea, a buoy was anchored in 125 fathoms, with 25 fathoms of light chain and 150 fathoms of No. 2 sounding-line, on the north part of the shallow ridge stretching across the Strait between Capes Spartel and Trafalgar.

The surface-current, and probably the bottom-water also, was running so strongly to the eastward that the buoy sank from the pressure of the water against it and the line. As it did not show itself again at slack water, it probably collapsed from pressure. Two buoys were then anchored in the same place with a similar line and chain. The lower buoy sank below the surface, leaving the second one floating with one foot of the top above water, indicating a strain of nearly 200 lbs. exerted by the pressure of the current on the line and buoys. At 2½ hours after high water the surface-current was running east 1·25 mile an hour by log-line. At a depth of 25 fathoms the direction and speed of the under-current were the same. At 3½ hours after high water, the drag was lowered to 100 fathoms, when a perceptible retardation of the under-current was observed, the surface-water setting to the east past the boat at an estimated rate of ¼ knot an hour.

The drag was then lowered to 119 fathoms, being 6 fathoms from the bottom, and attached to a buoy; and on being observed half an hour later, or about 5 hours after high water, it was found by mast-head angles to have drifted E.N.E. 0·09 mile$=$0·18 mile per hour. In the mean time the second buoy at anchor floated on its bilge, showing the surface-tide to have slackened and the strain to have lessened to about 110 lbs., estimated as being equal to ½ a knot. Soon after the top of the underneath buoy floated one foot out of water, indicating a further decrease of tide.

At low water the bottom current-drag had drifted during the last hour 0·1 mile N.W. by N. The surface-current was running past the buoy to the eastward, and was therefore exercising some influence on the current-

drag, preventing it going to the N.W. as fast as it otherwise would. And at one hour after low water the drag had drifted W. $\frac{1}{2}$ N. at a rate of 0·18 mile an hour, while the surface-current had stopped running, as shown by the buoys at anchor.

Two hours after lowwater, the drag being placed at a depth of 108 fathoms, 10 fathoms above the bottom, immediately ran away with its buoy to the westward, and drew it fast through the surface-water against a light west wind, force 1 ; at the same time the anchored buoys showed a very slight surface-current running to the S.W. Half an hour later the bottom current-drag had drifted west 0·48 of a mile = 0·96 mile an hour, the current itself at 1·2 mile an hour. The surface-current was running to the westward past the anchored buoys at an estimated speed of half a mile an hour.

From this day's observations it appears that the bottom-water turned to run to the westward a little before, and the surface-stratum at about an hour after low water.

On August 26, wind east, force 3, with a considerable swell from the eastward, while making observations on the penetration of light at different depths of water, some current-buoys were anchored in mid-channel south of Europa Point, in 480 fathoms, to indicate the direction of the surface-current. Half an hour before high water, when the current, if affected by tide alone, should have been running to the westward, it was found to be running towards the east at about half a mile an hour, indicated by the lowest of three buoys floating 2 feet above water. During the next 2 hours, or until 1$\frac{1}{2}$ hour after high water, the lowest buoy was out of sight and the second buoy had a slight strain on it, the tide estimated to be running three quarters of a mile an hour. From 2$\frac{1}{2}$ to 4 hours after high water the second buoy swimming, with half its depth immersed, indicated a tide of about 1 mile an hour. The pressure on the buoys now increased sufficiently to sink two of them completely, and the strain proved to be too much for the anchor, which was found to be shifting its position.

At low water the current was running at its greatest speed, estimated to be 3 miles an hour, with no appearance of changing.

August 28.—Wind easterly, force 6, with a slight swell from the eastward. As the strongest current was expected to be found on the African side of the Narrows, a position was taken up in the deep water from 2 to 3 miles north of Point Ceres. At 3$\frac{1}{2}$ hours after low water, when the current should have been running to the westward, a buoy anchored by a fine line (to diminish the strain as much as possible) showed it to be running to the eastward at an estimated speed of half a mile an hour, but from 1$\frac{1}{2}$ hour before high water to high water there was little or no movement of the surface-water.

At high water the surface-current again commenced running to the eastward past the anchored buoys, and soon after the line was observed to have been carried away.

One hour and a half later the current-drag was lowered 225 fathoms,

when its attached buoy immediately started off, moving towards the east much faster than the surface-current. Angles taken to positions on shore showed that the drag was moving to the eastward 2·1 miles an hour.

Subsequent observations, made on the south side of the ridge of shallow water between Capes Spartel and Trafalgar, and in the Narrows south of Tarifa, confirmed the foregoing results, and showed that while the surface-current was running east 1 and 2 miles an hour, the bottom-current was moving in the same direction from 2·4 to 2·6 miles. Somewhat later, the under-current at 50 fathoms and the surface-current were found to have the same rate of speed, namely 3·8 miles an hour. Differences of rate then took place, but the current maintained its direction and tidal character; and as the rate of the under-current gradually decreased, there is no reason to suppose that it did not turn to run to the westward at low water. The quicker movement of the under-current at the early part of the tide may have been due to retardation of the surface-current by a strong breeze from the east. For all practical purposes the bottom-current below 100 fathoms may be regarded as uniform. The current was found to turn with the tide, and to vary its rate in accordance with the progress of the ebb or the flow.

In concluding his report, Captain Nares remarks that on the 28th a much stronger east-running bottom current was experienced, which he attributes to the observations on that day being made in the position where the easterly in-set runs strongest; but it may have been occasioned by a reflux of water into the Mediterranean stronger than usual to replace that blown to the westward during the previous east winds, which the swell denoted had been blowing strongly a day or two before. For the first two hours of the ebb-tide the bottom-current ran to the eastward faster than the surface-current, the same as observed on the 28th.

The strong west-running under-current of 1·8 mile an hour thus shown to exist in the Narrows where the inflow of the Atlantic stream retards the surface-current most, agrees fairly with the slower movement of 1·2 mile an hour in the broader and shallower part to the westward, where the surface-water is less affected.

In the Narrows the last of the ebb and first of the flood sets across from Tarifa towards Point Ceres, joining the inshore current running to the westward along that shore.

These observations show that the under-current in the narrowest part of the Strait is affected by tidal influence, the same as the water on the shallow ridge to the westward. But the eddies, which would naturally be expected at this part, in consequence of the funnel-shaped mouth of the Strait, complicate the movements and prevent such exact demonstrations as those found further to the westward, where the current stream runs more steadily.

The Report will shortly be published in full by the Admiralty.

Surface-Current.

Date.	Rising Tide.		Falling Tide.				How Measurements were obtained.
	Surface Drift.		No. of Hours after		Surface Drift.		
	Direction.	Rate, in Miles.	L.W.	H.W.	Direction.	Rate, in Miles.	
1871. Aug. 22 ...	...	...	...	1	E.N.E.	1·7	Shore angles.
	...	...	...	4½	Eastward	1·5	Estimated.
	...	...	...	L.W.	East	0·5	"
	Westward	1·0	2	...	...	...	Log.
	"	1·0	4	...	...	...	Shore positions.
	S.S.W.	0·42	4½ to 5½	...	...	...	"
Aug. 23 ...	...	...	...	2	East	1·25	Log.
	...	...	...	4	"	0·75	Estimated.
	...	...	...	L.W.	"	0·1	"
	Slack water		1½	...	...	...	"
	S.W.	0·25	1½	...	...	...	"
	S.W.	0·5	2½	...	...	...	"
Aug. 26 ...	East	0·5	5½	...	...	...	Estimated.
	...	...	...	H.W. to 2	E.N.E.	0·75	"
	...	...	...	2½ to 4	East	1·0	"
	...	...	...	4 to 6	"	2·0	"
	...	...	...	L.W.	"	3·0	"
Aug. 28 ...	East	0·5	3½	...	...	...	Estimated.
	...	...	...	1 to 1½	East	1·0	"
	...	...	...	2 to 2½	"	2·0	"
	...	...	...	3	E. by S.	3·8	"
	...	...	...	3½	"	4·4	"
	...	...	...	4½	"	3·3	"
Aug. 29 ...	S.W.	0·6	2	...	...	...	Log.
	S.W. by W.	0·75	2 to 3	...	...	...	Mast-head angles.
	S.W.	0·75	5	...	...	...	Estimated.
	S.W.	0·2	5½	...	...	...	"
	...	...	...	H.W.	East	0·2	"
	...	...	...	3 to 3½	E. by N.	1·2	"
Aug. 30 ...	S. by E.	1·2	½ to 1½	...	...	...	Shore positions.
	S.W. ½ S.	1·55	1½ to 3	...	...	...	"
	W.S.W.	0·55	4 to 5	...	...	...	"
	...	...	...	1	East	0·5	Estimated.
	...	...	...	2	"	1·0	"

Lower Current.

Date.	Rising Tide. Drift of Current-drag. Direction.	Rising Tide. Drift of Current-drag. Rate, in Miles.	Rising Tide. Estimated true Drift of Lower Current.	No. of Hours after L.W.	No. of Hours after H.W.	Falling Tide. Drift of Current-drag. Direction.	Falling Tide. Drift of Current-drag. Rate, in Miles.	Falling Tide. Estimated true Drift of Lower Current.	Depth. Drag.	Depth. Water or Soundings.	How Measurements were obtained.
									fth.	fth.	
1871. Aug. 22.	...	...	...	...	4	East	1·5	1·5	150	330	Estimated.
	West	1·35	1·5	4	...	...	...	...	150	170	Mast-head angles.
Aug. 23.	...	...	...	...	2½	East	1·25	...	25	...	Log-line.
	...	...	...	...	3	East	1·0	0·88	100	...	Estimated.
	...	...	...	...	5	E.N.E.	0·18	Slck. w.	119	125	Mast-head angles.
	N.W. by N.	0·1	0·2	L.W.	...	...	...	...	...	...	
	W. ¼ N.	0·18	0·18	1	...	...	...	...	...	...	
	West	0·95	0·95	2¾	...	...	...	...	...	...	
Aug. 28.	...	...	...	...	1½	East	2·1	2·6	225	300 to 400	Shore positions.
	...	...	...	..	2	"	2·4	2·6	275		
	...	...	...	...	2½	"	2·2	2·4	300		
	...	...	...	...	3	E. ½ S.	3·8	3·8	50		
	...	...	...	...	3½	"	3·2	2·6	100		
	...	...	...	...	4	E. by S. ½ S.	2·7	1·9	150		
	...	...	...	...	4½	"	2·0	1·4	225		
	...	...	...	...	4¾	"	1·7	0·9	"		
	...	...	...	...	5	"	1·6	0·8	"		
Aug. 29.	W. by S.	1·05	1·25	3 to 3½	...	...	...	...	155	180	Mast-head angles.
	W. ½ S.	0·9	1·10	3½ to 4½	...	...	...	...	"	"	
	Slack water.			5½	...	...	...	...	"	"	
	...	...	...	...	3 to 3½	E. by N.	0·87	0·67	"	"	
Aug. 30.	W. ¾ N.	1·43	N.W. by 1·8	1½ to 2½	...	...	...	..	250	300 to 400	Shore positions.
	W. by N. ½ N.	1·36	N.W. to 1·6	2½ to 4	..	...	...	...	"		
	"	1·0	1·25	4 to 5	...	...	...	...	"		
	S.W.	0·8	0·8	5 to H.W.	...	...	...	...	"		
	...	...	...	...	H.W. to ½	E. ½ S.	0·84	0·8	"		
	...	...	...	...	½ to 1	E. by S.	0·84	1·0	"		
					1 to 2	E. ½ S.	1·36	1·5	"		

Soundings by H.M.S. 'Shearwater,' 1871.

Date.	Hour.	Latitude.	Longitude.	Temperature. Surface.	Temperature. Bottom.	Depth.
	h m	° ′ ″	° ′ ″	°	°	fths.
Aug. 16...	...	41 9 12 N.	10 2 45 W.	...	...	210 m.
„ 17...	...	38 20 30	9 23 0	66	56	600 m.
„ „ ...	...	37 53 18	9 21 15	...	...	325
„ „ ...	...	38 12 54	9 21 15	...	...	590 m.
„ „ ...	...	38 3 18	9 21 15	...	...	570 m.
„ 19...	...	36 46 44	9 38 45	68	37·5	1560 m.
„ 20...	...	36 2 25	7 43 0	68	51·5	665 m.
„ 21...	5 20 A.M.	35 47 30	6 41 0	69	51·8	355 s.
„ „ ...	6 30 „	35 45 0	6 36 30	...	...	330 s.
„ „ ...	8 0 „	35 43 0	6 33 45	70	52	290 s.
„ „ ...	9 40 „	35 40 14	6 28 30	...	...	225 s.
„ „ ...	11 0 „	35 38 0	6 25 0	70	58	115 s.
„ „ ...	11 40 „	35 36 44	6 23 30	...	...	75 hard grnd.
„ „ ...	12 50 „	35 40 30	6 23 45	...	...	150 „ „
„ „ ...	3 0 P.M.	35 47 14	6 24 0	70	55·7	325 s. m.
„ 22...	...	35 55 40	5 35 40	...	...	330 s.
„ „ ...	...	35 55 25	5 47 20	...	...	170 hard grnd.
„ 23...	...	35 57 10	5 58 10	...	...	125
„ 29...	...	35 48 20	6 4 50	...	...	190 hard grnd.
Oct. 3...	...	35 54 10	16 23 0 E.	80	56·75	1970 m.
„ 11...	...	32 17 30	26 44 0	79	56	1650 m.
„ 12...	...	31 21 30	28 3 20	76	57	355 m.

Temperatures obtained in H.M.S. 'Shearwater,' 1871.

	Atlantic.				Entrance of the Strait of Gibraltar.	Strait of Gibraltar.		Mediterranean.				
Latitude .... Longitude	41° 9′ 12″ N. 10 2 45 W.	36° 46′ 44″ N. 9 38 45 W.	36° 2′ 25″ N. 7 43 0 W.	35° 47′ 30″ N. 6 41 0 W.	35° 47′ 14″ N. 6 24 0 W.	35° 55′ 40″ N. 5 35 40 W.	35° 48′ 20″ N. 6 4 50 W.	35° 54′ 10″ N. 16 23 0 E.		32° 17′ 30″ N. 26 44 0 E.		31° 21′ 30″ N. 28 3 20 E.
Date	Aug. 17th.	Aug. 19th.	Aug. 20th.	Aug. 21st.	Aug. 21st.	Aug. 22nd.	Aug. 29th.	Oct. 3rd.		Oct. 11th.		Oct. 12th.
Fathoms 0	66°	68°	68°	69°	70°	70°	......	80°	80°	79°	79°	76°
" 10	......	......	68·5	......	......	......	......	76·2	77	76		
" 20	......	......	62	......	......	......	61°	72·6	74·2	76		
" 30	......	......	61	......	......	......	......	66·2	67	75·5		
" 40	......	......	59·5	......	......	......	60·8	63·5	62	69		
" 50	......	......	59·5	......	......	......	......	59·5	61	67	71	
" 60	......	......	59·0	......	......	......	......	59·7	......	65·5		
" 70	......	......	58·8	......	......	......	57·8	59·2	......	61·8		
" 80	......	......	58·5	......	......	......	......	58·8	59	61		
" 90	......	......	58	......	......	......	56·8	58·5	......	59·8	60·5	
" 100	......	......	57·7	......	......	......	......	58·5	58·5	59·5		
" 110	......	......	......	......	58	......						
" 120	......	......	......	......	58	......	55					
" 130	......	......	......	......	......	......	......	......	58·8			
" 140	......	......	......	......	......	......	55	......	......	......	59·5	
" 150	......	......	......	......	......	......	......	......	58·8			
" 160												
" 170												
" 180	......	......	......	......	......	......	54·8	......	58			
" 190												
" 200	......	......	......	......	56	57	56	......	56·5	......	58·5	
" 250	......	......	......	......	......	......	......	......	......	......	57·5	
" 300	......	......	......	52	......	......	......	......	......	......	57	
" 350	......	......	......	......	55·7	55·5	......	......	......	......	56	
" 400	......	......	......	51·8	......	......	......	......	......	......	56	57°
" 450	......	......	......	......	......	......	......	......	......	......	56·8	
" 500	......	......	......	......	......	......	......	......	......	......	57	
" 550												
" 600	54·5											
" 650	......	......	......	......	......	......	......	......	......	58		
" 700	......	50·5	51·5									
" 800	......	46										
" 900	......	42										
" 1000	......	38										
" 1100												
" 1200	......	38·5	......	......	......	......	......	......	......	56·35		
" 1500												
" 1560	......	37·5										
" 1650	......	......	......	......	......	......	......	......	......	56		
" 1750												
" 2000	......	......	......	......	......	......	......	56·75				

The two low temperatures on the 21st at about 350 fathoms show the marked differences between the Atlantic and Mediterranean water.

January 25, 1872.

Sir JAMES PAGET, Bart., D.C.L., Vice-President, in the Chair.

The following communications were read:—

I. "The Absolute Direction and Intensity of the Earth's Magnetic Force at Bombay, and its Secular and Annual Variations." By CHARLES CHAMBERS, F.R.S., Superintendent of the Colaba Observatory. Received October 26, 1871.

(Abstract.)

The observations discussed in this paper were taken at the Colaba Observatory during the years 1867 to 1870, and consist of observations of Dip, Declination, and Horizontal Intensity. The principal results deduced by the author from these observations are shown in the following statement:—

Magnetic element.	Epoch.	Value at epoch.	Value at common epoch, January 1st, 1869.	Secular change. — Per annum.	Semiannual inequality. — Excess of April to September over mean of year.	Calculated probable error of a single weekly determination.
Declination	April 1, 1868	0° 46′ 47″ E.	0° 48′ 36″ E.	+2′ 5″	+1″	±20″
Dip..................	Oct. 1, 1868	19° 4′·2	19° 4′·7	+1′·9	+0′·3	±0′·25*
Horizontal Force.	April 1, 1869	8·0591	8·0581	+·0040	·0000	±·0043*
Total Force	Jan. 1, 1869	8·5264	8·5264	+·0059	+·0003	......

In column 2 is entered the mean epoch to which the mean value of each element, entered in column 3, corresponds.

The absolute observations were taken at a height of 38 feet above the ground; and by comparing them with observations taken with differential instruments at a height of 6 feet above the ground, they are shown to indicate distinctly a diminution of terrestrial magnetic action with increase of height, with respect both to secular variation of Declination and Horizontal Force, and to diurnal inequality of Horizontal Force.

II. "On the Elimination of Alcohol." By A. DUPRÉ, Ph.D., Lecturer on Chemistry at Westminster Hospital. Communicated by W. ODLING, M.B., F.R.S. Received November 16, 1871.

(Abstract.)

Obviously three results may follow the ingestion of alcohol. All the alcohol may be oxidized and none be eliminated, or a portion only may be

* In English units.

oxidized and the rest be eliminated unaltered; or, lastly, all may be eliminated again unaltered. Assuming the last to be the case, it would follow that, if a certain quantity of alcohol be taken daily, the amount eliminated would increase from day to day until, at last, the amount eliminated daily would equal the daily consumption, be this time 5, 10, or more days. If, on the other hand, all the alcohol consumed is either oxidized or eliminated within 24 hours, no increase in the daily elimination will take place in consequence of the continuance of the alcohol diet. Guided by these considerations, the author undertook two series of experiments, in which the amount of alcohol eliminated by both kidneys and lungs was carefully estimated. The analytical processes employed are described in detail.

First series.—After a total abstinence from alcohol for 11 days, the urine and breath were examined, after which, from the 12th to the 24th day, both inclusive, the author took 112 cub. centims. of brandy daily (equal to 48·68 grms. absolute alcohol). The urine and breath were examined on the 12th, the 18th, and the 24th day. The urine was also examined during the 5 days following the cessation of the alcohol diet. The analytical results obtained are given in a Table.

Second series.—After having again abstained from the use of alcohol, in any shape, during 10 days, the author took 56 cub. centims. of brandy (same as above) at 10 A.M. on March the 29th. The urine was collected for every 3 hours up to the 12th, from the 12th to the 24th hour, and during the next succeeding 2 days. The alcohol eliminated in the breath was also estimated during the same intervals. The analytical results are also arranged in a tabular form.

The results of both series may be summed up as follows:—

The amount of alcohol eliminated per day does not increase with the continuance of the alcohol diet; therefore all the alcohol consumed daily must, of necessity, be disposed of daily; and as it certainly is not eliminated within that time, it must be destroyed in the system.

The elimination of alcohol following the ingestion of a dose, or doses, of alcohol ceases in from 9 to 24 hours after the last dose has been taken.

The amount of alcohol eliminated, in both breath and urine, is a minute fraction only of the amount of alcohol taken.

In the course of these experiments, the author found that, after six weeks of total abstinence, and even in the case of a teetotaller, a substance is eliminated in the urine, and perhaps also in the breath, which, though apparently not alcohol, gives all the reactions ordinarily used for the detection of traces of alcohol, viz. it passes over with the first portions of the distillate, it yields acetic acid on oxidation, gives the emerald-green reaction with bichromate of potassium and strong sulphuric acid, yields iodoform, and its aqueous solution has a lower specific gravity and a higher vapour tension than pure water. The presence of a substance in human urine and the urine of various animals which yields iodoform, but is not alcohol, had already been discovered by M. Lieben. The quantity pre-

sent in urine is, however, so small that the precise nature of this substance has not as yet been determined.

Finally, the author points out an apparent connexion between this substance and alcohol. It was found that, after the elimination due to the ingestion of alcohol had ceased, the amount of this substance eliminated in a given time at first remained below the quantity normally excreted, and only gradually rose again to the normal standard. A careful study of this connexion may perhaps serve to throw some light upon the physiological action of alcohol.

III. "On the Action of Low Temperatures on Supersaturated Solutions of Glauber's Salt." By CHARLES TOMLINSON, F.R.S. Received December 4, 1871.

When a solution of the ordinary ten-atom hydrate of sodic sulphate, saturated at about 93° F., its maximum point of solubility, is boiled and filtered into a clean flask, which, being closed, is left to cool to 40° and under, a modified or seven-atom hydrate is formed at the bottom of the solution; this increases in quantity as the temperature falls, and passes into solution as the temperature rises; and, so far, the observation is supposed to be complete.

But if a supersaturated solution of Glauber's salt be reduced from ordinary atmospheric temperatures to low ones by means of a freezing-mixture of snow and salt, the results obtained are so remarkable that I venture to think a short statement of them may be worthy of a place in the 'Proceedings,' by way of addenda to Section II. of my second paper "On Supersaturated Saline Solutions," contained in the Philosophical Transactions for 1871, page 59.

A solution of one part Glauber's salt in one of water was boiled and filtered into a two-ounce flask that had been previously filled with strong nitric acid and well rinsed with clean water. The solution was again boiled in this flask, into which a thermometer was passed, the stem being surrounded by several turns of lamp-cotton, which served to close the flask as soon as it was removed from the source of heat.

Next day the flask was put into a freezing-mixture at about 15° F. The solution slowly sank to 19°, when there was an abundant deposit of crystals of a peculiar opaque white, not like the transparent octahedra that are thrown down when these solutions cool to 40° and under, but very much like the octahedral crystals formed during the cooling of a strong solution of sal-ammoniac. There were tufts of regular octahedra and fern-like crystalline forms. During their formation the thermometer rose to 26°. The flask was now transferred to water at 48°, when the opaque-white crystals broke up into an amorphous woolly mass. As the temperature of the solution rose to 40°, then for the first time the usual transparent octahedra of the anhydrous salt fell down. Next day the

flask was opened; crystallization of the ordinary salt set in from the surface, and the temperature rose from 44° to 65°.

Thus one more hydrate is added to those already known as belonging to this remarkable salt. It doubtless contains less water than the seven-atom hydrate; but I know of no method of testing its hydration, since its existence depends upon the low temperature, and shelter from the action of nuclei. In this way it resembles the various hydrates described in my paper in the 'Transactions.'

The solution was next made twice as strong as before, that is, two parts of Glauber's salt were dissolved in one part of water, and after boiling and filtering and reboiling as before, the flask was set aside to cool. When the thermometer marked 42°, the flask was put into the freezing-mixture. At 38° a few transparent octahedra were thrown down, and the heat-currents thereby liberated delayed the cooling. In fourteen minutes it reached 26°, and the transparent crystals at the bottom became opaque white. The thermometer was stationary during some minutes at 26°, when it began again to descend; but on agitating the flask in the freezing-mixture, crystals of the opaque-white salt were formed, and the temperature regained 26°, the solution above being bright and clear, and still supersaturated. In a few minutes crystallization set in from the surface, and the thermometer rose from 26° to 53°, the whole being now solid.

These opaque crystals resemble in texture newly formed white lead; and at whatever temperature they may be formed below 26°, their formation causes the thermometer to rise to 26°, and that, too, in solutions of 1 part, 2 parts, or 3 parts salt to one of water. This opaque salt is sometimes amorphous, and then it covers the surface of the flask like thick whitewash. This effect occurs when the flask is much agitated in the freezing-mixture.

The same flask (2 salt to 1 water) was reboiled without any addition of water, so that the solution was really stronger than that indicated. At 40° there was a fall of transparent anhydrous crystals. The solution was now purposely cooled very slowly, so that in half an hour it descended only 3°, namely, to 37°. There was now a considerable increase of the anhydrous salt so as to cover the bottom of the flask, and to rise a little way up the sides. The flask was transferred to a freezing-mixture at 10°; when at 33° the anhydrous salt became opaque, doubtless from the fixation of a portion of water less than that required for the formation of the seven-atom salt. At 24° opaque tufts and fern-like crystals were formed. At 22° there was a sudden and copious deposit of this opaque-white hydrate; the thermometer rose to 26°, and then suddenly to 52°, when the whole mass was solid.

It is commonly supposed that the rise in temperature consequent on the solidification of a supersaturated solution is dependent on its mass; that when this is considerable, the rise in temperature is so too, but that when the mass is small there is but little heating. This does not accord

with my experience. Not much more than half an ounce of a comparatively weak solution of Glauber's salt, such as 1 salt to 1 water, may rise from 20° to 56° on suddenly becoming solid; and with 2 or 3 salt to 1 of water the rise may not be greater, especially if a considerable mass of the two abnormal hydrates be already formed, and only a small portion of the solution remain to become solid.

In another experiment, 3 parts salt to 1 of water were boiled and filtered into two test-tubes and one 2-ounce flask. One tube, on being put into the freezing-mixture, sank to 35°, when the solution suddenly became solid, and the thermometer rose to 78°. The other tube-solution threw down so large a quantity of anhydrous crystals as to prevent the reading of the thermometer. The solution in the flask threw down anhydrous crystals at 44°, and then sank very slowly to 40°, where it remained stationary upwards of ten minutes, in consequence of the liberation of heat-currents, occasionally rising to 41°. A large quantity of transparent crystals was now heaped up on the bulb of the thermometer; the temperature descended to 38°, with slight starts upwards; and in slowly descending to 33°, there was a large increase of the transparent crystals. At 32° the flask was transferred to a fresh freezing-mixture at 10°, and the solution slowly descended to 22°, when it was again removed to a fresh freezing-mixture, also at 10°. Soon a number of large fern-like crystals covered the side of the flask, starting, apparently, from the top of the copious deposit first produced, and rendering the upper part opaque in a well-defined line. The temperature rose to 26°, and continued there some minutes, when the solution suddenly crystallized, and the thermometer rose to 48°.

Supersaturated solutions of potash alum, exposed to low temperatures, behave much in the same way as the solutions of double salts described in my former paper. A solution of 300 grains of the salt in 1½ oz. of water, boiled and filtered into clean test-tubes, and, when cold, put into a freezing-mixture at about 0° F., displays the beautiful ivy-leaf kind of foliage, of a brilliant white colour, already referred to. The growth starts from the bottom or from the surface of the solution, or from both, and soon the whole solution becomes solid. If the tube be put into water at 32°, the solid rapidly melts, and the liquid is a clear bright supersaturated solution as before.

Löwel, in his first memoir (An. de Ch. et de Ph. 3 série, tome xxix.), found that when supersaturated solutions of Glauber's salt, in sealed tubes, were subjected to temperatures varying from −8° to −10° C., they often froze and burst the tubes. In one case, where the tube did not burst, the solution, in thawing, caused the state of supersaturation to cease. In another case the frozen solution thawed, and the liquor became supersaturated as before. Löwel could not reproduce this last effect, nor explain why the thawing should lead to the formation of the ten-atom salt. But as he did not know the conditions of clean and unclean, he was constantly looking out for some catalytic action in the sides of his vessels to explain the many

anomalous cases that occurred to him consequent on the use of vessels not chemically clean.

Among the numerous writers on the subject of supersaturation, I know of none that has noticed the formation of the second modified hydrate of sodic sulphate except M. Viollette, who, in a "Mémoire sur la Sursaturation" contained in the 'Annales Scientifiques de l'École Normale Supérieure" (tome troisième année, 1866), refers in about a dozen words, p. 223, to the formation of another hydrate, "qui cristallise difficilement en forme de choux-fleurs."

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"Some Remarks on the Mechanism of Respiration." By F. Le Gros Clark, Surgeon to St. Thomas's Hospital, Member of Council and of the Court of Examiners of the Royal College of Surgeons, and late Hunterian Professor of Surgery and Pathology in the College, and Examiner in Surgery at the University of London. Communicated by P. Martin Duncan, M.B., F.R.S., Professor of Geology in King's College, London. Received April 18, 1871*.

1. In performing some experiments on recently slaughtered animals, for the elucidation of a subject which I was then engaged in investigating, I was struck with the remarkable arched tension of the diaphragm, a condition which continued unabated until air was admitted into the pleural spaces, and then it became flaccid and descended. This altered condition was attended by collapse of the lungs, which receded from the thoracic walls, and occupied a much smaller space than previously.

2. In order to measure the quantity of air expelled from the lungs in the preceding experiment, and to ascertain how far the collapse of the lungs and the altered condition of the diaphragm were dependent on each other, the following experiment was performed. The trachea of a sheep, immediately after death, was exposed in the neck, divided, and tied over a glass tube, which was put in communication with a graduated receiver placed under water, and guarded by a stopcock. The pleuræ were then opened, and as air entered the diaphragm became flaccid; but the lungs remained unchanged in position and form. The stopcock was then opened, and a rush of air, displacing the water in the receiver, was accompanied by collapse of the lungs. The quantity of air thus forcibly expelled was from ten to twelve cubic inches.

3. The interpretation of this experiment is—

(*a*) That the passive tension of the diaphragm is due to the atmospheric pressure on its abdominal surface, which is not counterbalanced by a corresponding pressure on the opposite or thoracic surface, until air is admitted into the pleuræ.

(*b*) That the lungs retain their supplemental air by virtue of this tense condition of the diaphragm, the elasticity of the former being resisted by the tension of the latter.

(*c*) That the contractility of the lungs, tending to the expulsion of the supplemental air, removes the atmospheric pressure from the upper surface of the diaphragm, and thus produces and maintains its arched form and tense condition.

(*d*) That as soon as the pressure on the inner and outer surfaces of the lungs was equalized, by the admission of air into the pleuræ, their contractility forced out the supplemental air; and the quantity thus expelled

* Read May 25, 1871. See abstract, vol. xix. p. 486.

may be accepted as an approximate measure of the elasticity of these organs.

(*e*) That the diaphragm was rendered flaccid by the admission of air into the pleuræ, independently of the collapse of the lungs.

4. Thus there exists a constant antagonism between the atmospheric pressure on the interior of the lungs and their elastic and contractile properties, tending to the expulsion of the air they contain; and this expulsive power is limited by the resisting tension of the diaphragm. These several conditions are in no degree influenced by the direct admission of air into the abdominal cavity; for the trachea being treated as in the former experiment before the abdomen was opened, and an aperture being then made into the peritoneum, the abdominal walls rose as air rushed in, but no air was expelled from the lungs. The expansion of the abdomen under these circumstances is especially remarkable in oxen when turned on their back, and is probably in great measure due to the position favouring the expansion of the lower costal region.

5. In the recognition of the above facts, I think that the extremely tense condition of the diaphragm has not received the notice which its practical importance deserves. Independently of being the agency by which the supplemental air is retained in the lungs, the resistance thus offered limits the encroachment of the abdominal viscera on the thoracic cavity; and by this same property, combined with the broad and inseparable attachment of the central tendinous expansion to the fibrous pericardium, the heart is preserved from being impeded in its movements in the various stages of respiration.

That the tension in question fulfils these functions is exemplified pathologically as well as physiologically. It would indeed be impossible, without giving due weight to this property of the diaphragm, to account for the trifling interference, comparatively speaking, that pregnancy, or the presence of large ovarian tumours or ascitic accumulations, exercises over the mechanism of respiration; and it would appear still more surprising that the sudden removal of such distension, and with it the pressure on the diaphragm, is not attended by more serious consequences. Yet the respiratory mechanism is scarcely influenced appreciably even by the sudden collapse of an enormous ovarian cyst; and this is accounted for by the passive tension of the diaphragm, which alike resists encroachment from below and refuses the solicitation to descend which the removal of the pressure would seem to offer.

6. In the action of the diaphragm this tension is an essential condition, in order that there may be no waste of power, but that its earliest contraction may be accompanied, at once and simultaneously, by the entrance of the tidal air into the lungs; and it would seem probable that, for ordinary expiration, the suspension of its contraction and consequent return to its normal state of arched tension is alone sufficient.

7. But further considerations present themselves in reviewing this pro-

perty, in association with the attachments of the diaphragm, and the relations of other muscles credited severally with functions in both inspiration and expiration.

8. The thoracic parietes, both osseous and cartilaginous, are movable, admitting of an increase or diminution in the circumference of the chest; and the diaphragm is attached, by its circumference, to the chest-walls. As the force by which the tensely arched diaphragm is rendered plane is necessarily considerable, it may be asked, does the contraction of this muscle under any circumstances draw in the ribs? and why does it not do so always? I believe the answer to the first inquiry to be, that it does, under some circumstances, draw in the ribs; and the explanation of the second must be sought in a consideration of the agency of other muscles engaged in inspiration, to which attention will be presently directed.

9. The property of the diaphragm under consideration—its passive tension—is engaged in restoring the equilibrium of rest after a deep expiration. The supplemental air is expelled from the lungs chiefly by the action of the abdominal muscles, the circumference of the chest undergoing no appreciable change except at its lower part, where it is directly acted on by these muscles. When they are relaxed, and the lower costal region again expands, the diaphragm asserts its passive influence, by drawing in so much air as the counter-resistance of the contractility of the lung-tissue will permit.

10. But this agency is somewhat more complex than would appear from the above statement. The passive tension of the diaphragm is such that it would probably be sufficient to resist the efforts of the abdominal muscles to force it upwards, without the cooperation of another cause, which is the altered condition of the chest-walls. The lower ribs are perceptibly drawn inwards, as stated, when the supplemental air is expelled from the lungs; and, as a necessary consequence, the horizontal portion of the diaphragm is relaxed, and thus placed in a condition which permits of its being forced upwards, so as to compress the lungs.

11. But the deepest inspiration is also attended by hollowing or drawing in of the epigastrium; and the increasing circumference of the lowest costal region, which is limited as compared with the upper, is almost arrested before the act of forced inspiration has attained its maximum; and the rise in the abdomen is likewise suspended before this climax is reached. These conditions are due, I apprehend, to the combined effect of atmospheric pressure on the relaxed abdominal walls, concurrently with the extreme contraction of the diaphragm, overcoming the resistance of the intercostal muscles, and drawing in the lower ribs.

12. As exemplifying, in a remarkable and interesting way, the foregoing observations, I may mention the case of a patient recently an inmate of St. Thomas's Hospital, in whom there was fracture, with displacement of the sixth cervical vertebra, by which the cord was compressed.

He survived the injury less than three days, and there was complete paralysis of motion and loss of sensation below a line level with the nipples; the movement of the arms was also impaired. "The walls of the chest, across and below the nipples, were retracted at each inspiration." I quote the words of the Hospital Registrar; but I repeatedly proved this, and demonstrated the fact to others, by placing around the chest, just over the nipples, a tape, which became relaxed at each inspiration, to the extent of at least half an inch. The inspirations were short and sudden the expirations prolonged, and latterly intermittent. This is not the only instance in which I have observed this phenomenon under similar conditions.

The following case has more recently come under my observation, and I give it according to the report of Mr. Anderson, the Surgical Registrar of the hospital :—" F. F., aged 58, a bricklayer, was admitted into St. Thomas's Hospital in February 1871, having fallen from a considerable height, and struck some projecting object in his descent; he remained incapable of movement, though perfectly sensible. The lower limbs were paralyzed, and the movements of the upper extremities were impaired; and he complained of great pain shooting from the neck into the upper extremities. In breathing, the whole chest was very perceptibly drawn in during inspiration, and the abdomen became more prominent than is normal. The chest was slightly raised as a whole, and the sterno-mastoids were seen to act strongly. The lower ribs were not drawn downwards, but appeared to be pulled directly inwards, or inwards and slightly upwards. In expiration the chest-walls appeared to relax, as if from the cessation of some contracting influence. The respiration, as a whole, was jerking, but not increased in frequency. Subsequently the chest appeared to act unilaterally, as if from unequal action of the two sides of the diaphragm, the ribs on the left side being drawn inwards during inspiration, whilst the right half of the thorax was thrust outwards passively. He survived the accident about sixty hours; and the cord was found crushed between the fifth and sixth cervical vertebræ, which were fractured."

This peculiarity in the diaphragmatic breathing must be due, in the main, if not entirely, to the suspension of the intercostal action, whereby the ribs are left at liberty to be acted upon, and thus drawn in by the contraction of the diaphragm.

13. I would remark, as bearing upon the foregoing observations and those I am about to make, that in the unfettered adult body, in the erect or sitting posture, the sphere of respiratory movements, as seen and measured, is chiefly limited to the region which is bounded above by a horizontal line extending outwards from the lower extremity to the sternum, and below by a similar line extending from the umbilicus to the anterior spine of either ilium. In the recumbent posture, when the abdominal muscles are relaxed, the movement extends lower over the abdomen. Yet the measurements are by no means commensurate with the apparent move-

ments. The deeply notched form of the chest below, with its movable elastic boundary of cartilage, is well adapted to admit of these necessary movements of alternate expansion and contraction.

14. A difference of opinion exists as to the action of the intercostal muscles, some physiologists assuming that the external and internal sets of muscles act independently of each other and as antagonists; others supposing that different parts of the same muscle perform diverse functions. I am disposed to believe that both these conjectures are incorrect; and that Haller is right in his opinion, that both sets of intercostals act as muscles of inspiration. They act, in concert with the scaleni, in drawing the ribs upwards; they also approximate them, and rotate them on their axes,—a result which is facilitated by the increasing mobility of each pair of ribs as we descend from the first to the last. The effect of such action is to afford a fixed circumference from which the horizontal portion of the diaphragm can act without drawing the ribs inwards; at the same time that the general capacity of the chest is augmented, though its vertical diameter, so far as the intercostal action is concerned, is shortened; and the crura of the diaphragm must also aid importantly in steadying and fixing the central tendon during inspiration, and in preserving the pericardium from that encroachment to which it would be liable if the central tendon were not thus fixed at its back part, and drawn downwards from the chest.

15. But the action of the intercostal muscles, which has been a subject of so much dispute, will repay a more careful examination.

The posterior portion of the external intercostals, reaching from the angles to the tubercles of the ribs, unaccompanied by the internal intercostals, has an action similar to that of the levatores costarum, the upper of the two ribs to which each fibre is attached forming a fixed point from which the lower rib may be influenced in direction. The anterior part of the internal intercostals, passing between the costal cartilages unaccompanied by the external intercostals, must, from the direction of the fibres in relation to the direction of the portion of rib to which they are attached, act, like the posterior portion of the external intercostals, in elevating the ribs. (Here diagrams were employed to illustrate these points.)

It is hence obvious that the combined influence of these portions of the two muscles will be, where the fixed point is taken from the upper of the two ribs to which they are attached, to swing the rib in an opposite direction upon an imaginary axis drawn through the spinal and sternal attachments, taking a true rib as a type. The entire length of the external intercostals will also act with the levatores costarum, and produce the effect, in forced inspiration, of raising the anterior end of the rib, and thus thrust forward the sternum.

In the contraction of the decussating portions of each set of muscles simultaneously with the others, we get the diagonal of the action of both as the result of their joint action, the upper of the two ribs to which each pair of muscles is attached being relatively fixed.

16. The summary of the action of both sets of muscles may be thus stated:—

(1) They increase the transverse diameter of the chest, by raising the curve of the ribs more nearly to a level with the attachment of the ends. All the fibres must assist in this action, but especially the external intercostals and the anterior part of the internal intercostals.

(2) They increase the antero-posterior diameter of the chest by raising the anterior attachmeut of the ribs, and with them the sternum, more nearly to a level with the posterior attachment, thereby also separating the costal cartilages of the lower true and false ribs, and thus widening the interval which separates them on either side of the ensiform cartilage. The posterior fibres of the external intercostals will act with the levatores costarum and (in forced inspiration) with other indirect agents.

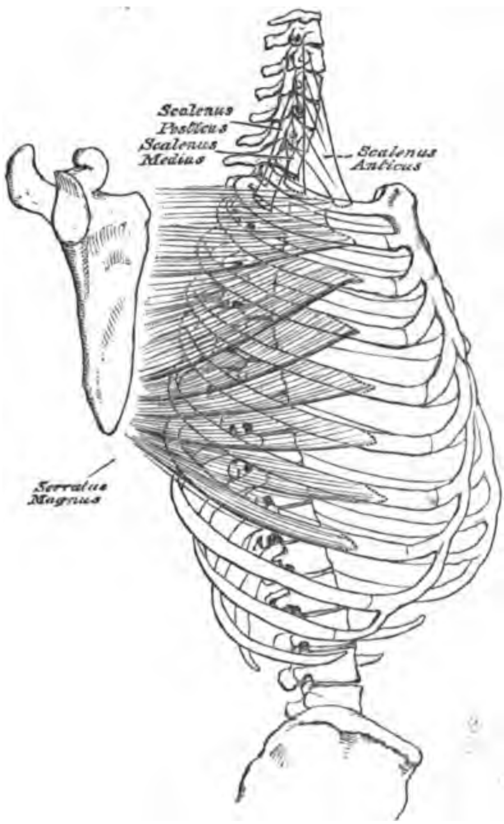
(3) They assist in rotating the ribs outwards, when they elevate them.

(4) They fix the thoracic walls. This action is a most important one: —*a*, by giving, when needed, a fixed attachment for the various muscles which are connected to the chest, and which have a double action, according to the relative mobility of their origin and insertion; *b*, by preventing the diaphragm from drawing in the walls of the chest during ordinary inspiration; *c*, by resisting the atmospheric pressure during inspiration, by virtue of the tension of the intercostal spaces.

17. The serratus magnus is usually described as a powerful muscle in forced inspiration, when the scapula is fixed; but from a careful study of the direction of its fibres, from observation of its action in deep breathing, and (so far as the experiment may be depended on) under the influence of electro-galvanism, I venture to question the correctness of this generally accepted description. The direction of the component parts of this broad muscle would seem to indicate that none but the lowest digitations can assist in elevating the ribs or increasing the capacity of the chest; on the contrary, the contraction of the upper digitations must rather tend to draw the ribs downwards, and to aid in expiration. It is true that extreme elevation of the scapula somewhat alters the relations of the origin and insertion of this muscle, but not to such a degree as to impart to it its assumed power, even if such elevation were, as it is not, a necessary condition in forced inspiration. Further, a careful observation of the well-marked digitations of this muscle in a well-developed and thin person, during the act of forced inspiration, failed to discover, either tangibly or visibly, any contraction of their fibres, which was very distinctly marked when the scapula was moved. In stimulation of the muscle by electro-galvanism, though the scapula was freely jerked and moved about, no movement whatever of the ribs or interference with respiration resulted. But it must be admitted that not much importancc can be attached to this experiment, as the more movable attachment of the muscle would necessarily yield most readily during its contraction. Indeed this remark applies to all experiments of this nature, in which a single muscle is stimulated to

contract, without reference to the cooperating or antagonistic action of other muscles. Such isolated experiments are therefore not only unsatisfactory, but often delusive.

Fig. 1.



But the question which I have raised may be discussed a little more in detail. The inferior angle of the scapula reaches as low as the eighth rib, while the thorax is in a state of repose after an ordinary inspiration. The lowest digitation of the serratus magnus, arising from the extreme end of the angle, follows the course of the eighth rib, and is attached to it. Assuming for a moment the capability of the serratus magnus to act in inspiration, it would be necessary for this result that the vertebral costa of the scapula be drawn back and rigidly fixed by the rhomboids &c.; the consequent extension of the fibres of the serratus is further enhanced by the expansion of the chest during forced inspiration—a condition which is not consistent with what is observed in other muscles during a passive

imitation of their action. In other words, the relation of the fibres of this muscle to the ribs is such, that a passive imitation of the action ascribed to them is to make tense and not to relax them; they have to run over a longer surface—a wider barrel. But the fact is, that the lower angle of the scapula is neither fixed nor in any marked degree drawn up in deep inspiration; but the angle and vertebral costa are carried further away from the spine; and as the serratus is, as already remarked, not visibly or tangibly in action, this result can be accomplished only by the costal attachments of the muscle being removed to a greater distance from the spine, by the expansion of the chest. Each digitation of the muscle acts at an increasing disadvantage as we ascend from the last to the first; therefore, if the lowest digitation is incapable of acting as an elevator of the ribs, the same negative conclusion must, *à fortiori*, be arrived at as regards the rest of the muscle.

It is no doubt true that the serratus is in action during deep inspiration, when the arm is raised. But this contraction is persistent during expiration also; and the action in each instance is explained by the fact that this muscle is required to assist the trapezius in rotating outward the lower angle of the scapula, and maintaining it in that position, in order to accommodate the relation of the glenoid cavity to the head of the humerus, and to afford a fixed attachment for the action of the deltoid. The chest is, under these circumstances, the relatively fixed origin of the serratus, and the scapula its movable insertion on which it rests.

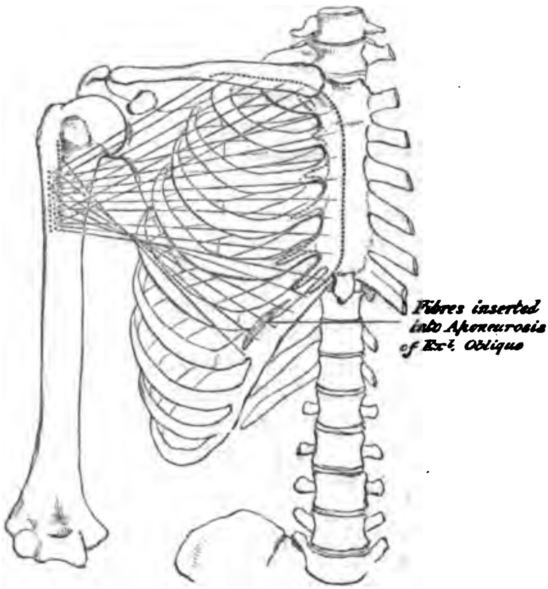
18. A parity of reasoning applies to the pectoralis major muscle. This muscle is described as assisting in forced inspiration, by raising the ribs and dilating the chest. I think this assumption incorrect for the following reasons:—

(1) A necessary condition to the action of this muscle on the thorax is the relative immobility of the scapula and humerus; but in a healthy chest the most powerful inspiratory effort may be made without any such assistance; and the pectoral fibres may be felt and seen to remain during this effort in a state of complete inaction. When the humerus is fixed, the fibres are in action; but this condition is due to their agency in fixing the bone.

(2) The clavicular and upper portion of the sternal fibres may be considered as incapable of raising the ribs, whilst the arm is not elevated; the lower fibres alone can be supposed to possess this property or power. Now the lowest fasciculus is not attached to the ribs, but terminates in the aponeurosis of the abdomen by a broad fibrous expansion: this portion of the muscle can take no part in raising the ribs. The set of fibres above these arises from the sixth rib and its cartilage, and from the adjacent portion of the sternum: they are inserted into the upper part of the outer margin of the bicipital groove, in a horizontal plane with the sternal attachment of the third rib and the spinal attachment of the sixth rib. In deep inspiration the effect of forward movement of the sternum will nearly, if

not entirely, correspond with that of the elevation of the anterior attachment of the rib; so that the pectoral fibres will not be appreciably shortened, and consequently cannot materially assist in the inspiratory effort. If we ascend a step higher, to the fifth rib, it would appear that the pectoral fibres will rather resist than assist in the act of inspiration; still more is this the case nearer to the clavicle.

Fig. 2.



When the arms are raised above the head, the great pectoral muscles would have an undoubted action in elevating the ribs; but such a position is never assumed, even in disease, for the purpose of assisting inspiration.

19. Indeed the same may be said of the serratus magnus and pectorales as of the intercostals and other muscles credited with agency in respiration,—that their action must be studied in relation to that of other muscles, which have an important influence in modifying or even in determining their action; and bearing this in mind, and for the reasons adduced, I think it very questionable whether any of these muscles can exercise an influence, except under special conditions and to a very limited extent, in the act of inspiration.

20. The conclusion to which the foregoing observations point is, that the act of inspiration is, under all circumstances, essentially, perhaps exclusively, dependent on the special muscles appropriated to this function—muscles which are competent, without extraneous aid, to answer the appeal made to them in emergencies as well as under ordinary circumstances.

21. The action of the scalenus anticus, a powerful muscle, is essential

for fixing the first rib in inspiration; and it may be distinctly felt so acting in a thin neck, during forced inspiration, when the rib into which it is inserted is raised with the sternum. The scalenus posticus has a similar action on the second rib. The pectoralis minor, when the coracoid process is fixed, and some few other unimportant muscles, may assist in the elevation of the ribs. In the absence of fixation of the upper or lower ribs, if such condition ever exist, it is difficult to resist the conclusion that the action of the intercostals must necessarily limit the capacity of the chest, by drawing the ribs towards each other and rendering the intercostal spaces rigid.

Of the lower ribs, experiment has satisfied me that both the tenth and eleventh are raised during deep inspiration—the latter in a less degree than the former. The intercostal spaces between the tenth and eleventh, and between the eleventh and twelfth ribs, are consequently widened considerably. The last rib is probably fixed by the attachment of the quadratus lumborum: this rib feels prominent and fixed during deep inspiration; whereas, under ordinary circumstances, it is loose and not easily felt.

With the view of ascertaining the mobility of the sternum, and of comparing the antero-posterior and lateral movements of the chest, I procured a large pair of callipers, with a fixing screw attached. I may thus state briefly the result of my observations on healthy individuals.

22. There is no perceptible increase in the antero-posterior diameter of the chest in ordinary inspiration. The antero-posterior diameter of the chest is augmented perceptibly in forced inspiration at the junction of the second rib with the sternum, and but slightly more so at the junction of the sixth rib with the sternum. The mean of the experiments gives about $\frac{5}{8}$ of an inch, the variation being from $\frac{3}{8}$ to $\frac{7}{8}$ between a state of rest and that of deep inspiration. (An explanatory diagram was here shown.)

The difference between expiration and a state of rest, in either of the above measurements, is about the same as between rest and inspiration, the latter being somewhat in excess, especially at the lower true ribs.

The lateral diameter of the chest, in deep inspiration, is augmented, absolutely as well as relatively, to a greater degree than the antero-posterior, in the proportion of 6 or 7 to 5, opposite the fifth or sixth rib; but the maximum increase appears to be attained at about the eighth rib, where the mean is as 10 to 5.

The movements of the sternum follow those of the costal cartilages, and are dependent on them. These movements, which occur in forced inspiration only, consist in an alternate advance and sinking of the bone, which is most marked at its lower extremity.

23. The first rib is certainly capable of elevation during inspiration, and the anterior and middle scalenus muscle performs this office: probably its agency is limited, in ordinary inspiration, to fixing the rib. In forced inspiration, the action of the clavicular portion of the sterno-mastoid is first apparent, and subsequently that of its sternal portion.

24. In ordinary expiration no muscular action is exerted; but in extraordinary expiration the ribs are drawn down by the abdominal muscles, at the same time that the abdominal viscera are pressed upwards. The intercostals assist in this act when the lower ribs are thus fixed. The levator ani is an essential auxiliary in forced expiration. I do not attach much value to the agency of the triangularis sterni and serratus posticus inferior as muscles of expiration. I am disposed rather to regard them as agents in steadying the ribs for the diaphragm, and as antagonists, severally, to the pectoral and latissimus dorsi muscles, thereby affording them a more fixed attachment at their origin.

25. The generally received assertion that there is a marked difference in the respiration of the two sexes, *i. e.* man and woman, confirmed, apparently, by the observations of Hutchinson on young girls, has always struck me as very remarkable; for, whatever hypothesis may be adduced to favour the relative advantage of such peculiarity, as regards woman, there certainly is no anatomical difference, either in the *natural* osseous conformation of the chest or in the muscular apparatus of respiration, to account for the predominance of the abdominal type of breathing in the male, and of the thoracic type in the female.

It naturally occurs to any one, in contemplating this circumstance, to attribute something to the peculiar dress of civilized women—the fashion of compressing the lower part of the chest, which is universal. There can be no doubt that this compression, commenced as it is at a very early period amongst *all* classes, long before the development of the skeleton is completed, must exercise a permanent influence in altering the form of the chest, and must thus impart a factitious reality to that which is not natural. I believe that the early age at which this compression is begun, even amongst the lower orders, as I have ascertained by inquiry, has possibly misled so careful an observer as Hutchinson. I have repeated his experiments on young boys and girls, but with results at variance with those which he obtained. These results may be thus stated.

26. In *ordinary respiration* the upper costal respiratory movements are equal in male and female; they are increased in both by girding the abdomen and lower ribs with a roller. The lower costal movements are also equal in the two sexes, and are not perceptibly affected by the action of the roller.

The difference in the thoracic girth between ordinary expiration and inspiration is very slight, indeed scarcely perceptible.

27. In *extraordinary respiration* the costal movements, both upper and lower, are much greater in forced inspiration in the male than in the female. The lower costal movements are much interfered with, in both, by the compression of the abdomen and lower part of the chest, while the upper costal movements are exaggerated.

I may add that, in the adult female, there is, as might be expected, a striking difference in the relative mobility of the chest and abdomen, when

the respiratory movements are observed whilst the usual tight dress is worn, and when the figure is at liberty to expand more naturally on removal of the dress.

28. Moreover, the form of the bust in the female lends a further aid to the influence of dress, by exaggerating the thoracic movements. Thus in stout women with full bosom, the rise and fall of the chest is much more apparent than in those in whom the bosom is spare and ill developed. This may be partly accounted for by the greater interference which is due to the increased compression exerted to reduce the size of the waist by stout women, but not wholly so. An artificial arrangement on the upper part of the chest of the male, by which prominence is given to this region, when the waist is compressed, at once exhibits distinct thoracic movements which were not otherwise perceptible. I am disposed, therefore, to believe that the asserted natural difference in the respiration of the two sexes is due to the altered form of the chest, consequent on compression, and to the habitual confinement of the lower costal region, which necessitates the habit of thoracic breathing. It is to be regretted that experiments to determine this question have not been conducted in some uncivilized community, which has not attained to the cultivated refinement of endeavouring to improve the natural symmetry of form at the expense of health and comfort. I am not aware that any such attempt has yet been made.

P.S.—Since writing the above, I have had the opportunity of conversing with M. Duchenne, who performed some experiments with electro-magnetism at St. Thomas's Hospital to demonstrate the action of various muscles.

He expressed himself strongly respecting the action of the intercostals as exclusively muscles of inspiration. This opinion is based partly on experiments performed upon decapitated criminals, and partly on clinical observations in cases of muscular atrophy. Immediately after death M. Duchenne applied the stimulus of an interrupted current to the intercostal muscles, with the result of raising the ribs. He stated that he isolated as far as possible the external from the internal set of muscles, and found stimulation of either set produced the same result. I give these statements as I received them, but I did not witness the experiments referred to.

The following are the chief points, either disputed or not previously recognized, which it is the object of the preceding observations and experiments to establish:—

1. The normal state of the diaphragm, when at rest, is that of arched tension; and this condition is due to the elasticity of the lungs resisting the atmospheric pressure on its thoracic surface.

2. This tension is such that the diaphragm cannot be forced upwards whilst the ribs are fixed; and is exerted in drawing the ribs inwards when the intercostal muscles are relaxed.

3. The results due to this passive tension are that (*a*) it retains the supplemental air in the lungs; (*b*) it limits the encroachment of the abdominal viscera on the thoracic cavity; (*c*) by virtue of the attachment of the pericardium to the cordiform tendon, the uniform calibre of this bag is secured, and the heart is thus protected from being impeded in its movements during respiration, the crura taking part in maintaining this condition when the muscle contracts; (*d*) it economizes active power in inspiration.

4. *Inspiration.*—When the upper ribs are fixed by the scaleni, both sets of intercostal muscles act in increasing the transverse diameter of the chest, by raising the curve of the ribs and the sternum.

They rotate the ribs outwards.

They fix the thoracic walls, and thus antagonize the tendency of the diaphragm to draw inwards the ribs to which it is attached. A fixed circumference is thereby secured, from which the diaphragm acts in altering its own form; and this is one of the most important functions of the intercostal muscles.

They assist in expiration when the lowest ribs are fixed by the abdominal muscles.

5. The scalenus anticus and posticus (especially the former) are auxiliaries in inspiration by raising and fixing the first and second ribs, and thus rendering them relatively immovable.

Thus, when the scaleni act, the intercostal muscles raise the ribs; when the scaleni are at rest, and the abdominal muscles act, the intercostals depress the ribs; if neither the scaleni nor abdominal muscles were to act, the ribs would be approximated at their centre by the action of the intercostal muscles.

6. The pectoralis minor (when the coracoid process is fixed), the lower costal portion of the pectoralis major, and some other muscles of minor importance might assist in elevating the ribs; but it is questionable whether they ever do so in such way as to assist in inspiration.

7. The serratus magnus has no action in inspiration; all but its lowest digitation must draw the ribs downwards, if they act on them at all.

8. The action which the sterno-mastoid is capable of exerting in inspiration is by fixing the first rib through the medium of the clavicle, and by raising the sternum. This is not required in health, but may be witnessed occasionally in disease.

9. *Expiration.*—Ordinary expiration is accomplished by the elastic resiliency of the lungs, the tense diaphragm resuming its arched form when the muscle ceases to contract: the elasticity of the ribs and abdominal parietes may assist to a limited extent; after a deep inspiration this elasticity has a more important share in expiration. The abdominal muscles, which, conjointly with the levator ani, are the agents of forced expiration, do not act by urging the abdominal viscera against the *tense* diaphragm, which would resist the pressure, to the injury of these viscera,

but against the *relaxed* diaphragm—relaxed, that is, by the abdominal muscles drawing down the lower ribs, and thus contracting the circumference of the lower part of the chest.

The intercostal muscles also contribute importantly to this result, as the effect of their contraction is reversed, by the lower ribs being relatively fixed during the action of the abdominal muscles. In this way both the long diameter and circumference of the chest are abridged.

10. The upper and lower costal movements in both sexes, when entirely unfettered, are equal, in ordinary inspiration and in the uncontracted chest.

The costal movements, both upper and lower, are much greater in forced inspiration in the male than in the female.

In both, the lower costal movements are much abridged by compression of the abdomen and lower part of the chest, while the upper costal movements are exaggerated.

The observed fact that women breathe more by the chest than by the abdomen is due to artificial compression, and to the altered form of the chest consequent on its early adoption.

February 1, 1872.

Sir JOHN LUBBOCK, Bart., Vice-President, in the Chair.

The following communications were read :—

I. "On the Lunar Variations of Magnetic Declination at Bombay." By CHARLES CHAMBERS, F.R.S., Superintendent of the Colaba Observatory. Received December 4, 1871.

(Abstract.)

This paper is in continuation of that "On the Solar Variations of Magnetic Declination at Bombay," published in the Philosophical Transactions for 1869; but the discussion is confined to the observations of the years 1861 to 1863, which alone have as yet been reduced. The point of principal interest brought out in the discussion is that whilst the mean lunar-diurnal variation is of the ordinary character, having as its principal feature a double oscillation in the lunar day, its range is very small as compared with the several ranges of the lunar-diurnal variations when the sun and moon have several specific varieties of relative position; and moreover, although in those latter variations the double oscillation is generally preserved as a main feature, correspondence of phase in the representative curves is as generally absent; and in some cases the curves are, whilst systematic, altogether different in character from the mean lunar-diurnal variation curve. The semiannual inequality in the lunar-diurnal variation,

whilst it is as definitely systematic, has twice the range of the mean lunar-diurnal variation; and it is also subject to remarkable modifications which accompany changes of phase of the moon.

II. "Note on a possible Ultra-Solar Spectroscopic Phenomenon." By C. Piazzi Smyth, F.R.S., Astronomer Royal for Scotland. Received December 13, 1871.

One great object with the solar-eclipse expeditionists at work to-day in the far East is to trace spectroscopically the existence of any faint solar luminous appendage to a further distance from the sun than the brighter parts of the corona hitherto so identified.

But *much* further they cannot go, on account of the large amount of general atmospheric illumination during every lunar-solar eclipse. The matter may, however, be taken up again during a *terrestrial* solar eclipse, *i. e.* an ordinary sunset below the horizon, if the sun be sufficiently far below to terminate *all* aërial twilight. Under such circumstances, too, it is that the zodiacal light, historically called the sun's atmosphere, is occasionally seen stretching away to distances of 60, 90, and even more degrees from the sun.

Now I have recently been trying to test in this manner an alleged foreign observation of the zodiacal light's spectrum, but in vain. Some sort of faint bluing of the dark night sky there certainly was over the north-western horizon when the sun was more than 18° vertically below, and its spectrum I noted at the time, but only as being that of the very last of the twilight.

Perhaps it was only that; but perhaps, also, it may have been the direct light of some solar appendage approximating to the outer coronal region; for, on making up the accompanying plate* of nine observed spectra, and comparing them with a tabular solar spectrum, a wide distance was manifested between the place of maximum light in the solar continuous spectrum, as given by Fraunhofer, or near wave-length 5620, and in my so-called residual twilight spectrum, which was wave-length 5300.

I tested, therefore, lately Fraunhofer's value for the solar spectrum in my own night-apparatus, by darkening its objective to all but extinction with four thicknesses of linen cloth in the middle of a dull, grey day, and found for the last visible portion of continuous spectrum wave-length 5700 nearly; that is, Fraunhofer's value was confirmed, and my residual-twilight spectrum left anomalous as regards the ordinary solar spectrum, but remarkably agreeable with the corona spectrum, whose chief line has wave-length 5322, and which spectrum has been shown by Mr. Lockyer to decrease its number of lines, and tend to form a faint continuous spectrum, as it thins out with increase of distance from the sun's limb.

* See plate 54, vol. xiii., "Astronomical Observations made at the Royal Observatory, Edinburgh."

III. "On the Normal Paraffins." By C. Schorlemmer, F.R.S. Received December 20, 1871.

(Abstract.)

Some of the results of this research have already been published in two previous communications*. It was there pointed out that the constitution assigned to the normal paraffins (*i. e.* that they contain the carbon atoms linked together in a single chain) was ascertained partly by preparing them by synthesis from other normal compounds, and partly by studying the oxidation products of the alcohols obtained from them. The best method to prepare these alcohols is to pass a current of dry chlorine into the vapour of the boiling hydrocarbon; a mixture of a primary and a secondary chloride is obtained†, and these, by heating the mixture with glacial acetic acid and potassium acetate to 200°, are completely decomposed, the primary chloride yielding the corresponding acetate, whilst the secondary compound partly splits up into an olefine and hydrochloric acid, and partly is converted into the acetate of the secondary radical. By treating the acetates with an alcoholic solution of caustic potash, the alcohols are formed, which can be only approximately separated by fractional distillation, as the difference between their boiling-points is only about 10°.

Pentane or normal amyl hydride, C_5H_{12}, boiling at 37°–39°, is found in considerable quantity in Pennsylvania petroleum. The secondary pentyl alcohol or *methyl-propyl carbinol*, $\left.\begin{matrix} CH_3 \\ C_3H_7 \end{matrix}\right\} CH\,OH$ (boiling-point 120°–122°), gives on oxidation *méthyl-propyl ketone*, $\left.\begin{matrix} CH_3 \\ C_3H_7 \end{matrix}\right\} CO$, which on further oxidation splits up into acetic acid and propionic acid. The *primary pentyl alcohol* is identical with the normal amyl alcohol, which Lieben and Rossi obtained from normal butyric acid, and yields on oxidation *normal valerianic* acid, boiling at 184°–187°.

Hexane or normal hexyl hydride, C_6H_{14}.—(1) *Hexane* from petroleum, boiling at 69°–70°, yields the following derivatives:—(a) *Methyl-butyl carbinol*, $\left.\begin{matrix} CH_3 \\ C_4H_9 \end{matrix}\right\} CH.OH$ (boiling-point 140°–142°), the oxidation products of which consist of *methyl-butyl ketone*, $\left.\begin{matrix} CH_3 \\ C_4H_9 \end{matrix}\right\} CO$, and acetic acid and *normal* butyric acid. (b) *Primary hexyl alcohol*, boiling at 150°–155°, from which *caproic acid*, boiling at 201°–204°, was obtained.

(2) *Hexane* from mannite was obtained by acting with hydrochloric acid and zinc upon the secondary hexyl iodide prepared from mannite. It boils at 71°·5, and its specific gravity at 17° is 0·6630. The deri-

* Proc. Roy. Soc. vol. xix. pp. 20 & 487.

† The same mixture is obtained by the action of chlorine in the cold, or in presence of iodine; but at the same time a large quantity of higher chlorinated substitution products is formed, which is not the case by acting with chlorine on the vapour.

vatives were the same as those of the hexane from petroleum; it must, however, be stated that the boiling-points of some were a little higher than those of the petroleum hydrocarbon, and there was also a marked difference observed between the two caproic acids. That from mannite gave a well-crystallized barium-salt, whilst that from petroleum could only be obtained in the amorphous state; but as the two secondary alcohols yield both normal butyric acid, the chemical constitution of the two hexanes must be the same.

(3) *Dipropyl.*—This hydrocarbon was prepared by acting with sodium on primary propyl iodide; boiling-point 69°–70°; specific gravity at 17°=0·6630. The quantity obtained was too small for further investigation; but the mode of its formation shows that it must have the same constitution as the two other hexanes.

Heptane or normal heptyl hydride, $C_7 H_{16}$; boiling-point 97°·5–99°.—This hydrocarbon, which is also found in petroleum, gives a secondary alcohol (boiling-point 160°–162°), which is *methyl-pentyl carbinol* $\left.\begin{matrix} C\,H_3 \\ C_5\,H_{11} \end{matrix}\right\}$ CH.OH, as the acetone obtained from it yields on oxidation acetic acid and *normal valerianic acid.* The primary heptyl alcohol boils at 170°–172; on oxidizing it, *œnanthylic* acid, boiling at 219°–222°, was formed, which was found to be identical with the acid obtained from castor-oil.

Octane or normal dibutyl, $C_8 H_{18}$, is easily obtained by the action of sodium on normal butyl iodide. It boils at 123°–125°, and has at 17° the specific gravity 0·7032. As the octane from methyl-hexyl carbinol, as well as that which Zincke obtained from primary octyl alcohol, have the same boiling-points and specific gravities, it appears most probable that these three hydrocarbons are identical.

IV. "Note on the Eclipse of the Sun (Dec. 1871) as observed at Sholoor." By M. Janssen. Communicated by the President. Received January 15, 1872.

Monsieur le Président,—J'aurai l'honneur d'adresser à la Société Royale de Londres un mémoire détaillé de mes observations de l'éclipse, mais je profite du départ de ce courrier pour vous informer des principaux résultats obtenus.

Sans entrer dans une discussion qui fera partie de ma relation, je dirai d'abord que la magnifique couronne observée à Sholoor s'est montrée sous un aspect tel, qu'il me paraissait impossible d'admettre ici une cause de l'ordre de phénomènes, ou de diffraction, ou de réflexion sur le globe lunaire, ou encore de simple illumination de l'atmosphère terrestre.

Mais les raisons qui militent en faveur d'une cause objective et circumsolaire, prennent une force invincible quand on interroge les éléments lumineux du phénomène.

En effet, le spectre de la couronne s'est montré dans mon télescope, non pas continu, comme on l'avait trouvé jusqu'ici, mais remarquablement complexe. J'y ai constaté: les raies brillantes quoique bien plus faibles du gaz hydrogène qui forme le principal élément des Protubérances et de la Chromosphère;

La raie brillante verte qui a déjà été signalée aux éclipses de 1869 et 1870, et quelques autres plus faibles;

Des raies obscures du spectre solaire ordinaire, notamment celle du sodium (D). Ces raies sont bien plus difficiles à apercevoir.

Ces faits prouvent l'existence de matière dans le voisinage du soleil, matière qui se manifeste dans les éclipses totales par des phénomènes d'émission, d'absorption et de polarisation.

Mais la discussion des faits nous conduit plus loin encore.

Outre la matière cosmique, indépendante du soleil, qui doit exister dans le voisinage de cet astre, les observations démontrent l'existence d'une atmosphère étendue, excessivement rare, à base d'hydrogène, s'étendant beaucoup au-delà de la Chromosphère et des Protubérances, et s'alimentant de la matière même de celles-ci, matière lancée avec tant de violence à travers la photosphère, ainsi que nous le constatons tous les jours.

La rareté de cette atmosphère, à une certaine distance de la chromosphère doit être excessive; son existence n'est donc point en désaccord avec les observations de quelques passages de comètes près du soleil.

J'ai l'honneur d'être,

Monsieur le Président,

Votre respectueux et dévoué serviteur,

JANSSEN.

February 8, 1872.

GEORGE BIDDELL AIRY, C.B., President, in the Chair.

The Right Hon. George Joachim Goschen was admitted into the Society.

The following communications were read:—

I. "A Letter from His Majesty the Emperor of Brazil," addressed to General Sir EDWARD SABINE, K.C.B.

Paris, 31 January, 1872.

DEAR SIR,—I am highly pleased by the honour that the Royal Society of London has conferred upon me, electing me as one of its Fellows, and very happy that the Statutes allow the intrusion into such a learned body to a Foreign Prince whose merit only consists in being an admirer of science and a friend to its noble interpreters.

I beg you, Sir, to present my hearty thanks to your colleagues, that I feel proud to call now *our colleagues*.

(Signed) D. PEDRO D'ALCANTARA.

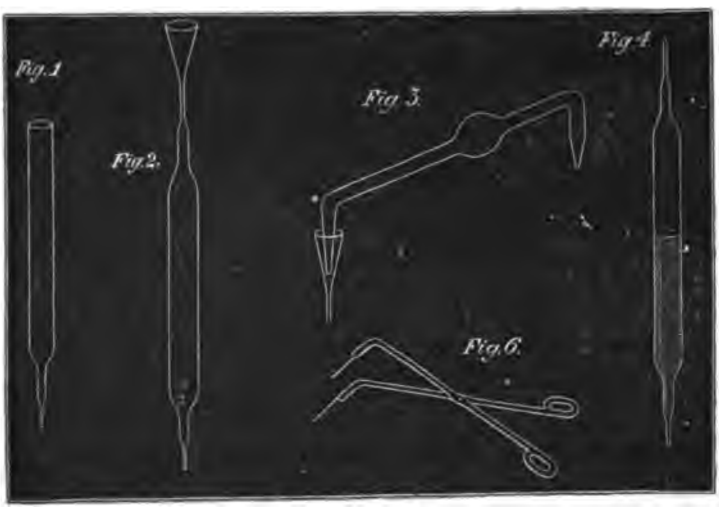
II. "Experiments concerning the Evolution of Life from Lifeless Matter." By WALTER NOEL HARTLEY, F.C.S., Demonstrator of Chemistry, King's College, London. Communicated by W. ODLING, M.B., F.R.S. Received December 7, 1871.

The work already accomplished, and the arguments adduced both in favour of and contradictory to the theory of spontaneous generation, have been so frequently under discussion of late, that it is needless to enter on a review of them. Furthermore, the question is one in which verbal argument is of little value compared with experimental evidence.

On June 30th, 1870, there appeared in 'Nature' a paper by Dr. Bastian, entitled "Facts and Reasonings concerning the heterogeneous evolution of Living Things;" the perusal of this, and its continuation, led to the belief that another interpretation might be put on the results obtained by Schwann, Pasteur, and others, not so much by virtue of the arguments made use of, as by accounts of experiments given in detail. The most remarkable case was that of Exp. 19, in which the author gave a drawing of a large organized mass obtained from a solution of sodium phosphate and ammonia tartrate, which had been exposed to a temperature varying between 146° C. and 153° C. for four hours. This organism was seen to grow within the flask till it attained a certain size, beyond which it did not increase. Now a fact so distinctly stated as the production of an organism, and its development to a considerable size, from a liquid containing nothing further than phosphate of soda and tartrate of ammonia, in a flask from which the air had been most thoroughly withdrawn, and which, when containing the liquid and hermetically sealed, had been heated to so high a temperature, was (admitting the conditions and performance of the experiments to be faultless) an absolute proof of the evolution of living matter *de novo*. For my own satisfaction, I determined to commence a series of careful experiments, in some cases adhering strictly to the conditions of those made by Dr. Bastian; but it was necessary to devise some refinement on the mode of examining the liquids experimented on without exposure to atmospheric air; the means for accomplishing this I will now describe. The most promising plan seemed to be, to open the sealed vessels in an atmosphere artificially prepared so as to be free of living matter. Hydrogen being fourteen times lighter than common air, may remain in contact with it without risk of contamination by floating matter; indeed Prof. Tyndall's demonstration, by means of a powerful beam of light, that such an atmosphere is free from dust, was sufficient to warrant its use. The means whereby this fact was made of further practical value are the following:—

1st. *The experimental tubes* in which the infusions and solutions were heated were made of ordinary combustion tubing drawn out at the lower end, first to a finer tube $\frac{1}{4}$ the diameter of the original, and after a space of an inch or so to extreme smallness. The solution or infusion was then made in a flask with distilled water, drawn by a siphon from a carboy

after standing at rest for many hours, the siphon dipping into the middle of the liquid. The flask was, after the usually careful cleansing that chemical vessels require, rinsed out with a solution of potassium dichromate mixed with strong sulphuric acid, then washed out with distilled water. A further quantity of distilled water collected in this vessel was used for the solution. The experimental tube, with the lower end drawn out but open, was cleansed with acid dichromate, and afterwards with hot distilled water. The fine point was open, in order to let the liquids run through, otherwise a drop might have collected in its capillary portion, and, not being dislodged, would have interfered with the experiment. This tube then, best described by fig. 1, had the fine end sealed up, and the liquid to be experimented on poured in till it about half-filled the tube. The upper part was then drawn out, so as to serve a purpose yet to be named, and also in such a manner that it could be adapted to the Sprengel exhauster; after this operation it had the form of fig. 2. It was then fitted to the Sprengel by means of a tube shown in fig. 3, which admitted of connexion by means of two Sprengel joints, the one over the experimental tube being made air-tight with water. The reason for water being used was this: had there been a leak, water would have entered the tube, and so no damage would have resulted; but had it been mercury or glycerine, the tube would necessarily have been rejected. The use of the bulb on the connecting-piece was to catch the water which boiled or distilled over during the exhausting process. After complete exhaustion, recognized by the clicking of the falling mercury, a blowpipe-flame was cautiously applied



to the fine tube till it fused thoroughly, and it was then drawn away from the other portion. What was so far accomplished was the sealing of a solution or infusion in a vacuous tube of the annexed form (fig. 4). The

tubes thus prepared were heated in an air-bath, consisting of horizontal iron pipes surrounded by two iron jackets. The tubes lying horizontally were not in contact with the liquid on the upper part; so, after being heated in the one direction for a period, at Prof. Tyndall's suggestion they were turned over and heated anew, so as to bring every part of the tube into contact with the heated liquid. These tubes had generally a bead of glass fused to one side, so that if the tube were heated with the bead uppermost, it had to be turned over and heated again, the bead being downwards. Generally four tubes were heated at a time; and one of these was soon after cooling opened to allow access of air, in order to observe whether any change occurred differing from any that might take place in the sealed tubes.

2nd. *Apparatus for examining the contents microscopically out of con-*

tact with air.—This consisted of a bell-jar closed at the top with a bung, and supported on a tripod; this bell-jar was kept filled with hydrogen by means of a self-regulating apparatus continually passing a gentle stream of gas into the upper part of the jar by means of a glass tube. The bung was bored with an eccentric hole; through this passed the tube containing the liquid for examination; the end of the tube passed very little below half-way down the bell-jar. Under this was a small tripod, on which rested a glass plate to be used as a stage for the microscope slips. The tube then being *in situ*, over the upper point was tightly slipped a piece of non-vulcanized india-rubber tube connected with a constant hydrogen generator. It was not deemed advisable to make use of coal-gas, because, had any lifeless organism been found in a tube, it might have been objected that a trace of benzole or naphthalene vapour or other impurity had been fatal to the experiment. On the india-rubber tube were two burette clips; now, by breaking the fine point within the india-rubber (a scratch with a

file being previously made upon it), hydrogen flows into the vacuous tube. Both clips are now closed, and by means of forceps, the hand being beneath the bell-jar, the lower end of the tube is broken off. No liquid, however, escapes, because the internal pressure is not much in excess of that of the atmosphere. The condition of things now can be explained only by the aid of a drawing (fig. 5): the whole arrangement consists of a pipette containing the experimental liquid above and below, which is an atmosphere of hydrogen; each drop of liquid expelled is received on a slip of glass in such an atmosphere. A drop of liquid is deposited by squeezing the space of india-rubber between the two clips, that clip nearest the bell-jar being open; before removing the pressure, it is again closed, and the partial vacuum made by compressing the india-rubber is filled up by allowing gas to flow in from the apparatus. This precaution prevents the possibility of the atmosphere of the bell-jar bubbling up into the tube after a little fluid has been discharged.

3rd. *Treatment of the glass slips.*—These are heated in an air-bath to about 200° C., taken out while hot with forceps, and placed on the stage in the hydrogen to cool, and kept there till wanted.

4th. *The glass covers.*—These are washed in the acid-dichromate solution, then in distilled water, and finally in alcohol, and, being picked up by a pair of peculiar forceps, are heated over an argand burner till very hot; they are then held in the glass vessel full of gas till cool enough to use. The forceps I devised (fig. 6) have points of watch-spring steel, so that a thin glass cover may be firmly gripped without breakage. They are made by cutting a small pair of crucible tongs an inch or so before the part where they bend at right angles; they have then two pieces of thin brass rivetted on, which are bent at right angles an inch or so from their ends; the points are made by rivetting on pieces of watch-spring steel a little more than an inch in length. When these tongs are held in the position of scissors, that is to say with the thumb above the fore finger, the ends point downwards. To take up a glass cover, the forceps being in the position mentioned, the wrist is turned over from right to left while the elbow is raised, the glass resting on the lower point while the upper is closed down on it, may be safely held and conveyed to where it is wanted. After a little practice these pincers are easily used.

The advantage of the bung closing the neck of the bell-jar being bored eccentrically is, that by simply turning the bell-jar horizontally the pipette point delivering the liquid may be shifted from a glass slip on which a drop of liquid has been deposited to another clean one, or be made to turn through such an angle as to be out of the way of the glass stage, in order to transfer the solution to a flask for further experiment.

Tartrate of ammonia was prepared by neutralizing a tartaric-acid solution with aqueous ammonia; this was mixed with phosphate-of-soda solution, made by dissolving carefully washed crystals of the salt in hot distilled water. The mixture containing about 5 per cent. of the two salts was

slightly acidified with tartaric acid. It was found that in no case should filtering be resorted to if possible, as the finest Swedish paper transfers myriads of its fibres to the liquid. Paper of nitro-cellulose (that is to say, Swedish filter-paper treated with a mixture of sulphuric and nitric acids) is disintegrated to a less extent than ordinary paper; and it is possible that, for special purposes, it might be of service in filtering a liquid so as to render it clear of fine particles, though no filtration will thoroughly clear a liquid. With proper care, however, filtration is unnecessary when dealing with solutions of definite salts.

On July 15th, 1870, a tube was half-filled with the above 5 per cent. solution of the mixed salts, and exhausted by the Sprengel. On trying whether it acted as a water-hammer on the following day the tube was split into fragments. The tubes require very careful handling, as the pointed ends give way readily.

July 16th, 1870.—Three other tubes were sealed up with the same liquid, the exhaustion in each case being perfect. The glass was heated to redness before introducing the liquid. After heating to 150° C. for four hours (during a few minutes the temperature accidentally rose to 180°), the tubes in a perpendicular position were placed at rest on a shelf, where the temperature was about 25° C.

Exp. I. The liquid, at first clear, after the lapse of three days had deposited flocculi. On October 7th one of the tubes was opened in the apparatus, and with the care already described. The gelatinous matter, on examination by a microscope magnifying 400 diameters, showed no signs of organization or structure of any kind. A small quantity was collected on a very small Swedish filter, and removed by means of a small platinum spatula. On heating it did not char, and was therefore inorganic matter; the usual blowpipe tests showed it to be silica. The disodic phosphate had attacked the glass; the silica deposited on standing, and hence the jelly-like mass. The remainder of the liquid was examined, but no trace of an organism could be detected. This tube had been kept exactly 84 days, during which time its temperature ranged between 18° and 25° C.

On July 21st, 1870, three other tubes containing freshly prepared ammonium tartrate and sodium phosphate were sealed up, the liquid being simply boiled to expel air; these were heated to 110° C. for three hours. Range of temperature 18° to 25° C.

Exp. II. No. 1 was opened October 7th, and examined with the same microscopic power. No living organism, and no trace of organic matter even, beyond a cotton-fibre and one or two small patches of indefinite and shapeless matter, which came from the solid residue of the liquid adhering to the glass within the capillary tube being charred in the act of sealing up. Several drops of liquid were examined at the lower point, the centre, and the surface, with the above results in the first case, but in the latter portions the liquid was absolutely clear. This tube had been kept 78 days.

Exp. III. No. 2, July 21st, 1870. Tartrate of ammonia and phosphate

of soda. The examination was made in the same manner. No trace of life was discernible. Time kept 78 days. Heated to 110° C.

Exp. IV. Tube containing phosphate of soda and tartrate of ammonia, perfectly exhausted. Sealed on August 6th, 1870. Heated to 140° C. for four hours. Opened October 8th.

Seven drops of liquid were taken from the tube, but no trace of living matter was discernible. The first drop contained a cotton-fibre and a small brown mass, identical in nature with that before mentioned. On October 12th a renewed examination was made with a higher power, $\frac{1}{8}$ inch, by Ross, kindly lent by Mr. Savory, which, with the eyepiece used before, gave an enlargement of about 600 diameters. A diligent search revealed nothing beyond what was before noticed. The contents of the tube were then allowed to run into a flask cleansed first with acid bichromate and then rinsed twice or thrice with hot distilled water, and finally dried in an air-bath. The flask and its contents were closed against dust of a coarse kind, which, though not floating in the air, might have fallen into the liquid. By inverting a small beaker over its mouth, it was allowed to remain with the unopened tubes at a temperature of 25° C. On November 3rd a small fungoid growth was noticed in the liquid, and on November 7th this had increased to $\frac{3}{4}$ inch in diameter. It proved to be a mass of *Pencillium* with abundance of mycelium-filaments interlaced. The fellow tube, sealed on August 6th and unopened, had nothing in it that the eye, aided by a lens and a powerful light, could detect. Several other tubes were prepared, but no further examination was made with the microscope. In no case, however (and some of the tubes had been sealed nearly six months), was any sign of life perceptible.

Unfortunately a very serious illness for some time prevented the continuance of my experiments. This work was done in the chemical laboratory of the Royal Institution. I cannot, therefore, omit giving my best thanks to Dr. Odling, and also to Prof. Tyndall, for their kindly interest and advice.

Part II.

Modification of Experiments.

In the renewed examination of liquids kept some time in sealed tubes, commenced in July 1871, a slight modification in the original method of proceeding was used. A bell-jar was chosen, the upper mouth of which was ground perfectly flat at the edge. Instead of inserting a bung with a hole in it to receive the tubes, a metal disk, with a wide metal tube placed eccentrically and projecting half an inch, was luted on to the mouth of the jar by means of grease, or, better still, what is known in pharmacy as *resinæ ceratum*. The glass sealed tube was then slipped into an india-rubber conical stopper, or rather ring *; for the thickness of it was so slight that

* These things are made and sold for the purpose of fixing the taps into beer-barrels.

a tube of any size could be made to fit it, either by the india-rubber stretching when the tube happened to be large, or by binding with a piece of copper wire when it fitted loosely. The pipette was scratched with a file at each end, and over the upper one was slipped a piece of india-rubber tube, attached to a tube of glass about $\frac{1}{4}$ inch in bore and 4 inches long, tightly packed with cotton-wool. The caoutchouc tube was pinched by a burette clip, and the extremity of the tube enclosed by the caoutchouc was broken at the file-mark. The vacuum was considered good if the india-rubber tube collapsed completely; the burette clip was opened, and filtered air thus admitted into the vacuous space. In order to render any thing that might be attached to the interior of the india-rubber tube harmless to the experiment, it was dipped in glycerine and the glycerine squeezed out of it, or treated in the same way with melted bees'-wax or paraffin. The pipette fits into its place in the disk by means of the flexible stopper. By closing the burette-clip, the tube can be broken at the lower point without more than a drop or two of the liquid escaping. After about one third of the liquid had been examined, one half of the remainder was allowed to run into a flask which had been previously heated to between 200° and 300° C.* The tube was then removed, and the fine capillary point, when possible, sealed at a gas-flame. The finer the point the more easily is this accomplished. A portion of liquid remains in the tube. On heating rather strongly a little of this is driven out, and then no air can pass to the remaining liquid without passing over red-hot glass, which readily melts together. The tube and flask were then placed side by side in a warm place to undergo further observation. If the tube, or class of tubes, were called A, after opening it was labelled A′, and the liquid out of it exposed to the unfiltered air, A″. The tubes and flasks labelled thus were kept in a cupboard, the bottom of which was the metal lid of a long water-bath. It was thought better not to place the flasks or tubes in water, because the aqueous vapour which would thus surround the mouths of the flasks would create an abnormal atmosphere which might or might not affect the experiments; besides, such a plan is not so cleanly. The objective made use of was obtained from Messrs. R. and J. Beck. It was a $\frac{1}{8}$ glass, without any immersion-arrangement, and gave, with the second eyepiece of one of their microscopes, a magnifying-power of 750 diameters. Occasionally, for convenience in drawing, a power of 420 diameters was employed.

Method of examining a liquid which it was difficult to retain in the pipette-tube.

When it happened that the finely drawn-out end of the pipette was too large to retain the liquid, it was allowed to run into a small glass vessel, really a beaker cut down so as to measure about $1\frac{1}{2}$ inch in diameter and 1 inch high. Drops of the solution were removed from this to the glass slides while it stood on the glass stage in the jar of hydrogen, by means of

* That is to say, baked in an oven the bottom of which was red-hot.

a tube like a very long-legged siphon, at the lower end of which was a piece of caoutchouc tube, with one end stopped by a little piece of glass rod. This was nothing more than a bent pipette; by compressing the india-rubber when the pointed tip of the shorter limb was dipped into the beaker-glass, and then releasing it, the liquid entered for the space of an inch or so, and could then be easily transferred to a glass slip. It was thought as well to blow hydrogen through the tube before use; and, of course, like all the other apparatus, it was carefully washed and heated.

Preparation of solutions.

The water used was very pure distilled water taken from a carboy, the contents of which had been tested with a beam of light, and found to reflect chiefly the blue rays. A previous attempt to obtain pure water by distillation with sulphuric acid and potassium permanganate, in glass vessels and an atmosphere of hydrogen, did not yield better specimens. It is impossible to prepare solutions of salts which do not show abundance of floating matter to a ray of light, even when such pure water is made use of. Solutions filtered through the finest Swedish paper are crowded with fibres, which may readily be seen by filling a globular flask with the solution, the eye and an argand burner being on the same horizontal line, and about a foot apart. The flask is interposed, and gradually lowered till the particles are seen brilliantly illuminated on a dark ground. The phosphate of soda used was recrystallized immediately before being dissolved, and the tartrate of ammonia was prepared from recrystallized tartaric acid and the strongest aqueous ammonia. When the solutions were mixed, the alkaline reaction was neutralized by tartaric acid, or rendered faintly acid. Of course it is of the first importance that the tubes, after being sealed, should be heated immediately to the temperature necessary to destroy life, and this was done in every case as soon as possible.

Accidental occurrence of a lifeless organism in a phosphate-of-sodium and ammonic-tartrate solution.

On examining the contents of tube A 2, several drops of liquid were found to contain nothing whatever. They had filtered through the gelatinous silica deposited in the capillary point, so that a fresh portion of the glass had to be broken off and a little of the liquid allowed to run into a bottle. After this a little débris was occasionally noticed, and in one drop of liquid an animalcule was found, measuring 0·003 inch in length. I failed to recognize it, probably from its being injured by the action of a liquid at so high a temperature as 150° C. Its presence in the solution is more easily accounted for than if it had been found in any of those prepared subsequently; for in this case the water was not tested by means of a beam of light, neither was the glass tube (although well washed with boiling water) cleansed with such very great care. There was much less débris of the nature of cotton-fibres and other indefinite matter found in the tubes

prepared subsequently. It is at any rate satisfactory to know that it was lifeless, and the only thing of the kind met with. It is impossible to get glass apparatus quite clean by means of hot water; those in the habit of making organic analyses are acquainted with the large amount of dirt which can be swept out of a tube by a plug of filter-paper or cotton-wool, after washing with abundance of boiling water.

Experimental Results.

The whole of the tubes, an account of which here follows, were kept for sixteen weeks at a temperature of 30° *to* 34° C. *during the daytime, and not lower than* 20° C. *at night. A fluctuating temperature is considered by Dr. Bastian to be favourable to evolutional changes*. From the month of February till the time of examination the temperature would never be lower than* 16° C., *and was never higher than* 30° C.

A. A solution containing about 4 per cent. of a mixture of sodium diphosphate and ammonic tartrate, and having a neutral reaction, was placed in three tubes, which were exhausted with a Sprengel, sealed up, and heated for three hours to 150° C. Prepared on July 16th, 1870.

No. 1. No trace of any organized matter discovered; nothing but a little silica dissolved out of the glass. Opened October 18th, 1870. Time kept three months.

No. 2. Examined July 17th, 1871. Kept over a year. No living organism found. In this tube the animalcule already mentioned was met with. The liquid was turbid with silica.

No. 3. Examined September 2nd, 1871. Kept a year and six weeks. Nothing found. The silica makes these tubes troublesome to examine, on account of the capillary point tending to become stopped.

B. Tube prepared July 15th, 1870. Solution of salts, as in preceding experiments. Air expelled by boiling for ten minutes. Heated to 130° C. Kept one year and two months. Examined September 12th, 1871. Vacuum good. Nothing noticeable seen.

C. Same solution, but with decidedly acid reaction. Prepared August 8th, 1870. Opened July 17th, 1871. Kept ten months and twenty-five days. No trace of any organism; many drops of liquid absolutely free from any thing whatever. The lower end of this tube was a little too large, so that, in spite of the upper end being closed, the liquid would drop out. It was caught in a little glass vessel placed in the bell-jar, and drops were removed as required by dipping the point of the tube in and touching the glass slide.

D. October 10th, 1870. A strong infusion was made by pouring warm distilled water over finely shred turnips, and allowing to digest for some time. The liquid was filtered twice through the finest Swedish paper, and

* See 'Nature,' vol. ii. p. 177.

allowed to stand for some hours, when a portion was siphoned off, placed in tubes completely exhausted with a Sprengel pump, and sealed up. The tubes were heated to 120° C. for three hours. The tube marked T, and called the "test-tube" in these and all the following experiments, was treated in just the same way as the others, and heated at the same time to ascertain whether a high temperature had any prejudicial effect on the development of life.

No. 1. Opened July 14th, 1871. Kept nine months and one week. Vacuum perfect. Liquid perfectly clear; flavour and smell like that of fresh infusion. No change had taken place.

No. 2. Opened July 15th, 1871. Kept nine months and eight days. Vacuum perfect. Liquid quite fresh in smell and flavour, and clear in appearance. Quite unchanged.

No. 3. Opened August 4th, 1871. Kept over ten months and two weeks. Vacuum perfect. Liquid perfectly fresh in smell and flavour, clear in appearance. Quite unchanged.

No. 4, T. Opened October 12th. On 19th, in a space of seven days, the liquid had become turbid. On October 31st it was crowded with white matter, and had a very offensive smell. The microscope showed masses of *Torula*-cells. The magnifying-power used was 400 diameters.

The original solution, which had been kept covered by an inverted beaker over the mouth of the flask, was turbid six days after its preparation. As turbidity was noticed in the solution, which had been sealed *in vacuo* and heated to 120° C., in less than seven days (the tube being opened at night and examined in the morning) after exposure to the air, we see that heating had no interference with the experiment.

E. October 12th, 1870. Sodium phosphate and ammonic tartrate. Solution containing 3 to 4 per cent. of the mixed salts. Completely exhausted with a Sprengel pump. Heated three hours to 130° C.

No. 1. Examined August 24th, 1871. Kept ten months and two weeks. Vacuum perfect. Many drops of liquid free from any thing. Quite unaltered. Contained nothing noticeable.

No. 2. Air expelled by boiling. Vacuum good. Opened July 19th, 1871. Kept nine months. Remarkably little solid matter seen: no organism met with.

No. 3, T. Opened October 18th, 1870. Exposed to the air twenty days. Not examined till November 7th. Five masses the size of peas were seen floating on the liquid; they proved to consist of mucor, with fructification.

F. October 18th, 1870. Urine boiled and filtered from mucus. Each tube exhausted with the Sprengel. Heated to 130° C. for three hours. After heating, what is believed to be a trace of phosphate of lime separated.

No. 1. In February 1871 the liquid was perfectly clear and seemingly unaltered; when next examined the point of the tube was found to have been broken and the liquid had been lost. Kept about fourteen weeks.

No. 2. Opened August 29th, 1871. Kept over ten months and a half. Nothing unusual was seen in the liquid; it smelt perfectly fresh, and looked quite bright and clear.

No. 3. Opened July 19th, 1871. Kept nearly nine months. The liquid was quite fresh and clear. There were seen two or three minute bodies, too small for any definite observation to be made concerning their form. They had apparently an irregular rotatory slow motion, which continued; and careful observation led me to conclude it was only the Brownian movement. This conclusion was afterwards confirmed. See F′ 3.

No. 4, T. Opened October 19th, 1870. Both *Torula* and mucors were discovered in abundance after three weeks' exposure to the air.

Liquids which, after prolonged preservation in sealed tubes, were exposed to air filtered through cotton-wool, kept at a temperature of 30°–34° C. *during the daytime, and not below* 24° C. *at night.*

D′. No. 1. Examined on July 21st, and again September 7th, 1871.—The liquid was quite unchanged. Nothing found in it after a period of fifty-two days.

No. 2. Examined August 22nd and September 8th. No trace of any organism. Quite unaltered. Kept fifty-four days.

No. 3. As the first sign of any change occurring in the solution is turbidity, this tube was not submitted to microscopical examination. On September 8th it was still perfectly clear to the eye, apparently quite unaltered after more than a month.

E′. No. 2. Opened July 19th, examined September 13th, 1871, after a period of two months. Quite unaltered.

F′. Examined after exposure to air for five weeks, from July 19th to August 23rd, 1871.

No. 3. The liquid smelt quite fresh, and was unaltered in appearance. Only two of the minute particles previously mentioned were seen: they were most certainly lifeless, and also for the most part motionless; one was seen to move with a current in the liquid, but the rotatory motions before noticed did not occur. A most attentive examination for a length of time was devoted to these bodies; in size they could not have been larger than 0·00002 inch. The conclusion as to their being lifeless was borne out by the fact that their small number (only five were seen) had not increased, though under favourable circumstances for reproduction, even after a period of five weeks.

No. 2. Sept. 13. Liquid quite unaltered. Kept nine weeks.

Liquids which, after prolonged keeping without development of life, were afterwards exposed to ordinary air at a temperature ranging between 24° C. *and* 34° C. *Any pipette or glass rod placed in these liquids had immediately before been heated to at least* 200° C.

D″. Examined July 18th, after exposure to the air four days. No. 1. The liquid was very turbid, and smelt very offensive; it swarmed with

vibriones and some excessively small bodies, all in rapid motion. An immense number of minute bodies, 0·000025 in length, consisting of two cells, which are believed to be *Torula* in an early stage, besides one or two full-sized *Torula*-cells, were noticed. On July 21st an immense quantity of *Torula* and some fibrous growth, most probably fungus or conferva, appeared in addition to the other minute organisms. The liquid was boiled violently for a few minutes, but not longer, as the liquid frothed much. The vibriones remained as lively as ever.

No. 2. Examined August 1st. Nothing observed; liquid unchanged. August 22nd the liquid had evaporated to some extent. A thick coating of *Mucor** and *Torula* covered the syrupy solution. Kept seventeen days without alteration; total time exposed to air more than five weeks.

No. 3. Opened August 24th. On August 27th the liquid was observed to have a very slight sediment; later on a downy growth was observed adhering to the side of the flask. On the following day, August 28th, the liquid, which had become very turbid, was examined. It contained immense quantities of the undeveloped *Torula*. None of these bodies were seen to move. Reexamined September 1st; the tuft of down proved to be conferva. On September 4th the liquid was a mass of *Torula*. The confervoid growth had increased very little; there was a quantity of protoplasmic, perhaps germinal, matter distributed through the liquid, which was invisible in many parts until stained with carmine.

E''. No. 1. On August 28th, after three days' exposure to air, some specks of white matter were noticed adhering to the sides of the little beaker-glass in which the liquid was placed. On removing and examining them with the microscope, they seemed to consist of spores of fungi germinating.

No. 2. Examined July 29th, 1871. Exposed to the air ten days. A small mass, like a tuft of down, was observed with the unaided eye; it proved to be a collection of mycelium-filaments, and *Mucor* with fructification. Reexamined August 22nd. A mass of green conferva covered the liquid, and both *Torula* and *Mucor* were present.

F''. No. 2. This liquid was examined on September 2nd, 1871, after barely four days' exposure to the air. There were many bodies such as I have described as undeveloped *Torula* (Exp. D'', No. 1). On September 4th there were many sarcinæ to be seen, besides vibriones and other very minute bodies, in the greatest state of activity. Some of them seemed to attach themselves by one end to a point, and swing themselves round and round at a great speed; they did not measure more than 0·00005 inch in length. There were seen some "figure-of-eight" particles in rapid motion; they differed from the minute *Torulæ* by being less oval in form, and capable of moving rapidly.

No. 3. The quantity of liquid was very small. On August 23rd it had become very turbid; instead of examining it with the microscope, an addi-

* Well represented by fig. 12, *a*, p. 197, 'Nature,' vol. ii.

tional quantity was added from tube F″ 3; and on August 25th it had become filled with white matter, consisting of chain-like bodies, most probably motionless *Spirilla.*

It may doubtless appear to some that my particular mode of experimenting involves the introduction of needless complications, that others have obtained the same results without the use of such apparatus; but the incorrectness of this will be admitted when I say that I was not wholly prepared for such a result, and the necessity of being guarded, of being safe, indeed, from every possibility of error, is absolute. Had living things been discovered, that the experimental method and apparatus was a matter of the first importance would have been evident, inasmuch as it excluded all possibility of atmospheric contamination of the experimental liquids during examination. In a word, it was necessary to be prepared for any result, and be guarded on every side. The foregoing description records all the experiments I have made with the view of obtaining the information I sought. It will be noticed that the evidence afforded is perfectly concordant.

The appearances described in Exp. 20*, by Dr. Bastian, are, with the exception of the fungus-spores and bicellular bodies, exactly what one sees in silica. Having ascertained the fact that phosphate of soda, and especially when not neutralized with acid, attacks glass tubes at a temperature of 150° C., and as in this case sodic phosphate and ammonic carbonate were heated for four hours at 146° C. to 153° C., I examined the silica deposited from my own tubes, and observed the gelatinous matter resembling such to be met with in solutions containing infusoria: there were two or three transparent spherules also which I believe to be water enclosed in silica; they are often seen in pectized silica. As for the matter becoming stained by magenta, that is no evidence of its nature, this property being shared by silica†. It may further be remarked that magenta would be precipitated on addition to such a solution by the alkaline phosphate and carbonate: those parts more deeply stained than the others would be those where rosaniline was precipitated; it would be impossible to use a salt of rosaniline for the purpose successfully. The nature of the solution, too, is such that no life will appear in it, even on exposure to the air. An alkaline solution of phosphate of soda and an ammonia salt was kept ten months open to the air, yet no change took place, while several other portions were kept for a shorter period at a temperature of 24° to 34° C. with a like result.

Dr. Bastian considers he has established by experiment the theory that living organisms, amongst which are vibriones and fungi of the genera *Mucor*, *Penicillium*, and *Torula*, and algæ, such as conferva, are evolved *de novo* from lifeless matter; he brings together a number of reasons, of a more or less decided kind, to show not only why it should be an intelligible

* 'Nature,' vol. ii. p. 200.

† Journal of the Chem. Soc. vol. ix. p. 452.

process, but also why others, and particularly M. Pasteur, have obtained results leading to directly an opposite conclusion. These arguments are not drawn from experimental evidence; they do not therefore fall within the bounds of this discussion; but I would point out that, in one case, not only do Dr. Bastian's own experiments deny the truth of a most important assumption of the evolutionists, but also at the same time my own experience proves the contrary. He says*:—"The disruptive agency of heat is fairly enough supposed by the evolutionists to destroy some of the more mobile combinations in each solution—to break up more or less completely, in fact, those very complex organic products whose molecular instability is looked upon as one of the conditions essential to the evolutional changes which are supposed to take place." Before granting such a supposition, it would be necessary to know, first, what are "the very complex organic products" of such peculiar "molecular instability" existing in a solution of tartrate of ammonia, sodic phosphate, acetate of ammonia, oxalate of ammonia, in a solution of sugar and calcined yeast, in turnip infusion, or any other putrescible liquid. My experiments show that there is no such disruptive agency in a high temperature, that it does not influence the "more mobile combinations," either in solutions of organic salts or vegetable infusions; for the "test-tubes" T were precisely the same as the others of the series, contents identical, heated at the same time to the same temperature, in fact taken from among them indiscriminately, the only difference being that one was exposed to the air and the others were not. Besides, there is in addition the evidence afforded by the tubes classed under D″, E″, and F″; yet we find the changes occurred in them as readily as in the unheated original solutions. Dr. Bastian records† the development of organisms in a liquid heated as high as 153° C.; yet the assumed "disruptive agency of heat" is supposed to have influenced the results of Schwann and Pasteur at a temperature of 100° C.! His experience is contradictory to his own theory, and at the same time to the experiments of others, to which his theory raises objection.

It has long been established by Pasteur, Payen, and other experimenters, that a temperature of less than 130° C. is insufficient to destroy all trace of life if the germs or spores are not immersed in a liquid; this is a fact admitted on all sides‡. In that case it is not difficult to understand how, in the experiments of Dr. Child and Prof. Wyman, organisms have been found in liquids to which air only which had passed through red-hot pipes was admitted. The former took a bulb with two narrow necks or tubes, and containing the experimental liquid; one tube was connected by a cork boiled in water with a red-hot porcelain tube filled with pumice, and connected with a gas-holder; the other tube dipped into sulphuric acid: the

* 'Nature,' vol. i. p. 176.

† 'Nature,' vol. ii. p. 200.

‡ 'Nature,' vol. ii. p. 170; Amer. Journ. Science, vol. xxxiv. p. 79; Proc. Roy. Soc. vol. xiii. p. 313.

liquid was boiled for ten or fifteen minutes, and heated air was made to pass through the apparatus till the liquid and flask were cool. "When the bulb is quite cool, the necks are sealed by means of a lamp." In Wyman's experiments an apparatus of much the same kind was used; but the liquids in four cases were boiled for from five to ten minutes in a Papin's digester under a pressure of from two to five atmospheres, or at a temperature of 120° to 150° C. Large flasks of 500 cub. centims., and even 850 cub. centims. capacity were used, containing 17 cub. centims. to 50 cub. centims, or thereabouts, of solution, so that in some cases only $\frac{1}{25}$ of their capacity was occupied; the air admitted was passed through red-hot iron pipes filled with iron wires. There is a similarity between these two sets of experiments; the flasks were not entirely in contact with the hot liquid. We see also, from the few words quoted from the description of Child's experiments, that a sufficient space intervened between the red-hot tube and the bulb to allow of the heated air becoming cool before it entered the glass bulb; it cannot be said, therefore, that the entering air was so hot as to destroy whatever living thing might be attached to the glass. The precautions taken, then, were not sufficient to render the experiments trustworthy, more particularly in the case of Wyman's work, because there he had an immensely large surface untouched by fluid, and naturally he obtained more results in favour of the view of evolution than any other experimenter. Bastian's own experiments are open to the same objection*; in fact it seems that much work has been rendered faulty by this neglect of bringing every part of the interior surface of the containing vessels in contact with the heated liquid. We have positive proof that such is the case when, as Bastian himself states, "it has long been known that a boiled fluid extremely prone to change will not yield infusoria if the vessel in which it is contained is filled with the fluid;" the commercial method of preparing cooked meat depends upon this to a great extent. With respect to the power of vibriones to resist the destructive action of heat, I at one time felt sure, from experiments on hay infusion and decomposing turnip infusion, that they were capable of living after being boiled. This hasty conviction at the moment of observation arose from the fact that the movement of these bodies was the same before and after boiling, and at the same time unlike any example of the Brownian movement with which I was familiar; but there is no evidence that they were really living in the first case; it is only a presumption.

A strong infusion of hay was made with lukewarm water, filtered twice through Swedish paper, and examined under the microscope; it swarmed with life, especially in the form of bacteria, mostly in rapid motion; some of these were excessively small, less than 0·00005 inch in length. The liquid was boiled violently for fifteen minutes until two thirds had been evaporated away; a drop placed under the microscope showed that most of

* It is worthy of remark that in two out of the four cases in which Wyman heated liquids at a temperature over 100° C., no organisms were found.

the living bacteria had been killed, or were, at any rate, motionless. Careful observation, however, showed the minute vibriones to be as lively as ever; they curled and twisted, and dived out of focus just as when alive. The same fact was noticed in a turnip infusion. Pasteur proved that the germs of vibriones in milk were not killed till the liquid was heated to 110° C.* I fancy that the recent experiments of Dr. Crace-Calvert† lead him to fix the fatal temperature at too high a rate (between 150° and 200° C.), supposing the vibriones to be immersed in the liquid; but if any of them be left adhering to the glass tube, possibly not. The fact that hay infusion contains, besides such multitudes of living things, great quantities of organic débris, led to the conclusion that no safe experiments could be made with such a liquid. If a liquid be boiled in a flask and sealed up, it depends very much upon the nature of the liquid whether life will be developed in it: thus, if we take a most carefully prepared saline or sugar solution, the water nsed being particularly pure, and the salts dissolved and recrystallized with all possible care, the chances are that nothing will come of it; but if, on the other hand, a liquid swarming with life be chosen, the conditions will be most favourable for depositing living things on the sides of the vessel, where they will be out of reach of the boiling liquid, and, getting washed off into the liquid when cool, will there multiply. On this account, the use of hay infusion in particular has led to erroneous deductions, from faulty experiments. To any one who will make a strong infusion of hay with lukewarm water, and examine it with the microscope, it will, I am sure, be simply inconceivable how any appearance of accurate evidence can be derived from such a fluid, particularly when the liquid is not heated above the boiling-point.

It must be allowed that Pasteur's experiments prove that when fermentable liquids were protected from matter floating in the air, fermentation would not take place. Now the first evidence we have of fermentation in, for instance, a saccharine solution, is the presence of *Torula*-cells; moreover, to set up fermentation, the *Torula* is placed in saccharine fluid, the operation of every brewery. It is evident, then, that either the *Torula* is the agent of fermentative change, or is closely connected with it. But whence arises the *Torula*? From matter floating in the atmosphere? in that case, seeing they are invariable and definite organisms, which are produced from variable liquids, it must be from invariable and definite matter that these organisms arise. Whence come, what is the origin, the nature, the chemical composition, or, more particularly, the chemical constitution of the so-called dead organic particles of this invariable nature, producing such invariable yet tremendous changes in such very simple substances, and to what class of chemical compounds do they belong? Until this question has been satisfactorily answered, ordinary reason would assign to them the possession of life, for their properties are the properties possessed by none but living things. If these organic nitrogenous particles are not living,

* Ann. Chim. Phys. tom. lxiv. p. 61. † Proc. Royal Soc. , vol. xix. p. 472.

how are we to account for the action of various antiseptics? In the Report to the Cattle-Plague Commissioners "On Disinfection and Disinfectants," by Dr. Angus Smith, an account is given (p. 10) of the action of a number of essential oils, such as oil of bitter almonds, oil of mustard, amylic alcohol, cresylic and carbolic acids, and ether, the vapour of which was diffused in air surrounding pieces of meat. Many substances had the property of preserving the meat for a great length of time, especially amylic alcohol and oil-of-bitter-almonds chloroform; and carbonic tetrachloride also shares this property. If these nitrogenous particles are not living things, how are we to account for the action of the substances? It cannot be a chemical action, because these substances are chemically inactive. In some cases the less active colytic agents having diffused away out of the bottle, mould formed on the meat, or putrefaction commenced; and this always happened on that part nearest the cork, showing that particles causing the change came in with air after the preservative agent had escaped. That these colytic agents were fatal to the growth of mould, or the spread of putrefaction, was ascertained by placing mouldy paste under a bell, the atmosphere of which contained a small quantity of the vapour.

It has been doubted lately whether carbolic acid has the power of destroying germs, whether, in fact, Prof. Lister's surgical treatment depends upon this power. The experiments which have given rise to the question tell us only that when carbolic acid is added to a putrescible liquid it is not preserved. There is nothing new in this. "The phenylic and cresylic alcohols, which experience has shown to be generally so active in the prevention of putrefaction, are not sufficiently constant when a large quantity of water is present"*. When in a state of vapour very greatly diluted with air, we find them preventing organic growths and putrefaction in an extraordinary manner. In order to preserve liquids, corrosive sublimate, sulphate of copper, and chloride of zinc are the most efficacious substances.

If those particles which are the origin of life are themselves lifeless, they could not influence the nature of the organisms developed; in that case different solutions would give rise to different organisms, and, conversely, the same solution would yield the same form of life. But we do not find this to be the case; for in the same infusion of turnip in Experiment D″ 1 and D″ 3 occur on exposure to the air at different times different forms of life. In the one case minute vibriones and masses of *Torula*-cells occurred, whereas in the other confervoid growth and some minute motionless organisms which had not previously been observed were the most noticeable forms. With different portions of a solution of alkaline phosphates and tartrates, in one case we got a fungoid and in the other an algoid form of life. Were it not for the great preponderance of sound experimental evidence to the contrary, it would be easier to believe the theory that life was evolved *de novo*. It is the crude idea which a superficial observer of everyday phenomena would entertain; the popular error that maggots are bred

* "On Disinfection and Disinfectants," Cattle-Plague Report, pp. 14 & 15.

out of corruption illustrates this. The theory involves the discovery of a new property of matter, the property that certain compounds (undefined nitrogenous particles in the atmosphere) must have of decomposing molecules of other substances with which they are in contact, and building out of their constituent atoms substances of a much more complicated nature, without the exertion of external forces; even beyond this, they must be capable of arranging those compounds into definite forms. In the course of nature complex substances do not increase but simplify their molecular complexity. By oxidation vegetable products are resolved into carbonic acid and water: we find no tendency in these oxides of carbon and hydrogen to become reduced to such products as sugar, starch, or cellulose; nay, much further, we know no process by which such transformation might be effected, still less are we acquainted with any internal forces which can mould them into cells with functions to perform. On this account a very great deal of thoroughly sound experimental evidence is necessary to establish the doctrine of evolution of life *de novo*. But, so far as our present knowledge guides us, whether we term it spontaneous generation, abiogenesis, or archebiosis, the process by which living things spring from lifeless matter must be said to be only ideal.

Note.—In describing certain organisms as *mucors*, those thecasporous fungi are meant of which *Ascophora mucedo* is the type; they are described by Pasteur as *mucédinées*; and he seems to include in this description *Penicillium*, which is basidiosporous *. *Mucors* he classes as those vegetable organisms which take the form generally on the surface of a liquid of a thin pellicle with a more or less fatty or gelatinous appearance. Such matter I have noticed accompanying conferva. The fungus in chains found in urine is identical with that supposed by Pasteur † to cause the production of carbonate of ammonia from urea; while those small cellules without a nucleus, which I surmise, for a reason already stated, to be *Torula* in an early stage, he believes to be specifically different ‡. With regard to filters of nitrocellulose, I some time since wished to modify ordinary filters so as to make them more easily combustible in the crucible when used for quantitative analysis. This I tried by dipping them in strong nitric acid, washing in water, and finally in ammonia. As far as my recollection goes, I obtained a denser kind of paper, which answered the purpose very well. On endeavouring recently to make this variety of filter-paper, I met with the same difficulty which Prof. Bloxam tells me some years ago he encountered when attempting the same object, namely, the parchmentizing of the paper, so as to render it waterproof. Either I have not used the proper strength of acid necessary, or else my former experience rested on the formation of nitrate of ammonia within the fibres of the paper.

* Ann. de Chimie et de Phys. tom. lxiv. p. 47. † *Ibid.* p. 52. ‡ *Ibid.* p. 52.

III. "Experiments on the directive power of large Steel Magnets, of Bars of magnetized Soft Iron, and of Galvanic Coils, in their action on external small Magnets." By GEORGE BIDDELL AIRY, Astronomer Royal, C.B., P.R.S.—With Appendix containing an Investigation of the Attraction of a Galvanic Coil on a small magnetic mass. By JAMES STUART, Esq., M.A., Fellow of Trinity College, Cambridge. Received January 6, 1872.

(Abstract.)

The author, after adverting to some imperfect experiments made by Coulomb in the last century, describes the apparatus which he had himself used. He employed a bar-magnet 14 inches in length, placed in one series with its edge towards the small compass on which its directive power was estimated, and in another series with its flat side towards the small compass; also a galvanic coil 13·4 inches in length, animated by a battery of three cells, and the same coil with the insertion of a soft iron coil. In the field of experiment the earth's magnetism was sensibly neutralized by external large magnets. The direction of the needle of the small compass was estimated by eye. The magnitude of the directive force was found by observing the position taken by the needle when the poles of a horseshoe-magnet were placed in a definite position above it: for the measure of the force of the galvanic coil without core, a very small magnet was used in the same manner; its power was found to be about $\frac{1}{120}$ that of the horseshoe-magnet. The circle on which the deflections were observed was graduated to cotangents, which gave immediately the measure of the force of the large magnet or coil, &c. In each case, observations were taken in 30 stations in one oval ring surrounding the magnet &c., and in 38 stations in another oval ring surrounding it at a greater distance. Omitting notice of the measures in general, the following specific points are remarked:—

At a constant distance from the steel, the greatest force exerted by a magnet is not the longitudinal force at the end, but the transversal force near the end. In going round the magnet there are six maxima and six minima of force.

The law of attraction of the core of a galvanic coil is not very different from that of a magnet.

The force produced by the core within the coil is very much greater than that produced by the coil alone. In some positions of the small compass it is about forty times as great, and in some about 170 times as great.

The law of force at different parts of the coil differs greatly from that at corresponding parts of the magnet or core. In the coil it is, proportionally, far greater at the end, and its direction is different. Near the end of the magnet or core the directions of force converge to a point within it,

distant from the end by about $\frac{1}{12}$ part of its length. Near the end of the coil, the directions of force converge to a point as exactly as possible at the centre of the end of the coil.

The author then describes the graphic process by which he has resolved the entire magnetic forces into constituent parts in the directions longitudinal and transversal to the magnet at every station, and gives tabular statements of the magnitudes of those constituent parts. A comparison is made with the results of an assumed law, but no satisfactory agreement is obtained.

An Appendix is subjoined, containing an investigation by James Stuart, Esq., of the theoretical attraction of a galvanic coil upon a small mass of magnetism, and a tabular comparison of the numerical values obtained from this investigation with the numerical values found by experiment. The agreement is satisfactory.

IV. "On a mode of Measuring the Internal Resistance of a Multiple Battery by adjusting the Galvanometer to Zero." By M. Jules Raynaud. Communicated by Prof. Stokes, Sec. R.S. Received January 11, 1872.

The author points out that the method given by Mr. Henry Mance for this purpose, and described in vol. xix. of the 'Proceedings of the Royal Society' (p. 252), is identical with that which he had himself previously given, and which is described in the 'Comptes Rendus' for July 22, 1867; at least the only difference is that M. Raynaud prescribes putting the poles in connexion with the earth, which of course is not necessary.

February 15, 1872.

GEORGE BIDDELL AIRY, C.B., President, succeeded by Mr. C. B. Vignoles (as Deputy appointed by the President), in the Chair.

The President, on the part of the Council, submitted to the Meeting the following Address of congratulation to be presented to the Queen:—

"We, Your Majesty's most dutiful and loyal subjects, the President, Council, and Fellows of the Royal Society of London for improving Natural Knowledge, desire humbly to offer to Your Majesty our sincere congratulations on the restoration to health of His Royal Highness the Prince of Wales.

"Having shared in the deep anxiety of Your Majesty and the Nation whilst His Royal Highness lay prostrate under his dangerous

illness, we, with heartfelt thankfulness, rejoice with Your Majesty and all Your Majesty's subjects throughout the Empire in His Royal Highness's happy recovery. That he may long be spared to Your Majesty, to his Family, and to the People of this Country, is the earnest wish and prayer of Your Majesty's loyal and devoted subjects, the President, Council, and Fellows of the Royal Society of London."

On the motion of Mr. C. B. Vignoles, seconded by Dr. Webster, it was resolved that the Fellows do most cordially concur in the Address now read from the Chair.

The Address was then signed by the President on behalf of the Council and Fellows.

The following communications were read:—

I. "On the Induction of Electric Currents in an Infinite Plane Sheet of uniform conductivity." By Prof. J. Clerk Maxwell, F.R.S. Received January 10, 1872.

1. When, on account of the motion or the change of strength of any magnet or electromagnet, a change takes place in the magnetic field, electromotive forces are called into play, and, if the material in which they act is a conductor, electric currents are produced. This is the phenomenon of the induction of electric currents, discovered by Faraday.

I propose to investigate the case in which the conducting substance is in the form of a thin stratum or sheet, bounded by parallel planes, and of indefinite extent. A system of magnets or electromagnets is supposed to exist on the positive side of this sheet, and to vary in any way by changing its position or its intensity. We have to determine the nature of the currents induced in the sheet, and their magnetic effect at any point, and, in particular, their reaction on the electromagnetic system which gave rise to them. The induced currents are due, partly to the direct action of the external system, and partly to their mutual inductive action; so that the problem appears, at first sight, somewhat difficult.

2. The result of the investigation, however, may be presented in a remarkably simple form, by the aid of the principle of images which was first applied to problems in electricity and hydrokinetics by Sir W. Thomson. The essential part of this principle is, that we conceive the state of things on the positive side of a certain closed or infinite surface (which is really caused by actions having their seat on that surface) to be due to an imaginary system on the negative side of the surface, which, if it existed, and if the action of the surface were abolished, would give rise to the actual state of things in the space on the positive side of the surface.

The state of things on the positive side of the surface is expressed by a mathematical function, which is different in form from that which expresses the state of things on the negative side, but which is identical with

that which would be due to the existence, on the negative side, of a certain system which is called the Image.

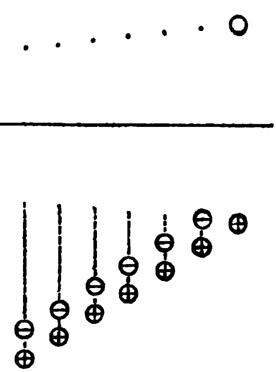
The image, therefore, is what we should arrive at by *producing*, as it were, the mathematical function as far as it will go; just as, in optics, the virtual image is found by producing the rays, in straight lines, backwards from the place where their direction has been altered by reflexion or refraction.

3. The position of the image of a point in a plane surface is found by drawing a perpendicular from the point to the surface and producing it to an equal distance on the other side of the surface. If the image is of the same sign as the point, as it is in hydrokinetics when the surface is a rigid plane, it is called a positive image. If it is of the opposite sign, as in statical electricity, when the surface is a conductor, it is called a negative image. The image of a conducting circuit is reckoned positive when the electric current flows in the corresponding directions through corresponding parts of the object and the image. The image is reckoned negative when the direction of the current is reversed.

In the case of the plane conducting sheet, the imaginary system on the negative side of the sheet is not the simple image, positive or negative, of the real magnet or electromagnet on the positive side, but consists of a moving train of images, the nature of which we now proceed to define.

4. Let the electric resistance of a rectangular portion of the sheet whose length is a, and whose breadth is $2\pi a$, be R.

R is to be measured on the electromagnetic system, and is therefore a velocity, the value of which is independent of the magnitude of the line a. (If ρ denotes the specific resistance of the material of the sheet for a unit cube, and if c is the thickness of the sheet, then $R=\frac{\rho}{2\pi c}$; and if σ denotes the specific resistance of the sheet for a unit (or any other) square, $R=\frac{\sigma}{2\pi}$.)



5. Let us begin by dividing the time into a number of equal intervals, each equal to δt. The smaller we take these intervals the more accurate will be the definition of the train of images which we shall now describe.

6. At a given time t, let a positive image of the magnet or electromagnet be formed on the negative side of the sheet.

As soon as it is formed, let this image begin to move away from the sheet in the direction of the normal, with the velocity R, its form and intensity remaining constantly the same as that which the magnet had at the time t.

After an interval δt, that is to say, at the time $t+\delta t$, let a negative image, equal in magnitude and opposite in sign to this positive image, be formed in the original position of the positive image, and let it then begin to move along the normal, after the positive image, with the velocity R. The interval of time between the arrival of these images at any point will be δt, and the distance between corresponding points will be $R\delta t$.

7. Leaving this pair of images to pursue their endless journey, let us attend to the real magnet, or electromagnet, as it is at the time $t+\delta t$. At this instant let a new positive image be formed of the magnet in its new position, and let this image also travel in the direction of the normal with the velocity R, and be followed after an interval of time δt by a corresponding negative image. Let these operations be repeated at equal intervals of time, each of these intervals being equal to δt.

8. Thus at any given instant there will be a train or trail of images, beginning with a single positive image, and followed by an endless succession of pairs of images. This trail, when once formed, continues unchangeable in form and intensity, and moves as a whole away from the conducting sheet with the constant velocity R.

9. If we now suppose the interval of time δt to be diminished without limit, and the train to be extended without limit in the negative direction, so as to include all the images which have been formed in all past time, the magnetic effect of this imaginary train at any point on the positive side of the conducting sheet will be identical with that of the electric currents which actually exist in the sheet.

Before proceeding to prove this statement, let us take notice of the form which it assumes in certain cases.

10. Let us suppose the real system to be an electromagnet, and that its intensity, originally zero, suddenly becomes I, and then remains constant. At this instant a positive image is formed, which begins to travel along the normal with velocity R. After an interval δt another positive image is formed; but at the same instant a second negative image is formed at the same place, which exactly neutralizes its effect. Hence the result is, that a single positive image travels by itself along the normal with velocity R. The magnetic effect of this image on the positive side of the sheet is equivalent to that of the currents of induction actually existing in the sheet, and the diminution of this effect, as the image moves away from the sheet, accurately represents the effect of the currents of induction, which gradually decay on account of the resistance of the sheet. After a sufficient time, the image is so distant that its effects are no longer sensible on the positive side of the sheet. If the current of the electromagnet be now broken, there will be no more images; but the last negative image of the train will be left unneutralized, and will move away from the sheet with velocity R. The currents in the sheet will therefore be of the same magnitude as those which followed the excitement of the electromagnet, but in the opposite direction.

11. It appears from this that, when the electromagnet is increasing in intensity, it will be acted on by a repulsive force from the sheet, and when its intensity is diminishing, it will be attracted towards the sheet.

It also appears that if any system of currents is produced in the sheet and then left to itself, the effect of the decay of the currents, as observed at a point on the positive side of the sheet, will be the same as if the sheet, with its currents remaining constant, had been carried away in the negative direction with velocity R.

12. If a magnetic pole of strength m is brought from an infinite distance along a normal to the sheet with a uniform velocity v towards the sheet, it will be repelled with a force

$$\frac{m^2}{4z^2}\frac{v}{R+v},$$

where z is the distance from the sheet at the given instant.

This formula will not apply to the case of the pole moving away from the sheet, because in that case we must take account of the currents which are excited when the pole begins to move, which it does when near the sheet.

13. If the magnetic pole moves in a straight line parallel to the sheet, with uniform velocity v, it will be acted on by a force in the opposite direction to its motion, and equal to

$$\frac{m^2}{4z^2}\,v\,\frac{\sqrt{R^2+v^2}+R-v}{(\sqrt{R^2+v^2}+R)^2}.$$

Besides this retarding force, it is acted on by a force repelling it from the sheet, equal to

$$\frac{m^2}{4z^2}\frac{v^2}{R^2+v^2+R\sqrt{R^2+v^2}}.$$

14. If the pole moves uniformly in a circle, the trail is in the form of a helix, and the calculation of its effect is more difficult; it is easy, however, to see that, besides the retarding force and the repelling force, there is also a force towards the centre of the circle.

15. It is shown, in my treatise on Electricity and Magnetism (vol. ii. art. 600), that the currents in any system are the same, whether the conducting system or the inducing system be in motion, provided the relative motion is the same. Hence the results already given are directly applicable to the case of Arago's rotating disk, provided the induced currents are not sensibly affected by the limitation arising from the edge of the disk. These will introduce other sets of images, which we shall not now investigate.

16. The greater the resistance of the sheet, whether from its thinness or from the low conducting-power of its material, the greater is the velocity R. Hence in most actual cases R is very great compared with v, the velocity of the external system, and the trail of images is nearly normal to

the sheet, and the induced currents differ little from those which arise from the direct action of the external system (see § 1).

17. If the conductivity of the sheet were infinite, or its resistance zero, R would be zero. The images, once formed, would remain stationary, and all except the last formed positive image would be neutralized. Hence the trail would be reduced to a single positive image, and the sheet would exert a repulsive force $\frac{m^2}{4z^2}$ on the pole, whether the pole be in motion or at rest.

I need not say that this case does not occur in nature as we know it. Something of the kind is supposed to exist in the interior of molecules in Weber's Theory of Diamagnetism.

Mathematical Investigation.

18. Let the conducting sheet coincide with the plane of xy, and let its thickness be so small that we may neglect the variation of magnetic force at different points of the same normal within its substance, and that, for the same reason, the only currents which can produce sensible effects are those which are parallel to the surface of the sheet.

Current-function.

19. We shall define the currents in the sheet by means of the current-function ϕ. This function expresses the quantity of electricity which, in unit of time, crosses from right to left a curve drawn from a point at infinity to the point P.

This quantity will be the same for any two curves drawn from this point to P, provided no electricity enters or leaves the sheet at any point between these curves. Hence ϕ is a single-valued function of the position of the point P.

The quantity which crosses the element ds of any curve from right to left is

$$\frac{d\phi}{ds}ds.$$

By drawing ds first perpendicular to the axis of x, and then perpendicular to the axis of y, we obtain for the components of the electric current in the directions of x and of y respectively

$$u=\frac{d\phi}{dy}, \qquad v=-\frac{d\phi}{dx}. \quad \ldots\ldots\ldots \quad (1)$$

The curves for which ϕ is constant are called current lines.

20. The annular portion of the sheet included between the current lines ϕ and $\phi+\delta\phi$ is a conducting circuit round which an electric current of strength $\delta\phi$ is flowing in the positive direction, that is, from x towards y. Such a circuit is equivalent in its magnetic effects to a magnetic shell of strength $\delta\phi$, having the circuit for its edge*.

* W. Thomson, "Mathematical Theory of Magnetism," Phil. Trans. 1850.

The whole system of electric currents in the sheet will therefore be equivalent to a complex magnetic shell, consisting of all the simple shells, defined as above, into which it can be divided. The strength of the equivalent complex shell at any point will be ϕ.

We may suppose this shell to consist of two parallel plane sheets of imaginary magnetic matter at a very small distance c, the surface-density being $\frac{\phi}{c}$ on the positive sheet, and $-\frac{\phi}{c}$ on the negative sheet.

21. To find the magnetic potential due to this complex plane shell at any point not in its substance, let us begin by finding P, the potential at the point (ξ, η, ζ) due to a plane sheet of imaginary magnetic matter whose surface-density is ϕ, and which coincides with the plane of xy. The potential due to the positive sheet whose surface-density is $\frac{\phi}{c}$, and which is at a distance $\frac{1}{2}c$ on the positive side of the plane of xy, is

$$\frac{1}{c}\left(\mathrm{P}-\tfrac{1}{2}c\frac{d\mathrm{P}}{d\zeta}+\&c.\right).$$

That due to the negative sheet, at a distance $\frac{1}{2}c$ on the negative side of the plane of xy, is

$$-\frac{1}{c}\left(\mathrm{P}+\tfrac{1}{2}c\frac{d\mathrm{P}}{d\zeta}+\&c.\right).$$

Hence the magnetic potential of the shell is

$$\mathrm{V}=-\frac{d\mathrm{P}}{d\zeta}. \qquad (2)$$

This, therefore, is the value of the magnetic potential of the current-sheet at any given point on the positive side of it. Within the sheet there is no magnetic potential, and at any point $(\xi, \eta, -\zeta)$ on the negative side of the sheet the potential is equal and of opposite sign to that at the point (ξ, η, ζ) on the positive side.

22. At the positive surface the magnetic potential is

$$\mathrm{V}=-\frac{d\mathrm{P}}{d\zeta}=2\pi\phi. \qquad (3)$$

At the negative surface

$$\frac{d\mathrm{P}}{d\xi}=2\pi\phi. \qquad (4)$$

The normal component of magnetic force at the positive surface is

$$\gamma=-\frac{d\mathrm{V}}{d\zeta}=\frac{d^2\mathrm{P}}{d\zeta^2}. \qquad (5)$$

In the case of the magnetic shell, the magnetic force is discontinuous at the surface; but in the case of the current-sheet this expression gives the value of γ within the sheet itself, as well as in the space outside.

23. Let F, G, H be the components of the electromagnetic momentum at any point in the sheet, due to external electromagnetic action as well as to

that of the currents in the sheet, then the electromotive force in the directions of x is

$$-\frac{dF}{dt}-\frac{d\psi}{dx},$$

where ψ is the electric potential*; and by Ohm's law this is equal to σu, where σ is the specific resistance of the sheet.

Hence

Similarly,

$$\left.\begin{aligned}\sigma u&=-\frac{dF}{dt}-\frac{d\psi}{dx}.\\ \sigma v&=-\frac{dG}{dt}-\frac{d\psi}{dy}\end{aligned}\right\}\quad . \quad . \quad . \quad . \quad (6)$$

Let the external system be such that its magnetic potential is represented by $-\frac{dP_0}{dz}$, then the actual magnetic potential will be

$$V=-\frac{d}{dz}(P_0+P), \quad . \quad . \quad . \quad . \quad (7)$$

and

$$F=\frac{d}{dy}(P_0+P), \quad G=-\frac{d}{dx}(P_0+P), \quad H=0. \quad . \quad . \quad . \quad (8)$$

Hence equations (6) become, by introducing the stream-function ϕ from (1),

$$\left.\begin{aligned}\sigma\frac{d\phi}{dy}&=-\frac{d^2}{dtdy}(P_0+P)-\frac{d\psi}{dx},\\ -\sigma\frac{d\phi}{dx}&=\frac{d^2}{dtdx}(P_0+P)-\frac{d\psi}{dy}.\end{aligned}\right\}\quad . \quad . \quad . \quad . \quad (9)$$

A solution of these equations is

$$\sigma\phi=-\frac{d}{dt}(P_0+P), \quad \psi=\text{constant}. \quad . \quad . \quad . \quad . \quad (10)$$

Substituting the value of ϕ in terms of P, as given in equation (4),

$$\frac{\sigma}{2\pi}\frac{dP}{dz}=\frac{d}{dt}(P_0+P). \quad . \quad . \quad . \quad . \quad (11)$$

The quantity $\frac{\sigma}{2\pi}$ is evidently a velocity; let us therefore for conciseness call it R, then

$$\frac{dP}{dz}+\frac{dP}{dt}+\frac{dP_0}{dt}=0. \quad . \quad . \quad . \quad . \quad (12)$$

24. Let P_0' be the value of P_0 at the time $t-\tau$, and at a point on the negative side of the sheet, whose coordinates are x, y, $(z-R\tau)$, and let

$$Q=\int_0^\infty P_0'd\tau. \quad . \quad . \quad . \quad . \quad (13)$$

At the upper limit when τ is infinite P_0' vanishes. Hence at the lower limit, when $\tau=0$ and $P_0'=P_0$, we must have

$$P_0=\frac{dQ}{dt}+R\frac{dQ}{dz}; \quad . \quad . \quad . \quad . \quad (14)$$

* "Dynamical Theory of the Electromagnetic Field," Phil. Trans. 1865, p. 483.

but by equation (12)

$$\frac{dP_0}{dt}=-\frac{dP}{dt}-R\frac{dP}{dz}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

Hence the equation will be satisfied if we make

$$P=-\frac{dQ}{dt}=-\frac{d}{dt}\int_0^\infty P_0'dr. \quad . \quad . \quad . \quad . \quad (16)$$

25. This, then, is a solution of the problem. Any other solution must differ from this by a system of closed currents, depending on the initial state of the sheet, not due to any external cause, and which therefore must decay rapidly. Hence, since we assume an eternity of past time, this is the only solution of the problem.

This solution expresses P, a function due to the action of the induced current, in terms of P_0', and through this of P_0, a function of the same kind due to the external magnetic system. By differentiating P and P_0 with respect to z, we obtain the magnetic potential, and by differentiating them with respect to t, we obtain, by equation (10), the current-function. Hence the relation between P and P_0, as expressed by equation (16), is similar to the relation between the external system and its trail of images as expressed in the description of these images in the first part of this paper (§§ 6, 7, 8, 9), which is simply an explanation of the meaning of equation (16) combined with the definition of P_0' in § 24.

Note to the preceding Paper.

At the time when this paper was written, I was not able to refer to two papers by Prof. Felici, in Tortolini's 'Annali di Scienze' for 1853 and 1854, in which he discusses the induction of currents in solid homogeneous conductors and in a plane conducting sheet, and to two papers by E. Jochmann in Crelle's Journal for 1864, and one in Poggendorff's 'Annalen' for 1864, on the currents induced in a rotating conductor by a magnet.

Neither of these writers have attempted to take into account the inductive action of the currents on each other, though both have recognized the existence of such an action, and given equations expressing it. M. Felici considers the case of a magnetic pole placed almost in contact with a rotating disk. E. Jochmann solves the case in which the pole is at a finite distance from the plane of the disk. He has also drawn the forms of the current-lines and of the equipotential lines, in the case of a single pole, and in the case of two poles of opposite name at equal distances from the axis of the disk, but on opposite sides of it, and has pointed out why the current-lines are not, as Matteucci at first supposed, perpendicular to the equipotential lines, which he traced experimentally.

I am not aware that the principle of images, as described in the paper presented to the Royal Society, has been previously applied to the phenomena of induced currents, or that the problem of the induction of

currents in an infinite plane sheet has been solved, taking into account the mutual induction of these currents, so as to make the solution applicable to a sheet of any degree of conductivity.

The statement in equation (10), that the motion of a magnetic system does not produce differences of potential in the infinite sheet, may appear somewhat strange, since we know that currents may be collected by electrodes touching the sheet at different points. These currents, however, depend entirely on the inductive action on the part of the circuit not included in the sheet; for if the whole circuit lies in the plane of the sheet, but is so arranged as not to interfere with the uniform conductivity of the sheet, there will be no difference of potential in any part of the circuit. This is pointed out by Felici, who shows that when the currents are induced by the instantaneous magnetization of a magnet, these currents are not accompanied with differences of potential in different parts of the sheet.

When the sheet is itself in motion, it appears, from art. 600 of my treatise 'On Electricity and Magnetism,' that the electric potential of any point, as measured by means of the electrodes of a fixed circuit, is

$$\psi=-\left(F\frac{\partial x}{\partial t}+G\frac{\partial y}{\partial t}+H\frac{\partial z}{\partial t}\right);$$

where $\frac{\partial x}{\partial t}$, $\frac{\partial y}{\partial t}$, $\frac{\partial z}{\partial t}$ are the components of the velocity of the part of the sheet to which the electrode is applied.

In the case of a sheet revolving with velocity ω about the axis of z, this becomes

$$\psi=\omega\left(x\frac{dP}{dx}+y\frac{dP}{dy}\right).$$

Note 2.—The velocity R for a copper plate of best quality 1 millimetre in thickness is about 25 metres per second. Hence it is only for *very* small velocities of the apparatus that we can obtain any approximation to the true result by neglecting the mutual induction of the currents.—Feb. 13.

II. "On some Derivatives of Uramidobenzoic Acid." By P. Griess, F.R.S. Received January 15, 1872.

This acid, of which I gave a short description some time ago*, has the composition $C_8H_8N_2O_3$. I obtained it in the first instance from the basic compound $C_{10}H_{12}N_2O_3$, which is one of the products of the action of cyanogen on an alcoholic solution of amidobenzoic acid. Its

* Zeitsch. f. Chem. 1868, p. 389.

formation takes place in the manner indicated in the following equation:—

$$\underbrace{C_{10}H_{12}N_2O_3}_{\text{Basic compound.}} + \underbrace{H_2O}_{\text{Water.}} = \underbrace{C_8H_8N_2O_3}_{\text{Uramidobenzoic acid.}} + \underbrace{C_2H_6O.}_{\text{Alcohol.}}$$

Since then I have shown * that this acid is also formed when urea and amidobenzoic acid are cautiously melted together:—

$$\underbrace{C_7H_7NO_2}_{\text{Amidobenzoic acid.}} + \underbrace{COH_4N_2}_{\text{Urea.}} = \underbrace{C_8H_8N_2O_3}_{\text{Uramidobenzoic acid.}} + \underbrace{NH_3}_{\text{Ammonia.}}$$

A third and more advantageous process of preparing uramidobenzoic acid, that of Menschutkin †, depends on the mutual decomposition which takes place when aqueous solutions of potassium cyanate and amidobenzoic-acid hydrochloride are mixed:—

$$\underbrace{C_7H_7NO_2, HCl}_{\text{Amidobenzoic acid hydrochloride.}} + \underbrace{CNOK}_{\text{Potassium cyanate.}} = \underbrace{C_8H_8N_2O_3}_{\text{Uramidobenzoic acid.}} + \underbrace{KaCl.}_{\text{Potassium chloride.}}$$

Menschutkin has described this acid under the name of oxybenzuramic acid; but I have satisfied myself that it is identical with the uramidobenzoic acid obtained by the two first-mentioned processes.

I will not here recapitulate the properties of this acid, which are described in my former papers on the subject, and in that of Menschutkin, who has also considered the constitution of the compound. The uramidobenzoic acid is particularly remarkable for the great number of derivatives it is capable of yielding, being surpassed by but few organic compounds in this respect. It is my intention in this communication to describe several of these derivatives.

Action of strong Nitric Acid on Uramidobenzoic Acid.

When uramidobenzoic acid, deprived of its water of crystallization, is gradually introduced into well-cooled fuming nitric acid that has been freed from nitrous acid, it is dissolved in large quantities, and without any evolution of gas. The solution, when nearly saturated, is allowed to stand for about one hour, and then poured into a large quantity of water, taking care to avoid rise of temperature. By this means an abundant yellowish-white crystalline precipitate is obtained, having strongly marked acid properties. It is soluble in alcohol, and easily so in ether, even in the cold, crystallizing therefrom in yellowish-white needles which have the composition $C_8H_6N_4O_7$. In spite of their appearance, however, they are by no means homogeneous, consisting, as I have ascertained, of three different acids, all

* Deut. Chem. Ges. Ber. 1869, p. 47.

† Ann. Chem. und Pharm. vol. cliii. p. 83.

of which have the formula $C_8H_6N_4O_7$, so that they may be regarded as isomeric dinitrouramidobenzoic acids, $C_8H_6N_4O_7 = C_8H_6(NO_2)_2O_3$. Although I have not yet been able to completely separate these acids from one another, owing to the great similarity of their properties, the following facts leave no doubt as to the correctness of this view regarding their nature.

Decomposition of the Isomeric Dinitrouramidobenzoic Acids by boiling with Aqueous Ammonia.

When the solution of the three isomeric dinitro-acids in aqueous ammonia is boiled for some time, decomposition ensues, resulting in the formation of three new isomeric acids of the formula $C_8H_7N_3O_5$, as represented in the following equation:—

$$\underbrace{C_8H_6N_4O_7}_{\text{Isomeric dinitro-acids.}} + H_2O = \underbrace{C_8H_7N_3O_5}_{\text{New isomeric acids.}} + \underbrace{NHO_3}_{\text{Nitric acid.}}.$$

From a consideration of the composition of these new acids, it will be evident that they may be regarded as mononitrouramidobenzoic acids, which view is, moreover, confirmed by their chemical reactions. By taking advantage of the difference in the solubility of their respective barium salts, they may be separated from one another. The following are the details of their preparation:—The dilute ammoniacal solution of the mixed dinitrouramidobenzoic acids is kept boiling for about an hour, when the decomposition may be regarded as complete. A sufficient quantity of barium chloride is then added to the hot solution, and on cooling a considerable amount of needle-shaped crystals separate, which consist of the barium salt of one of the new acids, the β nitrouramidobenzoic acid. When the mother-liquor separated from these crystals is sufficiently concentrated by evaporation, another barium salt begins to separate, the quantity of which may be considerably increased by allowing the solution to stand for some hours. The yellowish-white salt thus obtained appears amorphous to the unassisted eye, but on careful inspection will be seen to consist of minute needles. The acid corresponding to this salt I propose to call α nitrouramidobenzoic acid.

In order to obtain the barium salt of the third nitro-acid, the γ nitrouramidobenzoic acid, the mother-liquor from the previous salt is evaporated nearly to dryness on the water-bath, and the resulting mass washed with cold water *. The residue, when crystallized from hot water, taking care to avoid too long boiling, yields the pure barium salt of γ nitrouramidobenzoic acid in bright yellow scales.

As regards the separation of the three nitro-acids in question from their

* The washing contains, besides ammonium and barium chlorides, another barium salt, crystallizing in yellowish-red needles, which I shall revert to in another part of the paper.

respective barium salts, this is easily effected by adding a slight excess of hydrochloric acid to the hot aqueous solutions of the latter, the acids crystallizing out on cooling.

α nitrouramidobenzoic acid, $C_8 H_7 (NO_2) N_2 O_3$.—This acid crystallizes in bright yellow needles or small plates, which are difficultly soluble in hot water, and but very slightly so in cold water and ether. Boiling alcohol dissolves it readily. Its salts have the general formula $C_8 H_6(NO_2)N_2O_3, M'$, and are as a rule soluble with difficulty.

β nitrouramidobenzoic acid, $C_8 H_7 (NO_2) N_2 O_3$.—This acid crystallizes from its hot aqueous solution in very slender bright yellow needles, closely resembling in appearance its barium salt previously described. It is nearly insoluble in cold water, and only very slightly in hot; by boiling alcohol, however, it is taken up in considerable quantities. It is mono-basic like the α acid.

γ nitrouramidobenzoic acid, $C_8 H_7 (NO_2) N_2 O_3$, is obtained in small yellow scales, which are but very slightly soluble in all neutral solvents. It is mono-basic, and almost all its salts are more readily soluble than the corresponding salts of the other isomeric acids; moreover it is readily distinguished by the decomposition which it undergoes when boiled with water for a considerable time. It is then gradually dissolved, splitting up in the following manner:—

$$C_8 H_7 N_3 O_5 + H_2 O = C_7 H_6 N_2 O_4 + CO_2 + N H_3.$$

The compound, $C_7 H_6 N_2 O_4$, thus formed is also an acid which will be described further on. The salts of the γ nitro-acid likewise suffer a similar decomposition when their aqueous solutions are boiled; and the substance mentioned in the footnote on p. 170, as crystallizing in yellowish-red needles, is the barium salt of the acid $C_7 H_6 N_2 O_4$, formed in this way.

Action of Tin and Hydrochloric Acid on the Isomeric Nitrouramidobenzoic Acids.

When α nitrouramidobenzoic acid is heated with tin and hydrochloric acid, it is reduced in the ordinary way, yielding α amidouramidobenzoic acid, $C_8 H_9 N_3 O_3 = C_8 H_7 (N H_2) N_2 O_3$, crystallizing in scales, ordinarily of a greyish-white colour. It is but slightly soluble in boiling water, still less so in hot alcohol, and almost insoluble in ether. Its silver salt is a white precipitate, having the formula $C_8 H_8 N_3 O_3 Ag$. Its hydrochloric-acid compound, $C_8 H_9 N_3 O_3, H Cl$, crystallizes in scales, and is marked by its great insolubility in hydrochloric acid, even when very dilute. When the aqueous solution of the latter compound is acted upon with sodium nitrite, an azo-compound separates, crystallizing in needles, which are soluble in hydrochloric acid.

β nitrouramidobenzoic acid also, when treated with tin and hydrochloric acid, is converted into an amido-acid, $C_8 H_7 (N H_2) N_2 O_3$, isomeric with that last described. This new acid, which I call β amidouramidobenzoic acid,

crystallizes in white scales, rather insoluble in hot water and extremely so in cold. Curiously enough, it does not possess the property of combining with acids, but with bases it forms salts having the general formula $C_8 H_8 N_3 O_3, M'$. Its silver salt is a white crystalline precipitate. This new amido-acid is remarkable for its instability; boiled with hydrochloric acid or with baryta-water, it is decomposed in the following manner:—

$$\underbrace{C_8 H_9 N_3 O_3}_{\beta \text{ amidouramidobenzoic acid.}} = \underbrace{C_8 H_6 N_2 O_3}_{\text{New acid.}} + \underbrace{NH_3}_{\text{Ammonia.}}.$$

The new acid, $C_8 H_6 N_2 O_3$, thus obtained forms small white nodules, which are insoluble in all ordinary neutral solvents. It combines with ammonia, forming a salt which crystallizes in difficultly soluble long needles. Its hot ammoniacal solution, when mixed with barium chloride, solidifies to a pulp of white needles, which, when dried between filter-paper, have the composition $(C_8 H_5 N_2 O_3)_2, Ba + 4H_2 O$. With respect to its constitution, I am inclined to regard it as amidobenzoic acid, in which one atom of hydrogen is replaced by the group [(CO)N]:

$$C_8 H_6 N_2 O_3 = C_7 H_4(NH_2) [((CO)N)] O_2,$$

and therefore propose to call it β amidocarboxamidobenzoic acid.

γ nitrouramidobenzoic acid, when treated with tin and hydrochloric acid, behaves differently from either of the other isomeric acids, not yielding an amido-acid, but suffering at once a much deeper decomposition, as represented in the following equation:—

$$\underbrace{C_8 H_7 N_3 O_5}_{\gamma \text{ nitrouramidobenzoic acid.}} + H_6 = \underbrace{C_8 H_6 N_2 O_3}_{\text{New acid.}} + \underbrace{NH_3}_{\text{Ammonia.}} + \underbrace{H_2 O}_{\text{Water.}}.$$

This new acid, $C_8 H_6 N_2 O_3$, which has the same composition as the β amidocarboxamidobenzoic acid just described, is not identical but only isomeric with it, and I shall therefore designate it by the name of γ amidocarboxamidobenzoic acid. It crystallizes in white needles, which are almost insoluble in water, alcohol, and ether.

Action of strong Nitric Acid on the Isomeric Nitrouramidobenzoic Acids.

In the earlier part of this notice it was stated that three isomeric dinitrouramidobenzoic acids were produced by the action of strong nitric acid on uramidobenzoic acid, but that their separation could not be conveniently effected, owing to the great resemblance in their properties. Any of these isomeric dinitro-acids, however, may be obtained in a pure state by dissolving the corresponding nitrouramidobenzoic acid in fuming nitric acid, free from nitrous acid. On diluting the solution with water, the dinitro-

acid is precipitated in the crystalline state. It will be seen from the following description of their properties how closely they resemble one another.

α dinitrouramidobenzoic acid, $C_8H_6(NO_2)_2N_2O_3$.—It crystallizes in yellowish-white needles, which are very readily soluble in alcohol or ether, but scarcely so in cold water. On the addition of barium chloride to even a very dilute ammoniacal solution of the acid, a bright yellow precipitate is produced, consisting of very small nodules, which in their turn are built up of microscopic needles or plates.

β dinitrouramidobenzoic acid, $C_8H_6(NO_2)_2N_2O_3$, has the same crystalline form as the *α* acid, which it closely resembles, although it would seem to be somewhat less soluble in alcohol and ether. Its barium salt is a yellow amorphous precipitate.

γ dinitrouramidobenzoic acid, $C_8H_6(NO_2)_2N_2O_3$.—This acid crystallizes in yellowish-white plates or needles, which behave towards solvents in a manner similar to the *α* acid. Its barium salt is obtained in long slender yellow needles, when a solution of the acid in ammonia is decomposed by barium chloride, and is somewhat more readily soluble than the corresponding salts formed by the two before-mentioned acids.

It can be readily understood that when an ammoniacal solution of either of these isomeric dinitro-acids is boiled for a considerable time, that it will be again reconverted into the corresponding mononitro-acid in a manner similar to that previously described as taking place with the mixture of the dinitro-acids :—

$$\underbrace{C_8H_6(NO_2)_2N_2O_3}_{\begin{matrix}\alpha,\\ \beta,\\ \gamma\end{matrix}\left\{\begin{matrix}\text{dinitrouramido-}\\ \text{benzoic}\\ \text{acid.}\end{matrix}\right.} + \underbrace{H_2O}_{\text{Water.}} = \underbrace{C_8H_7(NO_2)N_2O_3}_{\begin{matrix}\alpha,\\ \beta,\\ \gamma\end{matrix}\left\{\begin{matrix}\text{nitrouramido-}\\ \text{benzoic}\\ \text{acid.}\end{matrix}\right.} + \underbrace{NHO_3}_{\begin{matrix}\text{Nitric}\\ \text{acid.}\end{matrix}}$$

Decomposition of the Isomeric Dinitrouramidobenzoic Acids on boiling their Aqueous Solution.

All three of the dinitro-acids are decomposed when their aqueous solutions are boiled for a considerable time, gas being evolved and new acids formed, which have the composition $C_7H_6N_2O_4$. These three new acids, from their formula and properties, may be regarded as isomeric mononitro-derivatives of amidobenzoic acid, $C_7H_6N_2O_4 = C_7H_4(NO_2)(NH_2)O_2$, their method of formation being expressed in the following manner :—

$$\underbrace{C_8H_6N_4O_7}_{\begin{matrix}\text{Isomeric dinitro-}\\ \text{uramidobenzoic acids.}\end{matrix}} = \underbrace{C_7H_6N_2O_4}_{\begin{matrix}\text{Isomeric nitro-}\\ \text{amidobenzoic acids.}\end{matrix}} + \underbrace{CO_2}_{\begin{matrix}\text{Carbonic}\\ \text{acid.}\end{matrix}} + \underbrace{N_2O.}_{\begin{matrix}\text{Nitrous}\\ \text{oxide.}\end{matrix}}$$

The acid obtained in this manner from *α* dinitrouramidobenzoic acid, which I have called *α nitroamidobenzoic acid,* crystallizes in yellow needles or prisms, which are only slightly soluble in cold water, and rather diffi-

cultly so in hot; they dissolve readily in hot alcohol, but scarcely at all in ether. The barium salt forms yellowish-red needles, which are very readily soluble in water even in the cold. Dried between filter-paper they were found to have the composition $(C_7H_5N_2O_4)_2$, $Ba+3H_2O$.

β nitroamidobenzoic acid is the name by which I have designated the acid obtained from β dinitrouramidobenzoic acid by the above-mentioned reaction. It is only very slightly soluble in hot water, but rather easily so in boiling alcohol, from which it crystallizes on cooling in clusters of yellowish-red glistening needles or small plates. The dry acid, when gently heated, sublimes in shining rhombic plates without previously melting. Its barium salt forms bright yellowish-red prisms, often well defined, which are slightly soluble even in boiling water. Dried between filter-paper they have the composition $(C_7H_5N_2O_4)_2$, $Ba+2H_2O$, the water of crystallization not being entirely expelled below 190° C.

The γ nitroamidobenzoic acid obtained from the γ dinitrouramidobenzoic acid, by boiling its aqueous solution, is easily distinguished from its two before-mentioned isomerides, in being very readily soluble not only in hot water, but also in alcohol and ether, even in the cold. It crystallizes in yellow prisms, which melt, when heated, to a brownish oil; at a higher temperature it decomposes with slight explosion and evolution of yellowish vapours. Its barium salt forms reddish-yellow needles, which are very easily soluble even in cold water, and when dried between folds of bibulous paper have the composition $(C_7H_5N_2O_4)_2$, $Ba+7H_2O$.

Action of Tin and Hydrochloric Acid on the Isomeric Nitroamidobenzoic Acids.

If the isomeric nitroamidobenzoic acids are gently warmed with tin and hydrochloric acid, they are reduced to the corresponding diamido-acids in the following manner:—

$$\underbrace{C_7H_4(NO_2)(NH_2)O_2}_{\left.\begin{matrix}\alpha,\\ \beta,\\ \gamma\end{matrix}\right\}\text{ nitroamidobenzoic acids.}} + H_6 = \underbrace{C_7H_4(NH_2)_2O_2}_{\left.\begin{matrix}\alpha,\\ \beta,\\ \gamma\end{matrix}\right\}\text{ diamidobenzoic acids.}} + 2H_2O.$$

These diamido-acids are separated from the tin chloride, formed at the same time, by the ordinary methods.

α diamidobenzoic acid, $C_7H_4(NH_2)_2O_2$, crystallizes from its solution in boiling water, in which it is sparingly soluble, in minute but well-defined short prisms, which have a greyish tinge. It is remarkable for the extremely sparing solubility of its sulphate, $C_7H_8N_2O_2$, SH_2O_4, a compound crystallizing in white needles.

β diamidobenzoic acid crystallizes in pale yellow-coloured plates, which are very difficultly soluble in cold water, but rather readily so when it is hot. Its sulphate has the formula $[C_7H_4(NH_2)_2O_2]_2$, SH_2O_4, and is very sparingly soluble in hot water, although not so much so as the cor-

responding salt of the acid just described. It is almost entirely deposited on cooling in white, soft, shining, almost oval-shaped plates.

γ *diamidobenzoic acid* crystallizes in long yellowish-white needles, whose solubility closely resembles that of the two other isomeric acids. Its sulphate forms white six-sided tables or prisms, which, when dried in the air, have the composition $(C_7H_8N_2O_2)_2$, $SH_2O_4 + 1\frac{1}{2}H_2O$; they are almost as insoluble as the corresponding compound of the α diamido-acid. When the solution of γ diamidobenzoic acid in dilute hydrochloric acid is decomposed by ferric chloride, a brownish-red semicrystalline precipitate is obtained, consisting of a new acid, which, however, I have not at present more closely investigated.

Action of Nitrous Acid on the Isomeric Diamidobenzoic Acids.

In this reaction a remarkable difference is observed between the α diamidobenzoic acid on the one hand and β and γ diamidobenzoic acids on the other. When α diamidobenzoic acid is treated with a quantity of warm and moderately dilute hydrochloric acid, insufficient to dissolve the whole, and on cooling the filtered solution is mixed with one of sodium nitrite, a semisolid mass of crystals is formed. These, after separation of the mother-liquor, are easily purified by crystallization from hot water, with the addition of a small quantity of animal charcoal. The compound thus obtained forms long needles or small plates, which explode when heated in a dry state. It is rather easily soluble in hot water, and by long boiling gradually decomposes, giving rise to a brown insoluble amorphous precipitate. Curiously enough it does not possess acid properties, being insoluble both in ammonia and potassa; whilst, on the contrary, it combines with the mineral acids, forming well-crystallized salts. Its hydrochlorate forms six-sided plates, which are readily soluble. Unfortunately I have not yet been able to establish the composition of this basic compound with certainty, although from a determination of the gold in its gold salt, which crystallizes in dark yellow needles, I believe it to have the composition indicated by the formula $C_{14}H_{13}N_5O_4$. Its formation, therefore, would take place in the following manner:—

$$\underbrace{2(C_7H_8N_2O_4)}_{\alpha\text{ diamidobenzoic acid.}} + \underbrace{NHO_2}_{\text{Nitrous acid.}} = \underbrace{C_{14}H_{13}N_5O_4}_{\text{New compound.}} + \underbrace{H_2O.}_{\text{Water.}}$$

It is necessary to mention that this basic compound is not formed when sodium nitrite acts upon α diamidobenzoic acid in the presence of free hydrochloric acid, instead of in the manner just described. In this case a brisk evolution of nitrogen takes place, and after some time the yellow solution deposits an amorphous reddish-brown acid.

The behaviour of the two other isomeric diamidobenzoic acids under the circumstances previously mentioned is quite different to that of the α diamidobenzoic acid. When their solution in dilute hydrochloric acid

is acted upon by sodium nitrate, a white crystalline azo-acid immediately separates, and that whether the hydrochloric acid is in excess or not. The azo-acid thus obtained from the β diamidobenzoic acid crystallizes in short needles, which are soluble with difficulty in hot water, and scarcely at all in cold. When heated in a dry state it melts and blackens, a small portion subliming, whilst the greatest part is decomposed, leaving behind a difficultly combustible carbonaceous residue. The composition of this azo-acid corresponds with the formula $C_7H_5N_3O_2$. Its barium salt has the composition $(C_7H_4N_3O_2)_2$, $Ba+4H_2O$, and crystallizes in very slender colourless needles, which dissolve readily in hot water, but only sparingly in cold.

The azo-acid prepared from the γ diamidobenzoic acid, although isomeric with that just described, differs considerably from it, crystallizing in long hair-like silky needles, which, on drying, shrink together to a felt-like mass. It is rather more easily soluble in hot water than the corresponding α acid, and when gently heated melts to a yellow oil, partial sublimation taking place at the same time; at a higher temperature it decomposes with slight explosion. Its barium salt crystallizes in white needles, which are rather difficultly soluble in hot water, and very sparingly so in cold. Its composition is expressed by the formula $(C_7H_4N_3O_2)_2$, $Ba+2H_2O$.

The formation of these two azo-acids may be thus expressed:—

$$\underbrace{C_7H_8N_2O_2}_{\substack{\beta,\ \gamma\ \text{diamidoben-}\\ \text{zoic acids.}}} + \underbrace{NHO_2}_{\substack{\text{Nitrous}\\ \text{acid.}}} = \underbrace{C_7H_5N_3O_2}_{\beta,\ \gamma\ \text{azo-acids.}} + \underbrace{2H_2O.}_{\text{Water.}}$$

I may add that both these acids contain water of crystallization, which is expelled at 100° C., and at the same time call attention to their great stability, in which respect they differ very remarkably from most other azo-compounds. Finally, I would remark that three other compounds, having the same formula as these azo-acids, are already known, to which I gave the names diazobenzimide, diazodracylimide, and diazosalylimide*. They were obtained by the action of ammonia on the perbromides of the corresponding diazo-acids, as shown in the equation—

$$\underbrace{C_7H_4N_2O_2,HBr,Br_2}_{\substack{\text{Perbromides of}\ldots\ \text{diazobenzoic acid,}\\ \text{diazodracylic acid,}\\ \text{diazosalylic acid.}}} + \underbrace{4NH_3}_{\text{Ammonia.}} = \underbrace{\left.\begin{matrix}(C_7H_4N_2O_2)'' \\ H\end{matrix}\right\}N}_{\substack{\text{Diazobenzimide,}\\ \text{diazodracylimide,}\\ \text{diazosalylimide.}}} + \underbrace{3NH_4Br.}_{\substack{\text{Ammonium}\\ \text{bromide.}}}$$

These bodies also form saline compounds with metals, but in their reactions, as well as in other respects, differ entirely from the azo-acids just described, rendering it certain that they have a different constitution from them.

* Zeits. Chem. 1867, p. 164.

Decomposition of the Isomeric Diamidobenzoic Acids at a high temperature.

When either of the diamidobenzoic acids is submitted to dry distillation in a retort, it splits up into carbonic acid, which is evolved, and a yellow oil, which crystallizes in the neck of the retort, thus:—

$$C_7H_8N_2O_2 = C_6H_8N_2 + CO_2.$$

The compound $C_6H_8N_2$, obtained in this manner from the α diamidobenzoic acid, is moderately soluble in hot water, and crystallizes therefrom in reddish-coloured scales, melting at 140° C. It exhibits the character of a base, and forms crystalline salts with the mineral acids. It can readily be seen that the composition of this base is the same as that of the phenylene diamines described by Dr. Hofmann*, $C_6H_8N_2 = C_6H_4(NH_2)_2$; and its properties leave no doubt that it is identical with one of the latter, namely, that formed by the action of reducing agents on the nitraniline prepared from substituted anilides.

The compound $C_6H_8N_2$, obtained by the dry distillation of β diamidobenzoic acid, differs not only from the base just described, but also from the phenylene diamine which Dr. Hofmann obtained by the reduction of dinitrobenzol. It is easily soluble in hot water, and crystallizes therefrom in white rectangular four-sided tables or plates, which usually have a reddish tinge. It melts at 99° C., and boils at about 252° C. It is likewise possessed of basic properties, and must be regarded as a new isomeric modification of phenylene diamine. The sulphate of this base crystallizes in pearly scales, the composition of which corresponds to the formula $C_6H_4(NH_2)_2, SH_2O_4 + 1\frac{1}{2}H_2O$, and which readily part with their water of crystallization at a temperature a little above 100° C. Its platinum-salt is precipitated in the form of brownish-red needles. When ferric chloride is added to a solution of this base in hydrochloric acid, ruby-red needles immediately form, consisting of the hydrochloride of a new base. In a free state the latter forms bright yellow microscopic needles, which are almost insoluble in all neutral solvents. I have reason to believe that it has the formula $C_{12}H_{10}N_4$, and that its method of formation is as follows:—

$$\underbrace{2(C_6H_8N_2)}_{\text{New phenylene diamine.}} + O_3 = \underbrace{C_{12}H_{10}N_4}_{\text{New base.}} + \underbrace{3H_2O.}_{\text{Water.}}$$

As one or other of the phenylene diamines just mentioned is obtained when the α or β diamidobenzoic acid is submitted to dry distillation, I fully expected that the γ diamidobenzoic acid, under the same circumstances, would give rise to a third phenylene diamine, and that this would be identical with that derived from dinitrobenzol. Strange to say, this is not the case, the phenylene diamine obtained by the dry distillation of γ

* Proc. Roy. Soc. vol. xii. p. 639.

diamidobenzoic acid being precisely the same as that produced in the same way from β diamidobenzoic acid.

The subjoined Table shows the characteristic difference that exists between the melting-points and boiling-points of the three isomeric phenylene diamines at present known:—

	Fusing-point.	Boiling-point.
Phenylene diamine from nitroacetanilid and from α diamidobenzoic acid	140°	267°
Phenylene diamine from β and γ diamidobenzoic acids	99°	252°
Phenylene diamine from dinitrobenzol	63°	287°

Besides the three isomeric diamidobenzoic acids which I have described in the course of this paper, a fourth acid exists, having the same composition, namely, the diamidobenzoic acid obtained by Dr. Voit* by reducing ordinary dinitrobenzoic acid with sulphuretted hydrogen, and which I have since further investigated†. I propose, at present, to retain for this the simple name diamidobenzoic acid. It is distinguished from the other diamidobenzoic acids, not only by its physical properties, but especially in not yielding a volatile organic base when submitted to dry distillation, becoming, on the contrary, completely carbonized with simultaneous evolution of ammonia. With nitrous acid also it behaves differently, being converted into an insoluble amorphous acid of a reddish-brown colour.

When it is considered that each of the four diamidobenzoic acids at present known is derived, at least in a certain sense, from ordinary α amidobenzoic acid, there is reason to suppose that the acids which are isomeric with amidobenzoic acid, namely amidodracylic acid and anthranilic acid, would, under favourable circumstances, give rise to four new diamido-acids; so that it is obvious that at least twelve isomeric diamidobenzoic acids may be conceived to exist.

In the preceding notice I have confined myself to the consideration of some of the most important chemical and physical properties of the substances described; but I hope to be able to lay before the Society at a future time some account of their rational constitution, and of the causes which produce the various isomerisms.

* Ann. Chem. Pharm. vol. xcix. p. 100.

† *Ibid.* vol. cxxviii.

February 22, 1872.

WILLIAM SPOTTISWOODE, M.A., Treasurer and Vice-President, in the Chair.

The following communications were read:—

I. "On the Contact of Surfaces." By WILLIAM SPOTTISWOODE, M.A., Treas. R.S. Received January 18, 1872.

(Abstract.)

In a paper published in the Philosophical Transactions (1870, p. 289), I have considered the contact, at a point P, of two curves which are coplanar sections of two surfaces (U, V); and have examined somewhat in detail the case where one of the curves, viz. the section of V, is a conic. In the method there employed, the condition that the point P should be sextatic, involved the azimuth of the plane of section measured about an axis passing through P; and consequently, regarded as an equation in the azimuth, it showed that the point would be sextactic for certain definite sections. It does not, however, follow, if conics having six-pointic contact with the surface U be drawn in the planes so determined, that a single quadric surface can be made to pass through them all. The investigation therefore of the memoir above quoted was not directly concerned with the contact of surfaces, although it may be considered as dealing with a problem intermediate to the contact of plane curves and that of surfaces.

In the present investigation I have considered a point P common to the two surfaces U and V, an axis drawn arbitrarily through P, and a plane of section passing through the axis and capable of revolution about it. Proceeding as in the former memoir, and forming the equations for contact of various degrees, and finally by rendering them independent of the azimuth, we obtain the conditions for contact for all positions of the cutting plane about the axis. Such contact is called circumaxal; and in particular it is called uniaxal, biaxal, &c., according as it subsists for one, two, &c. axes. If it holds for all axes through the point, it is called superficial contact; and in the memoir some theorems are established relating to the number of sections along which contact of a given degree must subsist in order to ensure uniaxal contact, as well as to the connexion between uniaxal and multiaxal contact. At the conclusion of § 3 it is shown that the method of plane sections may, in the cases possessing most interest and importauce, be replaced by the more general method of curved sections.

In the concluding section a few general considerations are given relating to the determination of surfaces having superficial contact of various degrees with given surfaces; and at the same time I have indicated how very much the general theory is affected by the particular circumstances of each case. The question of a quadric having four-pointic superficial con-

tact with a given surface is considered more in detail; and it is shown how in general such a quadric degenerates into the tangent plane taken twice. To this there is apparently an exceptional case, the condition for which is given and reduced to a comparatively simple form; but I must admit to having so left it, in the hope of giving a fuller discussion of it on a future occasion.

The subject of three-pointic superficial contact was considered by Dupin, 'Développements de Géométrie,' p. 12; and, as I have learnt since the memoir was written, a general theorem connecting superficial contact and contact along various branches of the curve of intersection of two surfaces (substantially the same as that given in the text) was enunciated by M. Moutard*.

In a corollary to this theorem, M. Moutard states that through every point of a surface there can be drawn twenty-seven conics, having six-pointic contact with the surface. This number is perhaps open to question; and I have even reason to think, from considerations stated to me by Mr. Clifford, that the number ten given in my memoir above quoted may be capable of reduction by unity to nine. But this question refers to the subject of that earlier memoir rather than to this.

II. "On a New Hygrometer." By Wildman Whitehouse, Esq. Communicated by Sir W. Thomson, F.R.S. Received January 6, 1872.

The use of Mason's wet-bulb thermometer as a means of hygrometric measurement, though it be admitted to be the most practically useful, and indeed the only recording instrument for the purpose, has yet this serious inconvenience, not to say defect, viz. that its indications either cease or are valueless at temperatures below 32° F.

In a conversation which the writer had with the Director of the Meteorological Office some months ago, the question arose whether any thing could be suggested to remedy this inconvenience.

It was obviously inadmissible to substitute any other fluid for, or to make any addition to, the water employed for the wet bulb, as then it would cease to be a test for the purely hygrometric capacity of the air. It became therefore necessary to fall back in another direction, and to find some hygrometric body which should readily and rapidly absorb moisture from the air, and at the same time afford some means of measuring and recording the amount of such absorption.

Fused chloride of zinc or of calcium seemed promising as very active agents, absorbing rapidly on their surface, and allowing the readiest possible escape of the fluid hydrate for measurement; yet no means presented itself either of accurately measuring, regulating, or maintaining the exact extent of surface exposed for absorption; nor could the substance itself be

* Poncelet, 'Applications d'Analyse à la Géométrie,' 1864, tom. ii. p. 363.

easily renewed when required, nor, indeed, could either of these substances be regarded as wholly free from the interference of frost, as the moisture absorbed from the atmosphere at a temperature much below freezing-point may remain frozen on the surface, and become incapable of continuous measurement. It seemed essential to the accuracy and practical utility of any instrument designed for this purpose :—

1st. That a fixed and invariable extent of surface should at all times be exposed for absorption of moisture;

2nd. That the apparatus should be simple, inexpensive, and not inconvenient in use;

3rd. That the hygrometric substance should be continuously and steadily renewable; and above all, if it were possible,

4th. That the measurement should be effected thermometrically.

No solid hygrometric substance seemed capable of meeting these requirements; but all the conditions seemed likely to be fulfilled by the use of concentrated sulphuric acid. This would admit of being spread in an exquisitely fine film over the surface of the bulb of a thermometer by means of a glass capillary siphon, of which one end should rest on the upper part of the bulb, while the other end dipped into a reservoir of the acid. A continuous supply could be maintained for any required length of time by suitable arrangements. The absorption of moisture would necessarily be attended by a rise in temperature, and this would be proportioned to the amount of hygrometric moisture absorbed; while the hydrated acid, having fulfilled its office, would fall in drops from the bulb into any tube or reservoir placed for the purpose.

An instrument has been constructed by the writer to test this principle, which has, by the courtesy of the Director of the Meteorological Office, been under observation for some weeks.

It consists essentially of three thermometers of similar construction, and used as a "wet bulb," a "dry bulb," and an "acid bulb," respectively, placed side by side on a suitable frame, and read together for comparison.

The experience already gained in the use of this instrument has shown that, with a reservoir of proper construction, the supply of acid may be made continuous for any required length of time, and that, from the very slight variations of flow which occur in its action, the supply to the thermometer will be sensibly equable.

The length of the siphon, and the size of the capillary bore, together with the difference of level between the surface of the fluid in the trough of the reservoir and the point of delivery on the bulb, will determine the rate of supply of the acid.

It is clear that either a too rapid and continuous stream of acid at the temperature of the air, or a too scanty supply, would diminish the readings; yet it is found that practically there may be a pretty wide range of variation in the supply of acid, within which no essential change in the sensibility of the instrument is noticed.

For a bulb having one square inch of surface one drop per minute is sufficient, though the time may range from 40 to 100 seconds without inconvenience, the time being noted as the hydrated acid, after having fulfilled its office, falls drop by drop from the bulb.

The quantity of acid required at this rate is about 3 fluid ozs. per diem, or one imperial pint per week, which is procurable of uniform density, sufficiently pure and free from lead, at a cost of about $2\frac{1}{2}$*d.*

The temperature of the acid in the reservoir is of course that of the surrounding air; the elevation of temperature shown by the acid-bulb thermometer is due to, and seems to be strictly a measure of, the amount of moisture absorbed by the film of acid spread on the surface of the bulb, say one square inch, continuously supplied in its concentrated state, and as constantly passing off hydrated.

While, therefore, this instrument is, like Mason's, intended to measure the amount of hygrometric moisture in the air, and to do so thermometrically, it yet is, in its principle and in its operation, essentially of an opposite character.

The ordinary wet-bulb thermometer is at the zero of its scale in an atmosphere of perfect saturation, and its action depends upon the amount of sensible heat absorbed and rendered latent by evaporation of the water from its surface.

The acid-bulb thermometer is at its zero in a perfectly dry atmosphere, and its action depends upon the amount of latent heat rendered sensible by the condensation of vapour into water on the surface of the bulb, and by the combination of this water with the concentrated acid.

It would appear that an hygrometer on this principle is entirely free from the action of frost; while its sensibility is so great as to be at first almost embarrassing.

This may, however, be easily regulated and toned down, if necessary, to any required range by the dilution of the acid with glycerine, a fluid which is also of itself hygrometric, though its thermal effects are far less marked than those of sulphuric acid.

The following series of observations, made hourly and otherwise, at intervals during the past few weeks, at the Meteorological Office, by the kindness of the Director, will suffice to show approximately the relations of the "acid" and the "wet bulb" respectively.

They have been chiefly actual out-door observations, and have extended over a considerable range of temperature and atmospheric variations.

It will require a most careful series of observations to elicit all the points noteworthy in the new instrument, and to determine the relative values of the wet and acid-bulb readings, noting the behaviour of each at every part of the scale, from absolute dryness to saturation, and at temperatures ranging from 75° or 80° down to 0°.

This will be necessary before the instrument can aspire to take its place among the recognized standards of meteorological science; but in the

mean time the writer has been advised to offer, at the earliest time, a brief description of it to the notice of the Royal Society.

TABLE.—Giving comparison of Readings of Wet- and Acid-bulb Hygrometers.

1871.		Mason's Hygrometer.			Mr. Whitehouse's.		Deductions from Wet-bulb Hygrometer.			Remarks.
Day.	Hour.	Dry Bulb.	Wet Bulb.	Difference, D−W.	Acid Bulb.	Difference, A−D.	Tension of Vapour.	Weight of Vapour in 1 cub. foot of Air.	Relative Humidity. Saturation = 100.	
	Acid flowing from bulb 1 drop in 33 seconds.									
	h. m.	°	°	°	°	°	in.	grs.		
Nov. 6 ..	10 7 A.M.	40·0	36·0	4·0	60·2	20·2	·172	2·0	69	
„ ..	10 30 „	40·5	36·3	4·2	61·5	21·0	·173	2·0	69	
„ ..	11 25 „	41·0	36·7	4·3	61·3	20·3	·175	2·0	68	At noon the wet bulb was washed and resupplied with water.
„ ..	11 33 „	41·0	37·0	4·0	62·0	21·0	·180	2·1	70	
„ ..	1 4 P.M.	43·0	38·0	5·0	64·0	21·0	·181	2·1	65	
„ ..	1 50 „	44·0	38·5	5·5	65·2	21·2	·180	2·1	62	
„ ..	2 5 „	44·0	39·0	5·0	65·3	21·3	·188	2·2	65	Sky has gradually become overcast since the morning.
„ ..	2 20 „	43·7	38·8	4·9	65·9	22·2	·188	2·2	66	
„ ..	2 30 „	44·0	39·0	5·0	65·7	21·7	·188	2·2	65	
„ ..	3 30 „	44·0	39·3	4·7	66·3	22·3	·193	2·3	67	
„ ..	3 55 „	44·0	39·6	4·4	67·0	23·0	·198	2·3	68	
Nov. 7 ..	9 45 A.M.	45·0	43·0	2·0	75·0	30·0	·253	2·9	85	Note the change in night! Dark and foggy day.
„ ..	10 10 „	45·5	44·0	1·5	77·8	32·3	·270	3·1	89	(!)
„ ..	10 20 „	46·0	44·2	1·8	76·0	30·0	·269	3·1	87	
„ ..	10 30 „	46·0	44·2	1·8	76·2	30·2	·269	3·1	87	
„ ..	11 5 „	46·8	45·0	1·8	75·2	28·4	·277	3·1	87	(!)
„ ..	11 35 „	47·7	45·6	2·1	78·0	30·3	·276	3·2	87	
„ ..	1 15 P.M.	50·0	47·0	3·0	82·0	32·0	·286	3·3	80	Vapour has increased, but humidity has decreased.
„ ..	3 15 „	50·0	47·0	3·0	82·4	32·4	·286	3·3	80	
„ ..	3 50 „	50·0	47·0	3·0	82·9	32·9	·286	3·3	80	
„ ..	4 0 „	50·0	47·0	3·0	83·0	33·0	·286	3·3	80	
„ ..	5 0 „	49·5	46·5	3·0	83·5	34·0	·281	3·2	80	
„ ..	6 0 „	49·5	46·5	3·0	82·5	33·0	·281	3·2	80	
„ ..	8 0 „	51·0	47·0	4·0	81·5	30·5	·276	3·1	74	
„ ..	9 0 „	49·5	46·0	3·5	79·5	30·0	·270	3·0	76	
Nov. 8 ..	10 40 A.M.	47·5	45·5	2·0	81·8	34·3	·280	3·3	86	Sky overcast; slight misty drizzle.
„ ..	10 50 „	50·3	48·0	2·3	89·0	38·7	·305	3·5	84	Vapour increased; humidity decr.
„ ..	11 15 „	50·0	47·6	2·4	88·0	38·0	·300	3·4	83	
„ ..	11 25 „	50·0	47·0	3·0	85·3	35·3	·286	3·3	80	
„ ..	NOON	50·3	47·0	3·3	82·0	31·7	·283	3·3	78	Sky clearing since 11.25 A.M.
„ ..	1 50 P.M.	51·0	45·5	5·5	81·0	30·0	·246	2·8	63	Humidity greatly reduced; vapour not so much.
„ ..	2 30 „	49·7	43·0	6·7	75·2	25·5	·210	2·4	59	Vapour decreased in greater proportion than humidity.
„ ..	3 0 „	49·0	43·2	5·8	76·0	27·0	·221	2·5	63	
„ ..	3 50 „	48·7	43·0	5·7	74·7	26·0	·219	2·5	66	

TABLE (*continued*).

1871. Day.	Hour.	Mason's Hygrometer. Dry Bulb.	Wet Bulb.	Difference, D−W.	Mr. Whitehouse's. Acid Bulb.	Difference, A−D.	Deductions from Wet-bulb Hygrometer. Tension of Vapour.	Weight of Vapour in 1 cub. foot of Air.	Relative Humidity. Saturation = 100.	Remarks.
	h. m.	°	°	°	°	°	in.	grs.		
Nov. 9 ..	9 0 A.M.	37·0	35·0	2·0	58·5	21·5	·182	2·1	83	Vapour *much* decreased; yet humidity has risen. Note acid-bulb.
" ..	9 10 "	37·0	35·0	2·0	58·0	21·0	·182	2·1	83	Fine day; rather cloudy P.M.
" ..	0 20 P.M.	45·0	40·0	5·0	68·0	23·0	·197	2·3	66	Vapour slightly increased, but humidity decreased. Note acid-bulb.
" ..	2 0 "	45·0	39·2	5·8	66·0	21·0	·184	2·1	61	
" ..	3 0 "	45·0	39·0	6·0	66·7	21·7	·181	2·1	60	
" ..	3 30 "	45·0	39·2	5·8	66·0	21·0	·184	2·1	61	
" ..	3 45 "	44·5	39·0	5·5	65·7	21·2	·185	2·1	63	
" ..	6 0 "	42·0	38·5	3·5	63·5	21·5	·198	2·3	76	Slight shower at 4.10 P.M.
" ..	6 30 "	42·0	38·5	3·5	62·5	20·5	·198	2·3	76	
" ..	7 0 "	41·0	38·0	3·0	62·0	21·0	·197	2·3	77	
" ..	8 0 "	39·5	37·0	2·5	63·0	23·5	·194	2·3	80	
" ..	9 0 "	39·5	36·5	3·0	61·0	21·5	·185	2·1	77	
Nov. 10..	11 30 A.M.	43·0	39·0	4·0	67·0	24·0	·181	2·1	65	Note these changes.
" ..	11 40 "	42·5	39·0	3·5	67·2	24·7	·202	2·3	75	
" ..	3 10 P.M.	45·0	39·0	6·0	68·0	23·0	·181	2·1	60	
" ..	6 0 "	42·5	37·0	5·5	62·0	19·5	·170	1·9	63	
" ..	6 30 "	41·5	37·0	4·5	61·0	19·5	·177	2·0	67	
" ..	7 30 "	40·0	36·5	3·5	60·0	20·0	·180	2·1	73	
" ..	8 0 "	39·5	36·0	3·5	61·0	21·5	·176	2·1	74	
" ..	8 30 "	38·5	35·5	3·0	60·0	21·5	·177	2·1	76	
" ..	9 0 "	38·5	35·0	3·5	59·5	21·0	·169	2·0	80	
Nov. 11..	8 50 A.M.	33·0	32·0	1·0	52·5	19·5	·167	2·0	89	Foggy and cold.
" ..	9 50 "	35·0	33·0	2·0	55·0	20·0	·164	1·9	80	Vapour increased; humidity steady.
" ..	11 20 "	39·0	36·5	2·5	62·0	23·0	·190	2·2	80	
	Acid flowing about 1 drop in 77 seconds.									
Nov. 4 ..	2 50 P.M.	48·0	44·5	3·5	?71·5	?23·5	·253	2·9	76	Acid-reading doubtful; taken too soon after starting.
" ..	3 10 "	47·3	44·0	3·3	74·3	27·0	·258	2·9	77	
" ..	3 30 "	47·5	44·3	3·2	75·0	27·5	·255	2·85	78	
" ..	3 50 "	47·2	44·0	3·2	74·0	26·8	·251	2·9	78	Humidity unaltered; vapour decreased.
Nov. 24..	11 24 A.M.	44·0	41·2	2·8	66·5	22·5	·228	2·6	78	
" ..	5 30 P.M.	42·0	38·5	3·5	63·3	21·3	·199	2·3	75	
" ..	6 0 "	41·5	38·0	3·5	63·0	21·5	·193	2·3	75	
" ..	6 30 "	41·5	37·5	4·0	63·0	21·5	·185	2·2	71	
Nov. 25..	11 30 A.M.	38·5	36·8	1·7	61·0	22·5	·200	2·3	86	Vapour hardly changed, humidity very largely increased. Note acid
Nov. 29..	11 15 "	39·0	37·0	2·0	61·5	22·5	·199	2·3	84	
" ..	11 25 "	39·0	37·0	2·0	[illegible]	[illegible]				
" ..	3 45 P.M.	40·0	37·3	2·7	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
" ..	5 0 "	39·0	37·5	1·5	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
" ..	6 0 "	[illegible]	[illegible]	2·0	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
" ..	6 30 "	[illegible]	[illegible]	2·5	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Nov. 30..	2 0 "	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
" ..	4 0 "	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
" ..	4 30 "	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
" ..	5 0 "	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
" ..	5 30 "	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

February 29, 1872.

FRANCIS GALTON, M.A., Vice-President, in the Chair.

The following communications were read :—

I. "On Putrefaction." By Dr. F. Crace-Calvert, F.R.S.

Received February 22, 1872.

(Abstract.)

This paper is intimately connected with those I have already published on protoplasmic life and the influence it exerts on putrefaction.

I have already shown that when albumen from a new-laid egg is introduced into *pure distilled water* and communication with the atmosphere prevented, protoplasmic life does not appear. If the same solution, however, be exposed to the atmosphere for from fifteen to forty-five minutes, minute globular bodies appear having an independent motion, which I denominate monads. The time required varies according to the time of the year, the amount of moisture present in the atmosphere, and the temperature.

Although M. Pasteur has already noticed the meteorological conditions which influence that life, he has not noticed the extraordinary rapidity with which the fluids are impregnated, and that this impregnation is proportional to the surface exposed.

On the 18th of May, 1871, two portions of albumen, measuring 400 grains, were placed, the one in a test-tube having a diameter of $\frac{3}{4}$ inch, the other in a test-glass which at the surface of the liquid had a diameter of 2 inches. In the tube vibrios appeared after twelve days, whilst in the glass only five days were required for their development. If in place of pure distilled water the water supplied by the Manchester Corporation (which is one of the purest waters in England) was used, the time required for the development of vibrios in a test-tube was only twenty-four hours.

These experiments prove that the rate of development of vibrio-life is influenced by the extent of surface exposed.

M. Pasteur has already demonstrated that oxygen is essential to the life of the Mucedines, but I am not aware that it has been proved that this gas is necessary to the existence of vibrio-life.

In the hope of throwing some light on this subject, the following experiments were made :—

Into each of five glass bulbs equal volumes of a solution of albumen in Manchester water were placed, and the first left in contact with the atmosphere for twenty-four hours, after which the ends of the tube were hermetically sealed about 2 inches on each side of the bulb. After passing oxygen, hydrogen, nitrogen, and carbonic acid over the other four solutions, the tubes were also hermetically sealed. These tubes were kept

closed for twenty-seven days, during which it was observed that the albumen in the bulb containing oxygen speedily became turbid, then the one containing air, while the other three remained clear. After this period the tubes were broken and the contents examined. A large quantity of vibrio-life was found in those containing oxygen and common air, whilst those containing nitrogen, carbonic acid, and hydrogen contained very small quantities, that with hydrogen the least,—thus provi ng that oxygen is an essential element to the production of putrefactive vibrios.

In further support of this view, I may state that under certain conditions these animalcules produce such an amount of carbonic acid and other gases as to exclude oxygen to such an extent that their own development and life are impaired.

This is easily proved by taking albumen full of animalcules, but not emitting any putrid odour, and placing it in test-tubes, closing some and leaving others open. If these tubes are examined after a few weeks, it will be observed that in those left in the air life has much increased, and they emit a very putrid odour; whilst the life in the closed tubes not only has not increased, but appears to be in a dormant condition; for if the corks are removed and the fluid again comes in contact with the oxygen of the air, its activity returns. The albumen also in the closed tubes does not emit any putrid odour.

M. Pasteur has also found that oxygen was necessary to the vibrios of putrefaction, although the same gas destroyed those produced in butyric fermentation; but he has not made any experiments to show that the products emitted by such vibrios are prejudicial to their development, and even to their power of locomotion.

Having stated above that liquids exposed to the atmosphere become impregnated with monads, I will now try to describe their gradual development into vibrios, and their ultimate transformation into microzyma.

A few hours after the albuminous fluid becomes impregnated, the monads, which have a diameter of about $\frac{1}{128000}$ of an inch, appear to form masses. Then some of the monads become elongated into vibrios, which, though attached to the mass, have an independent motion; so that as the force exerted by the vibrios predominates towards one or another direction, so is the mass moved over the field of the microscope. As the development proceeds, the mass is broken up, and ultimately each vibrio has an independent existence, and may be seen swimming or rolling about in the fluid. Their size at this stage is about $\frac{1}{20000}$ of an inch. These, which I call ordinary vibrios, gradually grow into long vibrios, which attain a length of $\frac{1}{6400}$ of an inch.

These long vibrios gradually become changed into cells, which I have called microzyms. The first process in the transformation is its division into two independent bodies. One extremely faint line appears across the centre of the animalcule, and increases in distinctness until the vibrio appears like two smaller vibrios joined together. The separation takes place

and each part acquires an independent existence. These parts again divide, and the process of subdivision is carried on until they appear to be nothing more than cells, which have a swimming-power so great as to pass over the field of the microscope with rapidity.

After twelve or eighteen months all the vibrios disappear and are replaced by microzyms, either in motion or at rest. If these microzyms are placed in a solution of fresh albumen, vibrios are abundantly developed. The apparent explanation of this fact is that in the fresh albumen they have all the circumstances favourable to their growth and reproduction, while the putrid albumen has become so completely modified as to be incapable of affording them the requisite conditions for reproduction.

I may also notice that at the same time a deposit has taken place which, under the microscope, appears to consist of shoals of small particles of matter which have no life. The solution has now become perfectly clear, possesses considerable refractive power, and has lost the property of becoming coagulated by heat.

The albumen solution does not emit a putrid odour until after the formation of the above-mentioned deposit, and the amount of odour is in direct ratio to the number of vibrios present.

I remarked during the investigation the presence of several other forms of animalcules which contribute to the decomposition and putrefaction of proteine substances, the description of which will be found in the original, memoir.

II. "On the Relative Power of Various Substances in preventing Putrefaction and the Development of Protoplasmic and Fungus-Life." By Dr. F. Crace-Calvert, F.R.S. Received February 22, 1872.

(Abstract.)

To carry out this series of experiments, small test-tubes were thoroughly cleansed, and heated to dull redness. Into each was placed 26 grammes of a solution of albumen containing one part of white of egg to four parts of pure distilled water, prepared as described in my paper on protoplasmic life. To this was added one thousandth, or ·026 gramme, of each of the substances the action of which I desired to study.

The reasons why I employed one part in a thousand are two-fold. First, the employment of larger proportions would, in some instances, have coagulated the albumen; secondly, it would have increased the difficulty of observing the relative powers of the most efficacious antiseptics in preventing the development of the germs of putrefaction or decay.

A drop was taken from each of the tubes, and examined under a microscope having a magnifying-power of 800 diameters. This operation was repeated daily with the contents of each tube for thirty-nine days, and from

time to time for eighty days. During this time the tubes were kept in a room the temperature of which did not vary more than 3 degrees, namely, from 12·5° C. to 15·5° C.

In order the better to show the influence of the antiseptics used, I examined two specimens of the same solution at the same time, one of which was kept in the laboratory, the other in the open air.

A marked difference was observed in the result, the one kept outside becoming impregnated with animal life in less than half the time required by the other, while as many vibrios were developed in six days in the tube kept outside as were developed in thirty days in the tube in the laboratory.

A summary of the results of the experiments is given in the following Table, in which the substances are grouped according to their chemical nature:—

	Days required for development of Fungi.	Days required for development of Vibrios.
1. *Standard Solutions.*		
Albumen kept in laboratory for comparison	18	12
Albumen exposed outside laboratory	None	5
2. *Acids.*		
Sulphurous acid	21	11
Sulphuric acid............................	9	9
Nitric acid	10	10
Arsenious acid............................	18	22
Acetic acid	9	30
Prussic acid..............................	None	9
3. *Alkalies.*		
Caustic soda	18	24
Caustic potash............................	16	26
Caustic ammonia...........................	20	24
Caustic lime..............................	None	13
4. *Chlorine Compounds.*		
Solution of chlorine......................	22	7
Chloride of sodium........................	19	14
Chloride of calcium.......................	18	7
Chloride of aluminium	21	10
Chloride of zinc	53	None
Bichloride of mercury.....................	81	None
Chloride of lime	16	9
Chlorate of potash	19	17
5. *Sulphur Compounds.*		
Sulphate of lime	19	9
Protosulphate of iron	15	7
Bisulphite of lime	18	11
Hyposulphite of soda	18	11

	Days required for development of Fungi.	Vibrios.
6. *Phosphates.*		
Phosphate of soda	17	13
Phosphate of lime	22	7
7.		
Permanganate of potash	22	9
8. *Tar Series.*		
Carbolic acid	None	None
Cresylic acid	None	None
9. *Sulphocarbolates.*		
Sulphocarbolate of potash	17	18
Sulphocarbolate of soda	19	18
Sulphocarbolate of zinc	17	None
10.		
Sulphate of quinine	None	25
Picric acid	19	17
Pepper	None	8
Turpentine	42	14
11.		
Charcoal	21	9

In comparing the results stated in the above Table, the substances can be classed under four distinct heads, viz.:—those which prevent the development of protoplasmic and fungus-life; those which prevent the production of vibrio-life, but do not prevent the appearance of fungus-life; those which permit the production of vibrio-life, but prevent the appearance of fungus-life; and those which do not prevent the appearance of either protoplasmic or fungus-life.

The first class contains only two substances, carbolic and cresylic acids.

In the second class, also, there are only two compounds, chloride of zinc and bichloride of mercury.

In the third class there are five substances, lime, sulphate of quinine, pepper, turpentine, and prussic acid.

In the fourth class is included the remaining twenty-five substances.

The acids, while not preventing the production of vibrio-life, have a marked tendency to promote the growth of fungus-life. This is especially noticeable in the case of sulphuric and acetic acids.

Alkalies, on the contrary, are not favourable to the production of fungus-life, but promote the development of vibrios.

The chlorides of zinc and mercury, while completely preventing the development of animalcules, do not entirely prevent fungus-life; but I would call special attention to the interesting and unexpected results obtained in

the cases of chlorine and bleaching-powder. When used in the proportion above stated they do not prevent the production of vibrio-life. In order to do so they must be employed in excess; and I have ascertained, by a distinct series of experiments, that large quantities of bleaching-powder are necessary. I found that part of the carbon was converted into carbonic acid, and part of the nitrogen was liberated.

If, however, the bleaching-powder be not in excess, the animal matter will still readily enter into putrefaction. The assumption on which its employment as a disinfectant has been based, namely, that the affinity of the chlorine for hydrogen is so great as to destroy the germs, is erroneous.

The next class to which I would call attention is the tar series, where neither the carbolic nor the cresylic acid fluids gave any signs of vibrionic or fungus-life during the whole eighty days during which the experiments were conducted.

The results obtained with sulphate of quinine, pepper, and turpentine, deserve notice. None of them prevent the development of vibrio-life; but sulphate of quinine and pepper entirely prevent the appearance of fungi. This fact, together with the remarkable efficacy of sulphate of quinine in intermittent fever, would lead to the supposition that this form of disease is due to the introduction into the system of fungus-germs; and this is rendered the more probable, if we bear in mind that these fevers are prevalent only in low marshy situations, where vegetable decay abounds, and never appear to any extent in dry climates, even in the midst of dense populations, where ventilation is bad and putrefaction is rife.

The results obtained in the case of charcoal show that it possesses no antiseptic properties, but that it prevents the emanation of putrid gases, owing to its extraordinary porosity, which condenses the gases, thus bringing them into contact with the oxygen of the atmosphere, which is simultaneously condensed.

The above results have been confirmed by a second series.

A series of experiments was also undertaken, substituting gelatine for albumen, and was continued for forty-seven days.

Vibrios appeared in two days in the standard gelatine-solution, and bacteria after four or five; and during the whole time of the experiment, life was far more abundant than in the albumen-solution. A distinct putrid smell was emitted after twenty-six days.

With bleaching-powder it took twenty days for life to appear, instead of seven, as in the case of albumen; while at no time during the twenty-seven days which remained was life abundant. No putrid odour was emitted; but a mouldy one could be detected on the thirtieth day.

With chlorine-solution vibrio-life was observed only after forty days; no putrid nor mouldy smell was given off at any time.

The protosulphate of iron gave, with this solution, results quite different from those with albumen, in which, it will be remembered, vibrios appeared in seven days, and fungi after fifteen; whilst with gelatine neither proto-

plasmic nor fungus-life appeared during the time the experiments were continued.

Another substance, arsenious acid, also presented a marked difference in its action in the two solutions; for although with albumen twenty-two days elapsed before vibrios were present, and eighteen before fungi, with gelatine animal life appeared after two days, and at no time did any fungi exist. The effects of the other substances with gelatine were so similar to those with albumen, that it is unnecessary to state them here.

III. "On the Relative Power of Various Substances in arresting Putrefaction and the Development of Protoplasmic and Fungus-Life." By Dr. F. Crace-Calvert, F.R.S. Received February 22, 1872.

(Abstract.)

This series of experiments was undertaken as being complementary to those described in my last paper, and consisted in adding to a solution of albumen, swarming with microscopic life, one-thousandth part of the substances already enumerated in that paper, and examining the results produced immediately after the addition of the substances, and after one, six, and sixteen days; but in this abstract only the results obtained in the first and last cases will be noticed.

The solutions were placed in test-tubes similar to those described in my last paper.

The experiments were begun on the 20th Sept. 1871, the solutions being kept at a temperature of 15°–18° C.

In the standard solution the amount of life and putrescence increased during the whole of the time.

The first class includes those substances which completely destroyed the locomotive power of the vibrios immediately, and completely prevented their regaining it during the time the experiments were conducted:—

Cresylic acid.

The second class contains those compounds which nearly destroyed the locomotive power of all the vibrios present when added, and afterwards only one or two could be seen swimming about in each field:—

Carbolic acid, sulphate of quinine, chloride of zinc, and sulphuric acid.

The third class are those which acted injuriously on the vibrios on their addition, leaving only a small number retaining the power of swimming, but which allowed the vibrios gradually to increase in number, the fluid, nevertheless, containing less life after sixteen days than the standard putrid albumen-solution:—

Picric acid and sulpho-carbolate of zinc.

The fourth class includes those substances which acted injuriously at first, but permitted the vibrios to regain their former locomotive power,

so that the fluid after sixteen days contained as much vibrio-life as the standard putrid albumen:—

Chloride of aluminium, sulphurous acid and prussic acid.

The fifth class contains those compounds which acted injuriously at first, destroying the locomotive power of most of the vibrios, but which afterwards permitted the vibrios to increase more rapidly than in the standard albumen-solution:—

Bleaching-powder, bichloride of mercury, chlorine-solution, caustic soda, acetic and nitric acids, sulphate of iron, and the sulpho-carbolates of potash and soda.

The sixth class contains those compounds which exercised no action on the animalcules, either at first or after sixteen days:—

Arsenious acid, common salt, chloride of calcium, chlorate of potash, sulphate of lime, bisulphite of lime, hyposulphite of soda, phosphate of lime, turpentine, and pepper.

The seventh class includes those substances which favour the production of animalcules and promote putrefaction:—

Lime, charcoal, permanganate of potash, phosphate of soda, and ammonia.

Presents received February 1, 1872.

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February 29, 1872.

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March 7, 1872.

GEORGE BIDDELL AIRY, C.B., President, in the Chair.

In accordance with the Statutes, the names of the Candidates for election into the Society were read as follows:—

Andrew Leith Adams, Surgeon-Major.
William Grylls Adams, M.A.
William Aitken, M.D.
Sir Alexander Armstrong, K.C.B., M.D.
Edward Middleton Barry, R.A.
John Beddoe, B.A., M.D.
Henry Bowman Brady, F.L.S.
Frederick Joseph Bramwell, C.E.
James Brunlees, C.E.
Edwin Kilwick Calver, Capt. R.N.
Alexander Carte, M.A., M.D.
William Chimmo, Commander R.N.
Prof. Arthur Herbert Church, M.A.
Frederick Le Gros Clark, F.R.C.S.
Prof. John Cleland, M.D.
Herbert Davies, M.D.
Henry Dircks, F.C.S.
August Dupré, Ph.D.
Michael Foster, jun., M.A., M.D.
Peter Le Neve Foster, M.A.
Wilson Fox, M.D.
Arthur Gamgee, M.D.
Prof. Thomas Minchin Goodeve, M.A.
Townshend M. Hall, F.G.S.
Edmund Thomas Higgins, M.R.C.S.
Rev. Thomas Hincks, B.A.
Rev. A. Hume, LL.D.
Henry Hyde, Lieut.-Col. R.E.
Prof. William Stanley Jevons, M.A.
Edmund Charles Johnson, F.R.G.S.
George Johnson, M.D.
Prof. Thomas Rupert Jones.
John Leckenby, F.G.S.
Clements R. Markham, Sec. Geog. Soc.
William Mayes, Staff-Comm. R.N.
Edmund James Mills, D.Sc.
Thomas George Montgomerie, Major R.E.
Robert Stirling Newall, F.R.A.S.
Edward Latham Ormerod, M.D.
Francis Polkinghorne Pascoe, F.L.S.
Prof. Oliver Pemberton.
Rev. Stephen Joseph Perry.
John Arthur Phillips, F.C.S.
Bedford Clapperton T. Pim, Capt. R.N.
William Overend Priestley, M.D.
Charles Bland Radcliffe, M.D.
Edward John Routh, M.A.
George West Royston-Pigott, M.D.
William Westcott Rundell.
William James Russell, Ph.D.
Osbert Salvin, M.A.
Harry Govier Seeley, F.L.S.
Alfred R. C. Selwyn (Geol. Survey Canada).
Peter Squire, F.L.S.
George James Symons, V.P. Met. Soc.
Edwin T. Truman, M.R.C.S.
Wildman Whitehouse, C.E.
Henry Woodward, F.G.S.
Archibald Henry Plantagenet Stuart Wortley, Lieut.-Col.

The following communications were read:—

I. "Letter from W. De La Rue, F.R.S., to Prof. Stokes, Sec. R.S., relative to the Observations taken with the Kew Heliograph."

The Observatory, Cranford, Middlesex, W.,
February 5th, 1872.

My dear Sir,—I beg herewith to hand you a statement of the expenditure of the £200 I received in February 1871 from the Government-Grant Fund for working the Kew heliograph and measuring and reducing the photographic pictures from February 1st, 1871, to January 30, 1872. I enclose the vouchers.

The work which I undertook to superintend during the last ten years has been brought to a close, so far as the observations are concerned. It has, however, been impossible to keep up the measurements of the sun-pictures and their reduction to the day. Indeed it will take eighteen months to bring these to a close; I have agreed with Mrs. Whipple (formerly Miss Beckley) and Mr. Loewy to complete this work for a specific sum (£170). As the funds placed at my disposal are exhausted, I will defray this expense myself.

It may be of interest to the Government-Grant Committee to know that, during the ten years (1862, February, to the end of January 1871), 2778 pictures have been obtained on 1724 days.

Since the Kew instrument was devised and constructed, I have been able, with the aid of Mr. Dalmeyer, to introduce many improvements, both in the mechanical parts and the optical portion of the instrument; but I did not think it desirable to interrupt the sun-work to make any changes in the Kew heliograph. Now that the work is completed, I would recommend that the secondary magnifier should be changed to the form adopted in the instrument made for the Russian Government, under my direction, and now at work at Wilna, by which very much better pictures are obtained than those procured at Kew. The cost of this change would be about £30.

II. "On the Organization of the Fossil Plants of the Coal-measures. —Part III. *Lycopodiaceæ.*" By W. C. Williamson, F.R.S., Professor of Natural History in Owen's College, Manchester. Received February 29, 1872.

(Abstract.)

An outline of the subject of this memoir has already been published in the Proceedings* in a letter to Dr. Sharpey. In a former memoir the author described the structure of a series of Lepidodendroid stems, apparently belonging to different genera and species. He now describes a very similar series, but all of which, there is strong reason for believing, belong

* *Suprà*, p. 95.

to the same plant, of which the structure has varied at different stages of its growth. The specimens were obtained from some thin fossiliferous deposits discovered by Mr. G. Grieve of Burntisland, in Fifeshire, where they occur imbedded in igneous rocks. The examples vary from the very youngest, half-developed twigs, not more than $\frac{1}{12}$ of an inch in diameter, to arborescent stems having a circumference of from two to three feet. The youngest twigs are composed of ordinary parenchyma, and the imperfectly developed leaves which clothe them externally have the same structure. In the interior of the twig there is a single bundle, consisting of a limited number of barred vessels. In the centre of the bundle there can always be detected a small amount of primitive cellular tissue, which is a rudimentary pith. As the twig expanded into a branch, this central pith enlarged by multiplication of its cells, and the vascular bundle in like manner increased in size through a corresponding increase in the number of its vessels. The latter structure thus became converted into the vascular cylinder, so common amongst Lepidodendroid plants, in transverse sections of which the vessels do not appear arranged in radiating series. Simultaneously with these changes the thick parenchymatous outer layer becomes differentiated. At first but two layers can be distinguished—a thin inner one, in which the cells have square ends, and are disposed in irregular vertical columns, and a thicker outer one consisting of parenchyma, the same as the epidermal layer of the author's preceding memoir. In a short time a third layer was developed between these two.

When the vascular cylinder had undergone a considerable increase in its size and in the number of its vessels, a new element made its appearance. An exogenous growth of vessels took place in a cambium layer, which invested the preexisting vascular cylinder. The author distinguishes the latter as the vascular medullary cylinder, and the former as the ligneous zone. The newly added vessels were arranged in radiating laminæ, separated from each other by small but very distinct medullary rays. At an earlier stage of growth traces of vascular bundles proceeding from the central cylinder to the leaves had been detected. These are now very clearly seen to leave the surface of the medullary vascular cylinder where it and the ligneous zone are in mutual contact; hence tangential sections of the former exhibit no traces of these bundles, but similar sections of the ligneous zone present them at regular intervals and in quincuncial order. Each bundle passes outwards through the ligneous zone, imbedded in a cellular mass, which corresponds, alike in its origin and in its direction, with the ordinary medullary rays, differing from them only in its larger dimensions. At this stage of growth the plant is obviously identical with the *Diploxylon* of Corda, with the *Anabathra* of Witham, and, so far as this internal axis is concerned, with the *Sigillaria elegans* of Brongniart. The peculiar medullary vascular cylinder existing in all these plants is now shown to be merely the developed vascular bundle of ordinary Lycopods, whilst the exogenous radiating ligneous zone enclosing

that cylinder is an additional element which has no counterpart amongst the living forms of this group.

Though the central compound cellulo-vascular axis continued to increase in size with the general growth of the plant, it was always small in proportion to the size of the stem. The chief enlargement of the latter was due to the growth of the bark, which exhibited three very distinct layers,—an inner one of cells with square ends, and slightly elongated vertically and arranged in irregular vertical rows, an intermediate one of prosenchyma, and an outer one of parenchyma. These conditions became yet further modified in old stems. The exogenous ligneous zone became very thick in proportion to the medullary vascular cylinder, and the differences between the layers of the bark became yet more distinct. These differences became the most marked in the prosenchymatous layer; at its inner surface the cells are prosenchymatous, but towards its exterior they become yet more elongated vertically, their ends being almost square, whilst numbers of them of exactly equal length are arranged in lines radiating from within outwards. These oblong cells often pass into a yet more elongated series with somewhat thickened walls, which become almost vascular, constituting a series of bast-fibres. In the transverse sections these prosenchymatous cells are always arranged, like the vessels of the ligneous zone, in radiating lines. Yet more external is the sub-epidermal parenchyma passing into leaves composed of the same kind of tissue. The petioles of the leaves have been long, if not permanently, retained in connexion with the stem, a character of Corda's genus *Lomatophloios*.

Where young twigs branch, the vascular medullary cylinder divides longitudinally into two parts; the transverse section of this cylinder now resembles two horse-shoes pointing in opposite directions. The break in the continuity of each half of the cylinder occasioned by the division is never closed by new vessels belonging to the cylinder; but when the stem develops exogenously, the cambium-layer, from which the new growths originated, has endeavoured to surround these openings in the cylinder, and, by closing them, once more to separate the medullary from the cortical tissues. Some beautiful specimens have been obtained, which exhibit these new exogenous layers in process of formation. The vessels or the young layers are not half developed. At first they meander vertically through masses of delicate cellular tissue; but they soon arrange themselves in regular radiating vessels and cells, becoming mere outward prolongations of the woody wedges and medullary rays of the older part of the stem. At this stage of their growth, the walls of the vessels are deeply indented by the contiguous cells, as if the plastic tissues of the former had been moulded upon the latter structures. As the new vessels enlarge, the superfluous intervening cells disappear, until each medullary ray finally consists of a single vertical pile of from one to a small number of cells, arranged as in many Coniferæ. The exceptional cases are those where

vascular bundles pass outwards to the leaves; these bundles have protected the contiguous cells above and below them from the pressure of the enlarging ligneous vessels and limited their absorption. Both these and the smaller ordinary rays pass outwards in horizontal and parallel lines. The evidences of an exogenous mode of growth afforded by these young, half-developed layers of wood is clear and decisive.

The Burntisland deposits are full of fragments of strobili, especially of torn sporangia and of macrospores. Several fine *Lepidostrobi* have been obtained like those to which the fragments have belonged, and which the author believes to have been the fruits of the stems described. The structure of these strobili is very clear and of interest; the primary branches from the central axis subdivide, so that each sporangium rests upon a separate bract, from the upper surface of which a vertical lamina arises, and, extending the entire length of the sporangium, ascends far into its interior, where it bifurcates. The cellular walls of the sporangium blend with the bract along each side of the base of this sporangiophore. The microspores occupy the upper part of the *Lepidostrobus*, and are usually triplospores, sometimes tetraspores. The macrospores occupy the lowermost sporangia, are of large size, and are very remarkable from having their external surfaces clothed with numerous projecting caudate appendages, each one of which is slightly capitate at its extremity. So far as the author is aware, this is an undescribed form of macrospore.

Two new forms of *Lepidodendron* are described from the Oldham beds, in one of which the medullary axis attains to an unusually large size, even in the young shoots; whilst the other is remarkable for the magnitude of its leaves. It is obvious that the plant which is the chief subject of the memoir is a true example of Corda's genus *Diploxylon*, so far as its woody axis is concerned; whilst its bark and leaves are those of a *Lomatophloios*, and its slender twigs are *Lepidodendra*. The author also points out the probability that the plant had a true Stigmarian root.

The structure of these fossil types is compared with that of recent *Lycopodiaceæ*. The vascular medullary cylinder is shown to be an aggregation of the foliar vascular bundles, so that the vascular connexion between the leaves and the stem is maintained exclusively by means of these vessels, which thus correspond most closely with the central vascular axes of living Lycopods. On the other hand, the exogenous layers do not communicate directly with the leaves in any way—but are homologous with the corresponding layers in the Stigmarian root, in which latter they receive the vascular bundles from the rootlets. The medullary cylinder does not enter the roots, but appears to terminate at the base of the stem, though the pith is prolonged through them. Hence it seems probable that the nutritive matters were taken up from the soil by the Stigmarian rootlets, that they ascended into the Diploxyloid stem through the exogenous layer, but that, in order to reach the leaves, if conveyed by the vessels and not by the cellular tissues, they had to be transferred by endosmosis to

those of the medullary cylinder. The bark of the fossil plants is compared with those of *Lycopodium chamæcyparissus* and *Selaginella Martensii,* which two combined represent the former.

These discoveries necessitate some changes in generic nomenclature, since the several parts of the plant not only represent the three genera above mentioned, but also several others. Meanwhile some other errors require correction. Corda erroneously defined his genus *Diploxylon* as having no medullary rays, and Brongniart relied upon this distinction in separating *Diploxylon* from *Sigillaria*; but no difference exists between the ligneous structures of the two genera, so far as *Sigillaria* is illustrated by Brongniart's *S. elegans.* Corda, Brongniart, and King all agree in regarding *Diploxylon* (which is identical with Witham's *Anabathra*) as belonging to the Gymnospermous Exogens. The necessity for abandoning this separation of the plants in question from the *Lycopodiaceæ,* urged in the author's previous memoir, is now made more obvious than before, the distinctions upon which the great French botanist relied in his classification being now shown to be such as mere differences of age can produce. The author concludes from his own observations that the genera *Diploxylon, Anabathra, Lomatophloios,* and *Leptoxylon* must be united. Brongniart had already brought into one generic group Corda's genera *Lomatophloios, Leptoxylon,* and *Calamoxylon,* Göppert's genus *Pachyphyllum,* and Sternberg's genus *Lepidophloios,* giving the latter name to the whole. Hence no less than six obsolete generic names are disposed of. The author finally follows Brongniart in adopting the term *Lepidophloios,* and *temporarily* assigns to the plant described the trivial name of *L. brevifolium.* The further relations of this genus to more ordinary forms of *Lepidodendron* require further investigation.

Much credit is due to G. Grieve, Esq., of Burntisland, for the energy with which he has worked amongst the deposits at Burntisland; and the author acknowledges his great obligations to that gentleman for liberal supplies of specimens for examination.

March 14, 1872.

The EARL OF ROSSE, D.C.L., Vice-President, in the Chair.

The following communications were read:—

I. "Contributions to the History of the Opium Alkaloïds.—Part IV." By C. R. A. Wright, D.Sc., Lecturer on Chemistry in St. Mary's Hospital Medical School. Received January 29, 1872.

§ 1. *On the Action of Hydriodic Acid on Morphia in presence of Phosphorus.*

It has been shown in Part III. of these researches* that when hydriodic acid acts on codeia in presence of phosphorus, a series of products are ulti-

* *Suprà*, p. 8.

mately obtained which may be considered as formed by the following train of reactions :—

$$\underset{\text{Codeia.}}{C_{18}H_{21}NO_3} + HI = CH_3I + \underset{\text{Morphia.}}{C_{17}H_{19}NO_3},$$

$$\underset{\text{Morphia.}}{C_{17}H_{19}NO_3} + 2HI = I_2 + \underset{\text{Hypothetical new base X.}}{C_{17}H_{21}NO_3},$$

$$\underset{\text{Hypothetical new base X.}}{C_{17}H_{21}NO_3} + 2HI = I_2 + H_2O + \underset{\text{Hypothetical new base Y.}}{C_{17}H_{21}NO_2}.$$

These two hypothetical bases, X and Y, then serve as the foundations of two series of new products expressible by the general formulæ :—

$$4X + nHI \pm pH_2O,$$
$$4Y + nHI \pm pH_2O.$$

In accordance with these views, it might be anticipated that, on treating morphia with hydriodic acid and phosphorus, either the same products, or at least products belonging to these same series, would ultimately result, which is in fact the case.

The morphia used in these experiments was presented for the purpose by Messrs. Macfarlane of Edinburgh, to whom the writer has already been so much indebted for similar acts of liberality ; the hydriodic acid was prepared as described in Part III., and contained 50 to 55 per cent. of HI.

On dissolving morphia by the aid of heat in about four times its weight of this acid, a marked brown coloration is visible, indicating the separation of iodine ; on adding phosphorus and continuing to heat, this colour ultimately disappears, a colourless syrupy liquid being obtained, which is freed from amorphous phosphorus and the phosphorus acids produced during the reaction by filtration through asbestos while hot, precipitation by water &c., precisely as in the case of the similar codeia products. On thus treating codeia, one or other of three products appear to be formed, according to the temperature employed, viz. :—

At 100°........ $4X + 6HI$.
At 110°–115° .. $4X + 6HI - 2H_2O$.
Boiled up to 130° $4Y + 6HI - 2H_2O$.

In the case of morphia, however, the resulting product is the same at whatever of these temperatures the reaction ensues, and has the composition $4X + 6HI - 2H_2O$. Thus the following numbers were obtained after complete drying at 100° :—

(A) Digested four hours at 100°.

0·3695 grm. gave 0·5920 CO_2 and 0·1710 H_2O.
0·3325 „ 0·2410 AgI.

(B) Gently boiled ten minutes.

0·3955 grm. gave 0·6240 CO_2 and 0·1770 H_2O.
0·3465 „ 0·2530 AgI.
0·4420 „ 0·3260 AgI.

(C) Boiled till the thermometer stood at 132°.

0·3150 grm. gave 0·4990 CO_2 and 0·1400 H_2O.
0·4405 „ 0·3280 AgI.

	Calculated.		Found. A.	B.	C.	Mean.
C_{68}	816	43·40	43·69	43·03	43·20	43·31
H_{86}	86	4·58	5·14	4·97	4·94	5·02
I_6	762	40·53	39·17	39·46 39·86	40·24	39·68
N_4	56	2·98				
O_{10}	160	8·51				
$C_{68}H_{82}I_2N_4O_{10}, 4HI$	1880	100·00				

Hence this product is formed from morphia in accordance with the equation

$$4C_{17}H_{19}NO_3 + 14HI = 2H_2O + 4I_2 + C_{68}H_{82}I_2N_4O_{10}, 4HI.$$

In physical properties and qualitative reactions the substance thus got is indistinguishable from the product of the same composition obtained from codeia; like the codeia product, too, it loses the elements of HI on long-continued boiling with water.

§ 2. *Action of Water on the foregoing Compounds.*

When the original substance $C_{68}H_{82}I_2N_4O_{10}, 4HI$ is boiled for five hours with about three hundred times its weight of water, a liquid is obtained from which white flakes separate on cooling; these have the same curious microscopical structure as the body similarly obtained from codeia, and gave the following numbers after drying at 100°:—

0·3680 grm. gave 0·6220 CO_2 and 0·1770 H_2O.
0·4240 „ 0·7165 CO_2 and 0·2050 H_2O.
0·3780 „ 0·2520 AgI.

	Calculated.		Found.	
C_{68}	816	46·58	46·10	46·09
H_{85}	85	4·85	5·34	5·37
I_5	635	36·24	36·02	
N_4	56	3·20		
O_{10}	160	9·19		
$C_{68}H_{81}IN_4O_{10}, 4HI$	1752	100·06		

Hence this substance is formed by the reaction

$$C_{68}H_{82}I_2N_4O_{10}, 4HI = HI + C_{68}H_{81}IN_4O_{10}, 4HI,$$

identical with that which takes place with the corresponding compound in the codeia series.

When the compound $C_{68}H_{81}IN_4O_{10}, 4HI$ from codeia is further treated

with excess of water and boiled for several hours, a further elimination of the elements of HI has been shown to take place, the end product having the composition $C_{68}H_{80}N_4O_{10}, 4HI$; as stated in Part III., however, it is very difficult to push this reaction to its extreme. Precisely the same facts are observable with the above morphia product; by boiling this with three hundred times its weight of water for three hours, half the basic iodine it contains is eliminated as HI, forming a product which may be either a mixture of equivalent quantities of $C_{68}H_{81}IN_4O_{10}, 4HI$ and $C_{68}H_{80}N_4O_{10}, 4HI$, or a single substance of the formula

$$C_{136}H_{161}IN_4O_{10}, 8HI.$$

If this latter be the case, the formulæ hitherto attributed to the derivatives from codeia and morphia obtained by the action of HI are only half the true ones; and the formation of this substance may be expressed by the equations

$$C_{136}H_{164}I_4N_8O_{20}, 8HI = 2HI + C_{136}H_{162}I_2N_8O_{20}, 8HI,$$
$$C_{136}H_{162}I_2N_8O_{20}, 8HI = HI + C_{136}H_{161}IN_8O_{20}, 8HI.$$

The following considerations tend to show that this body is a single substance and not a mixture:—

1st. By treating the compound hitherto described as $C_{68}H_{81}IN_4O_{10}, 4HI$ from codeia with water, a body which has the composition of

$$C_{136}H_{161}IN_8O_{20}, 8HI$$

is produced previously to the production of the substance hitherto described as $C_{68}H_{80}N_4O_{10}, 4HI$. Now it is not probable that in two separate instances *one* compound should split up into mixtures of *two* bodies of analogous though slightly different constitutions, these two being formed in each case *in equivalent quantities*.

2nd. A body which is without doubt a single compound, and which has the formula $C_{136}H_{153}IN_8O_{20}, 8HI$, has been produced (as will be described in a subsequent communication) by the simultaneous action of HI and P on a polymeride of codeia obtained from that base by the action of phosphoric acid: in physical and chemical properties this product much resembles the two bodies thus obtained from morphia and codeia products by the action of water; and hence these two bodies probably contain, like it, C_{136} associated with I in the base.

In order to show the resemblance between, or rather the identity of the codeia and morphia products, the formulæ given in Part III. have been adopted in this paper (viz. those containing C_{68}); but the author has no doubt that each of the substances has really double the formula ascribed to it (*i. e.* that each contains C_{136}).

The substances of composition $C_{136}H_{161}IN_8O_{20}, 8HI$ obtained, as abovementioned, from codeia and morphia products by the continued action of water gave the following numbers on analysis after drying at 100°:—

A. Codeia product,	0·3985 grm. gave	0·7050 CO_2 and 0·200 H_2O.	
	0·3670 „	0·2280 AgI.	
B. Morphia product,	0·3190 „	0·5635 CO_2 and 0·1610 H_2O.	
	0·3140 „	0·5560 CO_2 and 0·1570 H_2O.	
	0·2840 „	0·1775 AgI.	

	Calculated.		Found. A.	Found. B.	
C_{136}	1632	48·34	48·25	48·17	48·29
H_{169}	169	5·01	5·55	5·61	5·55
I_9	1143	33·86	33·57	33·77	
N_8	112	3·31			
O_{20}	320	9·48			
$C_{136}H_{161}IN_8O_{20}$, 8HI	3376	100·00			

In physical character and chemical deportment the derivatives from morphia obtained as above described are indistinguishable from those of the same composition obtained from codeia. The physiological experiments of Mr. Stocker given in the next section show that no particular difference is discernible in this respect also; hence it is concluded that the codeia products are not merely isomeric, but are identical with the corresponding morphia derivatives.

From the fact that hydriodic acid alone does not eliminate methyl from codeia in the form of methyl iodide, but causes the separation of free iodine, it appears more probable that the formation of the compound $C_{68}H_{82}I_2N_4O_{10}$, 4HI, obtained by the action of hydriodic acid in presence of the phosphorus on codeia, is brought about in accordance with the equations:—

(1) $4C_{18}H_{21}NO_3 + 14HI = 4I_2 + 2H_2O + C_{72}H_{90}I_2N_4O_{10}, 4HI$,

(2) $C_{72}H_{90}I_2N_4O_{10}, 4HI + 4HI = 4CH_3I + C_{68}H_{82}I_2N_4O_{10}, 4HI$,

rather than the equations

(3) $C_{18}H_{21}NO_3 + HI = CH_3I + C_{17}H_{19}NO_3$,

(4) $C_{17}H_{19}NO_3 + 2HI = I_2 + C_{17}H_{21}NO_3$,

(5) $4C_{17}H_{21}NO_3 + 6HI = 2H_2O + C_{68}H_{82}I_2N_4O_{10}, 4HI$;

i. e. that the action does *not* consist in the production of morphia from codeia and its subsequent alteration by addition of H_2, polymerization, addition of 2HI, and subtraction of $2H_2O$; but that these alterations take place in the codeia molecule before the elimination of methyl as CH_3I, this elimination forming the last stage instead of the first. This circumstance may account for the non-production from morphia of compounds belonging to the series $4Y + nHI \pm pH_2O$, which are formed from codeia when the temperature of the reaction reaches 130°, and under other circumstances; for it might naturally be expected that the elimination of the

methyl group would place a portion of the molecule in a quasi-nascent condition, thereby rendering further changes more easy.

The foregoing experiments, taken into consideration together with those formerly obtained in conjunction with the late Dr. Matthiessen *, lead to several noteworthy conclusions and speculations.

(1) The actions of hydrochloric, hydrobromic, and hydriodic acids on morphia and codeia are not precisely analogous; thus the action of HCl appears to give rise more especially to products derived from non-polymerized bases; *e. g.*, chlorocodide, which regenerates ordinary codeia by the action of water†. By the action of HBr, codeia yields not only bases apparently formed from non-polymerized codeia (bromocodide, deoxycodeia, deoxymorphia), but also bases derived from polymerized codeia and accordingly containing at least C_{72}, and probably C_{144} (chloro- and bromotetracodeia). Hydriodic acid, on the other hand, yields no body whose formula can be written as containing less carbon than C_{34}; and from the physical characters of the first products of the action and the constitution of their derivatives (many of which contain at least C_{68} and some apparently C_{136}), this proportion of carbon must certainly be doubled and probably quadrupled.

Hence HCl yields single-molecule derivatives chiefly; HBr yields single-molecule derivatives, and also polymeride derivatives, the polymerides containing at least C_{68} or C_{72} (possibly the formulæ attributed to bromotetracodeia and analogous bases may require doubling, as the physical character of the bases and their salts indicate that they belong to the same rank as the iodine derivatives); HI yields polymeride derivatives only.

(2) It being assumed that the molecules of codeia and morphia contain respectively either C_{18} and C_{17} or C_{36} and C_{34} (which latter is probably the case, experiments now in progress indicating that the molecular formulæ of these bases are double those usually ascribed to them), the above experiments lead to the conclusion that there exist polymerides of these alkaloids containing C_{72}, C_{144}, ..., or C_{68}, C_{136}, ..., these polymerides being formed by the action of strong acids, and serving as starting-points for new series of derivatives. Experiments to obtain these polymerides in an unaltered condition are, as has been previously stated, in progress, and apparently with success.

This facile disposition to form polymerides is not an unknown feature in alkaloids, the experiments of Anderson having shown that the pyridine bases are characterized by this property. This fact would appear to warrant the speculation that morphia and codeia contain carbon groups analogous

* Matthiessen and Wright, Proc. Roy. Soc. vol. xvii. pp. 455, 460; and vol. xviii. p. 83.

† Experiments are in progress which appear to show that the action of HCl on both codeia and morphia is capable of giving rise, when pushed to an extreme, of bases insoluble in ether, and of characters similar to chloro- and bromo-tetracodeia, with less ease, however, than HBr.

to, if not identical with, those contained in the pyridine bases; and, in fact, experiments now in progress, in conjunction with Herr L. Mayer, apparently lead to the conclusion that pyridine is obtainable from morphia derivatives by treatment which, though energetic, is nevertheless far short of destructive distillation: indeed it may be doubted whether the carbon groups contained in the pyridine series of bases do not preexist in the bodies from which these bases are obtained by destructive distillation.

(3) A comparison between the formulæ of the products obtained by the three hydracids HCl, HBr, and HI shows that while the action of HCl is simply to replace OH by Cl, or to remove the elements of H_2O (sometimes also replacing CH_3 by H), that of HBr is (in addition to the changes produced by HCl) to cause the addition of hydrogen to that one of the two resulting products that is derived from the non-polymerized molecule. Thus

HCl and codeia give $C_{17}H_{17}NO_2$ apomorphia,
HBr „ „ $C_{17}H_{19}NO_2$ deoxymorphia,

which may be represented as $C_{17}H_{19}NO_3 - H_2O + H_2$.

This hydrogenizing action is carried still further in the case of the derivatives obtained by HI; thus the expressions

$$4C_{17}H_{19}NO_3 - 2H_2O + 2HI + H_8,$$
$$4C_{17}H_{19}NO_3 - 6H_2O + 2HI + H_{16}$$

represent the composition of the bases obtained respectively from morphia and codeia at 130°.

(4) Codeia appears to be a species of methylic ether of morphia, their relative constitutions being probably

$$C_{17}H_{17}NO\begin{cases}OH\\OCH_3\end{cases} \text{ and } C_{17}H_{17}NO\begin{cases}OH\\OH\end{cases}$$

(doubling the formulæ will not alter their relations in this respect). Adhering to the formula hitherto employed, the production of the same apomorphia from both alkaloids is readily accounted for thus:—

$$\underset{\text{Codeia.}}{C_{17}H_{17}NO\begin{cases}OH\\O.CH_3\end{cases}} + HCl = \underset{\text{Chlorocodide.}}{C_{17}H_{17}NO\begin{cases}Cl\\O.CH_3\end{cases}} + H_2O,$$

$$\underset{\text{Chlorocodide.}}{C_{17}H_{17}NO\begin{cases}Cl\\O.CH_3\end{cases}} = \begin{Bmatrix}Cl\\CH_3\end{Bmatrix} + \underset{\text{Apomorphia.}}{C_{17}H_{17}NO\{O''},$$

$$\underset{\text{Morphia.}}{C_{17}H_{17}NO\begin{cases}OH\\OH\end{cases}} = H_2O + \underset{\text{Apomorphia.}}{C_{17}H_{17}NO\{O''}.$$

According to this view morphia should contain two hydroxyl groups for every C_{17}, and codeia only one. Experiments are contemplated, with reference to this point, on the action of aniline, acetyl chloride, and glacial acetic acid on these alkaloids, whereby it is hoped that definite information may be gained as to the presence, or otherwise, and the number of the groups CHO (aldehyde group), OH, &c.

(5) It appears not improbable that codeia and morphia may contain in their molecules benzene residues. Schiff has pointed out* that phenols give colorations with ferric chloride, whereas the corresponding ethers or anisols do not do so; the well-known distinction between morphia and codeia in this respect, therefore, gives some support to the idea that both may be benzene derivatives.

§ 3. *On the Physiological Action of some of the foregoing Derivatives. By* REGINALD STOCKER, *M.B., Pathologist in St. Mary's Hospital Medical School.*

Doses of 1 decigramme of the compound $C_{68}H_{81}IN_4O_{10}, 4HI$ from codeia, and of the similar compound from morphia, were given to an adult terrier by the mouth without producing any perceptible effect whatever; when the dose was increased to 3 decigrammes, in each case repeated defæcation in the course of a few hours was produced, the stools being more loose than ordinarily and frequently of a dark greenish colour; no other symptom was noticeable, and no appreciable difference in the action of the two compounds was perceptible.

Doses of 5 decigrammes of the compound $C_{68}H_{82}I_2N_4O_{10}, 4HI$ from each of these sources were given to the same dog by the mouth, with the result of producing similar repeated defæcation in the course of two or three hours; the sole difference discernible between these and the former experiments being that the effect was produced somewhat sooner and was of longer continuance in the latter cases, a result probably produced solely by the larger dose. No material differences were observed between the codeia and morphia derivative.

The same dog was employed throughout, two or three days being allowed to intervene between each experiment, so that the animal had recovered from the effects of a former dose before the administration of another.

It would hence appear that the derivatives of polymerized $C_{17}H_{21}NO_3$ are less active than those of polymerized $C_{17}H_{19}NO_3$; and also that there is no reason for considering the derivatives from codeia as different from those of morphia, the corresponding bodies having respectively the same quantitative composition and the same physical, chemical, and physiological properties.

II. "Further Investigations on Planetary Influence upon Solar Activity." By WARREN DE LA RUE, D.C.L., F.R.S., BALFOUR STEWART, LL.D., F.R.S., and BENJAMIN LOEWY, F.R.A.S. Received January 30, 1872.

1. In a previous communication by us to this Society, an Abstract of which was published in the Proceedings, vol. xiv. p. 59, we showed some

* Ann. Chem. Pharm. vol. clix. p. 158.

grounds for believing that the behaviour of sun-spots with regard to increase and diminution, as they pass across the sun's visible disk, is not altogether of an arbitrary nature. From the information which we then had, we were led to think that during a period of several months sun-spots will on the whole attain their minimum of size at the centre of the disk; they will then alter their behaviour so as on the whole to diminish during the whole time of their passage across the disk; thirdly, their behaviour will be such that they reach a maximum at the centre; and, lastly, they will be found to increase in size during their whole passage across the disk. These various types of behaviour appeared to us always to follow one another in the above order; and in a paper printed for private circulation in 1866, we discussed the matter at considerable length, after having carefully measured the area of each of the groups observed by Carrington, in order to increase the accuracy of our results. In this paper we obtained nineteen or twenty months as the approximate value of the period of recurrence of the same behaviour.

2. A recurrence of this kind is rather a deduction from observations more or less probable than an hypothesis; nevertheless, it appeared to us to connect itself at once with an hypothesis regarding sun-spot activity. "The average size of a spot" (we remarked) "would appear to attain its maximum on that side of the sun which is turned away from Venus, and to have its minimum in the neighbourhood of this planet." In venturing a remark of this nature, we were aware it might be said "How can a comparatively small body like one of the planets so far away from the sun cause such enormous disturbances on the sun's surface as we know sun-spots to be?" It ought, however, we think, to be borne in mind that in sun-spots we have, *as a matter of fact*, a set of phenomena curiously restricted to certain solar latitudes, within which, however, they vary according to some complicated periodical law, and presenting also periodical variations in their frequency of a strangely complicated nature. Now these phenomena must either be caused by something within the sun's surface, or by something without it. But if we cannot easily imagine bodies so distant as the planets to produce such large effects, we have equal difficulty in imagining any thing beneath the sun's surface that could give rise to phenomena of such a complicated periodicity. Nevertheless, as we have remarked, sun-spots do exist, and obey complicated laws, whether they be caused by something within or something without the sun. Under these circumstances, it does not appear to us unphilosophical to see whether as a matter of fact the behaviour of sun-spots has any reference to planetary positions. There likewise appears to be this advantage in establishing a connexion of any kind between the behaviour of sun-spots and the positions of some one prominent planet, that we at once expect a similar result in the case of another planet of nearly equal prominence, and are thus led to use our idea as a working hypothesis.

3. We have now a larger number of observations at our disposal than we

had in 1866. We had then only the groups observed by Carrington, the positions and areas of all of which we had accurately measured. We have now in addition five years of the Kew observations, for each group of which the positions and areas have been recorded by us in our previous communications to this Society. We have thus altogether observations extending from the beginning of 1854 to the end of 1860, forming the series of Carrington; and observations extending from the beginning of 1862 to the end of 1866, forming the Kew series, as far as this is yet reduced. We have, in fact, altogether a nearly continuous series, beginning a year or two before one minimum, and extending to the next, and thus embracing rather more than a whole period.

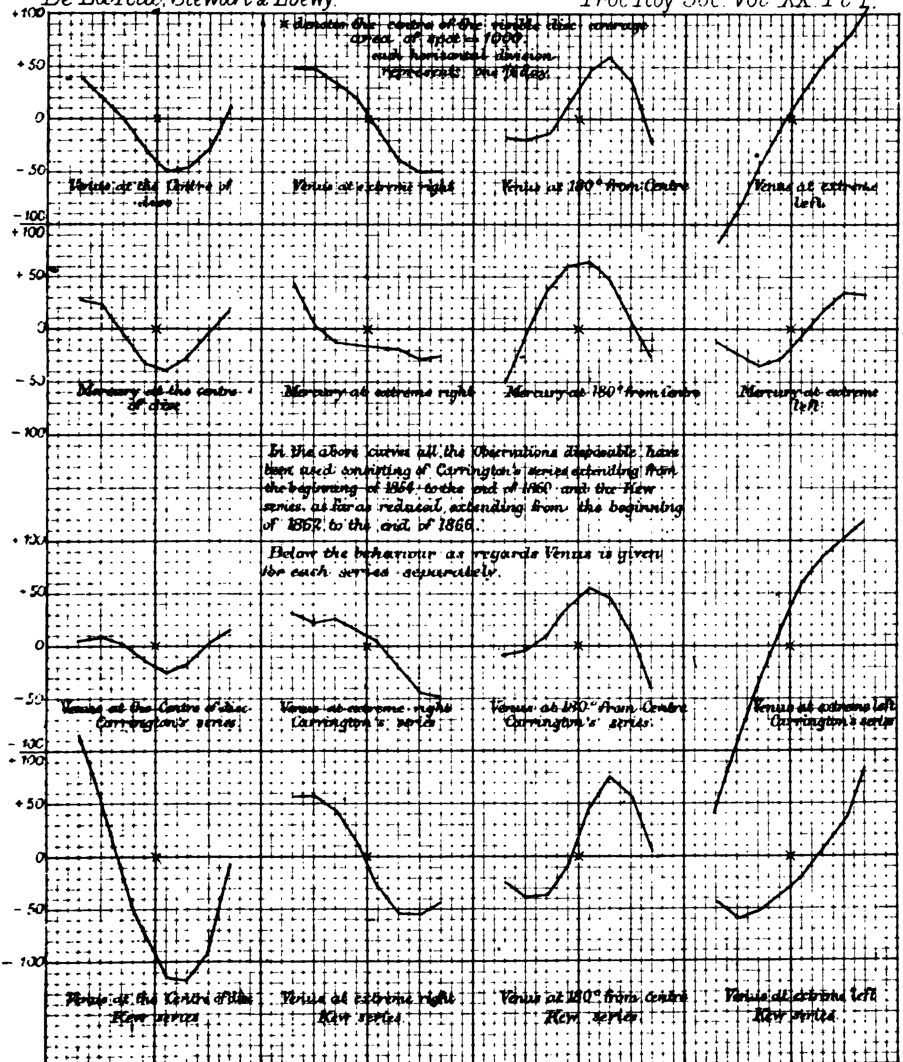
We propose in the following pages to discuss the behaviour with regard to size of the various groups of these two series, as each group passes from left to right across the sun's visible disk. Unfortunately for this purpose, a large number of groups has to be rejected; for, on account of bad weather, we have frequent blank days, during which the sun cannot be seen, and on this account we cannot tell with sufficient accuracy the behaviour of many groups as they pass across the disk. In our catalogue of sun-spot behaviour we have only retained those groups for which, making the times abscissæ, and the areas ordinates, we had sufficiently frequent observations to enable us to construct a reasonably accurate curve exhibiting the area of the group for each point of its passage across the disk. From these curves a Table was then formed denoting the probable area of each non-rejected group at the following heliographic longitudes (that of the visible centre of the disk being reckoned as *zero*):—

$$-63^\circ-49^\circ-35^\circ-21^\circ-7^\circ+7^\circ+21^\circ+35^\circ+49^\circ+63^\circ;$$

in fact giving the area of the group for the ten central days of its progress, and rejecting those observations that were too near the sun's border on either side, on account of the uncertainty of measurement of such observations. We have succeeded in tabulating in this manner 421 groups of Carrington's series, and 373 groups of the Kew series up to the end of 1866, in all 794 groups. In this catalogue the area is that of the whole spot, including umbra and penumbra; and in measuring these areas a correction for foreshortening has always been made, as described in a paper which we presented to this Society, and which constitutes the first series of our researches. These areas are expressed in millionths of the sun's visible hemisphere.

4. When we began this present investigation into the behaviour of spots, we soon found reason to conclude that in the case of sun-spots the usual formula for foreshortening is not strictly correct. Perhaps if a sun-spot were strictly a surface-phenomenon, the usual formula might be correct, though even that is doubtful; for the earth as a planet may not impossibly affect the behaviour of all spots as they cross the disk, so as to render the formula somewhat inexact. However this may be, a spot is probably always surrounded more or less by faculous matter, forming in many cases

De La Rue, Stewart & Loewy. Proc Roy Soc. Vol XX. Pl 1.



W. West & Co Lith.

a sort of cylindrical wall round the spot. Now the effect of such a wall would be to allow the whole spot to be seen when at or near the centre of the disk, but to hide part of the spot as it approached the border on either side. A spot thus affected would therefore appear to be more diminished by foreshortening than the usual formula would indicate; and we should therefore expect, if this were the case, that, on the whole, and after making the usual allowance for foreshortening, spots would nevertheless be found deficient in area near the borders as compared with their area at the centre of the disk. As a matter of fact we have something of this kind, as will be seen from the following Table, in which we have used the whole body of spots forming the catalogue to which we have made allusion. In this Table the first column denotes the heliocentric longitude from the centre of the disk reckoned as zero; the second denotes the united areas at the various longitudes of all those groups from both series, the behaviour of which we have been able to obtain with accuracy; while the third column exhibits the residual factor for foreshortening, which will bring the areas of the second column into equality with each other.

TABLE I.

Longitude observed.	United areas of all groups at longitude of column 1.	Residual factor for foreshortening necessary to equalize the areas of column 2.
-63°	147,508	1·229
−49	156,758	1·156
−35	168,697	1·075
−21	176,417	1·028
− 7	178,990	1·013
+ 7	181,336	1·000
+21	178,638	1·015
+35	175,747	1·032
+49	171,140	1·059
+63	162,541	1·115

5. From the above Table it appears that the average behaviour of spots, as far as can be judged from the information at present attainable, is not quite symmetrical as regards the centre of the disk. Without attempting at present to enter into an explanation of this remarkable phenomenon, we may point to it as a confirmation of our view previously stated, that most spots are accompanied by a wall-shaped surrounding of facula. Observations show that on the whole the life-history of the facula begins and ends earlier than that of the spot which it surrounds, and that throughout a gradual subsidence or dissipation of this elevated mural appendage seems to be taking place. But such a diminution of the wall discloses more of the spot itself; and hence the spot-areas measured in the eastern half of the hemisphere

might be presumed, *cæteris paribus*, to be smaller than those observed in the western half, a fact strikingly demonstrated by the above Table.

Our present object, however, is not to account for the average behaviour of spots, but rather to investigate the causes or concomitants of a departure from this average behaviour. We have therefore in all cases made use of the factors given in the above Table as those which, judging by the average behaviour, tend to equalize the areas that pass the various longitudes. We have called this *earth-correction*, and have limited our discussion to any well-marked behaviour that remains after the earth-correction has been applied.

6. Let us now divide the whole mass of observations into four portions, depending upon the position of the planet Venus with reference to the Earth or point of view. *First*, let us take each occasion on which the planet is in the same heliographic longitude as the Earth, that is to say, when the Earth and Venus are nearly in a line on the same side of the sun.

Let us use five months' observations for each such occasion, extending equally on both sides of it; thus, for instance, if the planet Venus and the Earth had the same heliocentric longitude on September 30, 1855, we should make use of sun-spots from the middle of July to the middle of December of that year as likely to represent any behaviour that might be due to this particular position of Venus. Let us do the same for all similar occasions, and finally add all the spots thus selected together. We have thus obtained a mass of observations which may be supposed to represent any behaviour due to this position of the planet Venus with reference to the Earth or point of view.

Secondly, let us now take each occasion on which Venus is at the same longitude as the extreme right of the visible disk, that is to say, 90° before the Earth, and do the same as we did in the previous instance, using five months' observations for each occasion. We shall thus, as before, obtain a mass of observations which may be supposed to represent the behaviour due to a position of Venus 90° before the Earth. *Thirdly*, let us obtain in a similar manner a mass of observations representing the behaviour of sun-spots for a position of Venus 180° before the Earth, Venus and the Earth being now at exactly opposite sides of the sun; and *fourthly*, let us finally obtain, in a similar manner, those observations representing the behaviour of sun-spots when Venus is 270° before the Earth, being now of the same heliocentric longitude as the extreme left of the visible disk.

These four series of five months each will in fact split up the whole body of observations into four equal parts, the synodical revolution of Venus being nearly twenty months. The following Table exhibits these series after the earth-correction has been applied to each; it also represents each series reduced so as to exhibit its characteristic behaviour for an average size of spot=1000.

TABLE II.

Longitude.	Sum of areas corrected for earth-effect.							
	(A) Venus = Earth + 0°.		(B) Venus = Earth + 90°.		(C) Venus = Earth + 180°.		(D) Venus = Earth + 270°.	
		1000		1000		1000		1000
−63°	48905	+54	60573	+56	44031	−16	27776	−152
−49	48385	+42	59869	+43	44075	−15	28881	−118
−35	47508	+23	60210	+49	43606	−25	30023	− 84
−21	46203	− 4	59847	+43	43974	−17	31331	− 44
− 7	45026	−30	58493	+20	45084	+ 7	32711	− 1
+ 7	43603	−61	56496	−15	47446	+61	33791	+ 31
+21	44134	−49	54867	−44	47768	+68	34547	+ 55
+35	45306	−25	54184	−55	46821	+47	35068	+ 71
+49	46476	+ 1	54782	−46	43693	−23	36285	+107
+63	48742	+49	54473	−51	40875	−87	37143	+135
	464288	10000	573794	10000	447373	10000	327556	10000

7. We may do the same for the planet Mercury as we have done for Venus, that is to say, we may split up the whole body of observations into four parts, representing the behaviour of sun-spots when Mercury is in the same four positions with respect to the Earth as those which are given for Venus in the above Table. Only in this case we must bear in mind that, owing to the eccentricity of Mercury's orbit, this planet will sometimes take a longer, and sometimes a shorter time to go from one configuration to another. Thus, for instance, we have

Mercury = Earth + 0° on March 24, 1854;
Mercury = Earth + 90° on May 6, 1854;
and Mercury = Earth + 180° on May 29, 1854.

We should therefore take the observations between April 15, 1854, and May 18, 1854, as representing the behaviour of sun-spots due to a position of Mercury 90° before the Earth, and so on for other cases. The following Table has been constructed on this principle, and it may be regarded as exhibiting for Mercury precisely what the above Table exhibited for Venus:—

TABLE III.

ngi- de.	Sum of areas corrected for earth-effect.							
	(A) Mercury = Earth + 0°.		(B) Mercury = Earth + 90°.		(C) Mercury = Earth + 180°.		(D) Mercury = Earth + 270°	
		1000		1000		1000		1000
63°	45298	+22	45555	+85	39034	−84	50409	+ 0
49	45492	+26	44183	+52	40288	−54	49868	−10
35	45978	+36	41723	− 7	42303	− 8	48996	−28
21	43870	−11	41398	−14	44554	+46	48453	−39
7	42568	−40	41386	−15	45266	+62	48817	−31
7	42384	−44	41096	−21	45502	+68	49844	−11
21	42885	−33	41460	−13	44817	+52	51341	+18
35	44270	− 2	40649	−31	42740	+ 3	53000	+51
49	45780	+32	40337	−39	41478	−27	51772	+27
63	44922	+14	42157	+ 3	40122	−58	51562	+23
	443447	10000	419944	10000	426104	10000	504062	10000

8. The following is a Table constructed on a precisely similar principle with reference to the planet Jupiter :—

TABLE IV.

Longitude.	Sum of areas corrected for earth-effect.							
	(A) Jupiter = Earth + 0°.		(B) Jupiter = Earth + 90°.		(C) Jupiter = Earth + 180°.		(D) Jupiter = Earth + 270°	
		1000		1000		1000		1000
−63°	29348	− 34	35369	−20	48871	−25	42794	+39
−49	28665	− 57	35256	−24	50118	− 1	43163	+48
−35	28836	− 51	35176	−25	51432	+26	40747	−11
−21	28623	− 57	34962	−32	51029	+18	41318	+ 3
− 7	28779	− 53	35739	− 9	51116	+20	40500	−17
+ 7	30321	− 1	36494	+11	50560	+ 9	40599	−15
+21	31309	+ 31	37264	+32	50266	+ 3	40979	− 5
+35	31488	+ 36	36935	+24	50489	+ 7	41579	+ 9
+49	32400	+ 67	36584	+13	49558	−11	40376	− 7
+63	34017	+119	37147	+30	47792	−46	39373	−44
	303786	10000	360926	10000	501231	10000	411928	10000

9. If we now examine the two Tables for the planets Venus and Mercury, we shall find in them indications of a behaviour of sun-spots appearing to have reference to the positions of these planets, and which seems to be of the same nature for both. This behaviour may be characterized as follows: —the average size of a spot would appear to attain its maximum on that side of the sun which is turned away from Venus or from Mercury, and to have its minimum in the neighbourhood of Venus or of Mercury.

10. The apparent behaviour is so decided with regard to Venus, that the whole body of observations will bear to be split up into two parts, namely Carrington's series and the Kew series, in each of which it is distinctly manifest. The following treatment will serve to render this effect more visible to the eye.

In Table II., column (A) (Venus = Earth + 0°), we have ten final numbers denoting the behaviour of a spot of average area = 1000 at ten central longitudes as follows: $+54+42+23-4-30-61-49-25+1+49$.

Let us take the mean of the first and second of these, the mean of the second and third, and so on, and we get the following nine numbers:—

$$+48+32+10-17-45-55-37-12+25.$$

Performing the same operation once more, we obtain the following eight numbers, corresponding to the eight central longitudes :—

$$+40+21-3-31-50-46-25+7.$$

In the following Table we have exhibited the results obtained by this process :—

TABLE V.

Longitude.	Venus (whole series).	Venus (Carrington's series).	Venus (Kew series).	Mercury (whole series).
°	(A) (B) (C) (D)	(A) (B) (C) (D)	(A) (B) (C) (D)	(A) (B) (C) (D)
−49	+40+48−18−118	+ 8+30−10−160	+117+58−27−46	+28+45−50−12
−35	+21+46−20− 82	+ 9+24− 5− 95	+ 47+58−39−59	+21+ 6− 6−26
−21	− 3+39−13− 43	+ 1+24+10− 37	− 16+45−38−52	− 6−12+36−34
− 7	−31+17+15− 3	−12+16+36+ 16	− 74+13− 9−36	−33−16+60−28
+ 7	−50−14+49+ 29	−23+ 2+53+ 58	−113−29+45−20	−40−18+63− 9
+21	−46−40+60+ 53	−15−20+46+ 82	−119−57+77+ 4	−28−20+43+19
+35	−25−50+34+ 76	+ 4−45+13+100	− 91−56+59+36	− 1−28+ 7+36
+49	+ 7−50−22+105	+14−50−40+118	− 9−44+ 1+82	+19−27−27+32

The results of this Table are exhibited graphically in the Plate which accompanies this paper.

11. If we now refer to the Table for Jupiter, we find that we cannot detect the same kind of behaviour that we did in the case of Venus and Mercury. We cannot say that such a behaviour does not exist with reference to this planet; but, if it does, it is to such an extent that the observations at our disposal have not enabled us to detect it.

12. The following evidence from a different point of view goes to confirm the results we have now obtained. We might expect, if there really is a behaviour of sun-spots depending upon the position of Venus, and of the nature herein stated, that the average area of a spot as it passes the central longitude of the disk ought to be greatest when Venus is 180° from the earth, and least when Venus and the Earth are together, and the same ought to hold for Mercury and for Jupiter, if these planets have any influence. Taking the mean of the four central areas as giving the best value of the area of a spot as it passes the centre, we have for Venus the following results:—

Mean of four central areas—

(A)	(B)	(C)	(D)
44741	57426	46068	33095

and the number of groups for these are as follows:—

229	265	150	181

hence the mean area of one group will be—

195	217	307	183

from which we get (A)=195; mean of (B) and (D)=200; (C)=307; that is to say, A is least, and C is greatest.

Doing the same in the case of Mercury, we get

(A)=204; mean of (B) and (D)=217; (C)=246;

and finally, doing the same in the case of Jupiter, we get

(A)=185; mean of (B) and (D)=207; (C)=282:

it thus appears that in all these cases the same order is preserved.

13. We leave it to others to remark upon the nature and strength of the

evidence now deduced as to a connexion of some sort between the behaviour of sun-spots and the positions of the planets Venus and Mercury. We think, however, it must be allowed, that the investigation is one of interest and importance, and we trust that arrangements may be made for the systematic continuance of solar observations in such localities as will ensure to us a daily picture of the sun's disk.

The influence of blank days in diminishing the value of a series of sun-observations is very manifest. We have been able to record the behaviour across the sun's disk of 421 groups of Carrington's series out of a total number of 885 groups, and we have been able to record the same behaviour for 373 out of 544 groups observed at Kew. Thus, out of a total of 1429 groups, we have only been able to record the behaviour of 794. Nor are the records which we have obtained so perfect as we could wish, on account of blank days, which make interpolations necessary. It is therefore of much importance for the future of such researches as the present, that there should be several observing-stations so placed that we may reckon on having at least a daily picture of the sun's disk.

It will be easily seen that such observations are very different from experiments, which may be multiplied *ad libitum*; for in this case Nature gives us in a year or in ten years a certain amount of information, and no more, while it depends upon ourselves to make a good use of the information which she affords.

It is already universally acknowledged that we ought to make the best possible use of the few precious moments of a total eclipse; but such observations must necessarily be incomplete unless they are followed up by the equally important, if more laborious, task of recording the sun's surface from day to day.

III. "The Decomposition of Water by Zinc in conjunction with a more Negative Metal." By J. H. Gladstone, Ph.D., F.R.S., and Alfred Tribe, F.C.S. Received February 8, 1872.

Pure zinc is incapable of decomposing pure water, even at 100° C., but at a considerably higher temperature it is known to combine with its oxygen. Davy exposed pure water for two days to the action of a pile of silver and zinc plates, separated only by pasteboard, without obtaining any hydrogen; Buff, however, has shown that a very minute trace of gas can be formed at the ordinary temperature by a pair of zinc and platinum plates.

During a series of experiments, of which we have already published an instalment, it occurred to us to ascertain whether by bringing the two metals closer together, and so increasing the electrical tension of the liquid, we could effect the same combination of zinc with oxygen at the ordinary temperature which takes place without the second metal at a very high

temperature. Thin sheets of zinc and copper were hammered together and placed in a bottle filled with distilled water. Small bubbles of gas were formed. The experiment, however, was tried in a more perfect form. Some zinc-foil was allowed to remain in a somewhat dilute solution of copper sulphate until its surface was well covered with spongy copper. The metals were thoroughly washed with distilled water, and then they were immersed in a bottle of distilled water with a delivery-tube. Minute bubbles of gas quickly made their appearance, which proved to be hydrogen, and zinc-oxide was formed. Two experiments were made quantitatively, the gas being collected and measured at the end of 24 or 48 hours. The quantity of gas in cubic centimetres is given in the third and fourth columns of the subjoined Table, corrected for temperature and pressure. The mean temperature in the second column is simply the mean of the maximum and minimum during the period. In experiment A, 33·4 grms. of zinc-foil were employed, being 2·6 metres long and 0·05 wide. The coils were kept apart by muslin. In experiment B there was used 1 metre of similar foil crumpled up.

Day.	Mean temp.	Exper. A.	Exper. B.	Day.	Mean temp.	Exper. A.	Exper. B.
	C.	c. c.	c. c.		C.	c. c.	c. c.
1.	12·8°	117·1	49·6	18.	6·7°	20·0	7·6
2.	12·2	93·8	37·5	19, 20.	6·1	17·2 (×2)	5·7 (×2)
3.	11·7	73·8	27·6	21.	4·4	20·0	6·6
4.	11·1	66·2	24·7	22.	5·0	15·3	4·8
5, 6.	10·0	49·3 (×2)	17·5 (×2)	Interval.			
7.	8·9	41·1	14·9	44.	10·0	20·5	5·5
8.	10·5	40·9	15·8	45, 46.	10·5	22·5 (×2)	6·5 (×2)
9.	10·0	40·9	14·8	47.	11·1	22·3	6·5
10.	7·8	33·8	10·3	48.	11·1	24·1	8·1
11.	6·7	28·0	9·4	49.	11·1	20·5	7·4
12, 13.	6·1	21·9 (×2)	7·7 (×2)	Interval.			
14.	6·1	20·1	7·6	82.	10·0	18·0	4·7
15.	7·2	31·1	10·3	83.	10·0	18·9	6·1
16.	10·0	30·0	10·2	84.	10·0	14·0	5·1
17.	8·3	29·4	8·5				

The two experiments have evidently gone on almost *pari passu* for months, the amount of hydrogen evolved gradually diminishing, but showing, at the same time, a certain dependence on the heat of the day.

Under the microscope the bubbles of gas are seen to form, not on the zinc, but among the copper crystals, and sometimes to make their appearance on the glass at some distance off.

From the position of platinum in the electro-chemical series we anticipated that the effect would be still more marked with that metal in a spongy state on the zinc. It was deposited from the tetrachloride, and, of course, thoroughly washed. There was only 0·6 metre of foil, but the following quantities of hydrogen were obtained :—

Day.	Mean temp.	Vol. in cub. centims.
1.	11·7° C.	143·6
2.	11·4	93·6
3, 4.	10·0	38·8 (×2)
5.	8·6	26·0
6.	10·8	21·0
7.	9·4	17·1
8.	7·7	12·3

The first action, therefore, was about five times as great as in the case of the copper, and it diminished more rapidly, doubtless through the zinc becoming more quickly protected by oxide.

Lest it might be contended that the free oxygen, usually present in distilled water, had been the means of starting this action, the experiment was repeated with water as free from oxygen as could be obtained by boiling. One metre of the same zinc-foil, covered with copper, was employed, and the result was nearly as before, 40 cub. centims. of gas being obtained the first day at the mean temperature of 9° C. This arrangement was taken advantage of to examine the effect of a high temperature. Without removing the delivery-tube, the contents of the flask were heated to near 100° C., when 123·5 cub. centims. of hydrogen were given off in ten minutes. The apparatus was allowed to cool, with the mouth of the tube under water, when the production of gas became small again, and after two days it was again heated nearly to the boiling-point, when it gave off 93·4 cub. centims. in ten minutes; after another period of two days it gave 64·1 cub. centims. and after three days more 132·1 cub. centims. in the first thirty minutes, 108·4 in the second thirty minutes, 94·3 in the third, and 89·9 in the fourth.

Iron and lead, under similar circumstances, also decomposed pure water, and the action of magnesium was greatly increased by conjunction with copper. The effect of the more negative metal was the same as would have been produced by an increase of heat.

In a practical point of view this experiment may serve as a ready means of preparing pure hydrogen; in a theoretical point of view, its interest seems to lie in the fact that the dissociation of a binary compound by means of two metals may take place at infinitesimally short distances, when it would not take place where the layer of liquid is enough to offer resistance to the current, and also in the correlation between this force and heat*.

P.S. March 14.—At the suggestion of Prof. Stokes, we tried to ascertain if the well-known influence of points had much to do with the separation of this hydrogen gas. Two thin plates of copper were taken, the one

* Since the above was written we have accidentally heard that Dr. W. Russell has been working in the same direction.

smooth, the other rough with electrolytically-deposited copper; these were separated from thin plates of zinc merely by pieces of muslin, and the metals were folded over at each end and hammered together. Each couple was placed in water, and for some days very minute bubbles of gas formed, but only at the junction of the metals, and about equally in each case.

As might be expected, this zinc in conjunction with copper is capable of decomposing other liquids than water. Chloroform yields readily to its power, and iodide of ethyl, which Prof. Frankland decomposed by zinc only at a great heat, is split up rapidly at the ordinary temperature.

March 21, 1872.

WILLIAM SPOTTISWOODE, M.A., Treasurer and Vice-President, in the Chair.

The following communications were read:—

I. "New Researches on the Phosphorus Bases." By A. W. HOFMANN, LL.D., F.R.S., Professor of Chemistry in the University of Berlin. Received March 6, 1872.

About twelve years have elapsed since I submitted to the Royal Society, partly in conjunction with M. Cahours, a series of papers* on the remarkable group of phosphorus compounds, the existence of which was first pointed out by M. Paul Thénard as far back as 1846. These researches were devoted to the investigation of the *tertiary* and *quartary* derivatives of phosphoretted hydrogen, exclusively accessible by the methods then at our disposal. The study of the *primary* and *secondary* phosphines, the examination of which promised even more noteworthy results than that of the bodies then investigated, still remained to be achieved.

New tasks of life have since that time presented themselves, and I have not been able to devote myself as much to research as in former days. Nevertheless, numerous attempts were made to procure the primary and secondary phosphines, which were clearly indicated by theory and partly even by M. Thenard's early observations. For a long time, however, these experiments proved unsuccessful, and it was only in the course of last summer that I at last discovered an easy method for their production. I may now fairly hope to complete an inquiry, the first part of which the Royal Society have done me the honour of inserting in the 'Philosophical Transactions' †; still some time will be required for surveying a field which appears to expand as one advances in its investigation, and I therefore beg leave to present to the Society the results of my observations in the measure as they are obtained, even before the whole investigation be terminated.

* Proceedings, vol. viii. pp. 500, 523; vol. ix. pp. 287, 290, 487, 651; vol. x. pp. 100, 189, 603, 606, 610, 613, 619; vol. xi. pp. 286, 290.

† Phil. Trans. 1857, p. 575; 1860, pp. 409, 449, 497.

I. Formation of Alcohol Phosphines by means of Phosphonium Iodide.

The starting-point of the new series of researches was a lecture experiment. Wishing to exhibit to my class the decomposition of phosphoretted hydrogen by the spark-current of an induction-coil, I was unable to procure, by any of the methods hitherto described, phosphoretted hydrogen of sufficient purity for this experiment. I was thus led to select a rather unusual substance as a source for phosphoretted hydrogen, viz. the beautiful compound of the latter with hydriodic acid, generally designated as phosphonium iodide. This substance, formerly accessible only with difficulty, may now be easily prepared in any quantity. If a slow stream of water, or better of potash or soda, be allowed, by means of a dropping-tube, to flow into a small vessel containing phosphonium iodide, a regular current of perfectly pure phosphoretted hydrogen is evolved, which may at once be introduced into a eudiometer provided with spark-wires, and be submitted to experiment. With the first spark that passes, a brown cloud of finely divided phosphorus appears in the eudiometer, lining gradually the inside of the tube. After the lapse of five minutes, two volumes of phosphoretted hydrogen have become expanded into three volumes of pure hydrogen gas.

The facility with which phosphonium iodide is thus seen to split up into its constituents, hydriodic acid and phosphoretted hydrogen, led me to think that this body might be made available for the preparation of the compounds I had so long endeavoured to obtain. Two different processes suggested themselves, both aiming at a reproduction of the conditions under which, as I have shown now more than twenty years ago*, the alcohol derivatives of ammonia are readily obtained. For this purpose it was necessary to disengage phosphoretted hydrogen in the presence of an alcohol iodide under pressure. This could be easily accomplished by submitting a mixture of an alcohol iodide and phosphonium iodide in sealed vessels to the action of an agent (such as water or a metallic oxide, zinc oxide for instance), slowly liberating the phosphoretted hydrogen from the iodide. But this process appeared to be capable of a further simplification. Instead of withdrawing the hydriodic acid in the phosphonium iodide from the reaction by means of water or a metallic oxide, it seemed worth trying to utilize this acid in the production of the very alcohol iodide to be acted upon by phosphoretted hydrogen; and the question arose whether this result might not be readily attained by decomposing under appropriate circumstances the phosphonium iodide by the alcohols themselves.

These several anticipations have been fully confirmed by experiment. Both processes yield alcoholic phosphines easily and copiously; and, remarkably enough, whilst the former (action of alcohol iodide upon phosphonium iodide) gives rise to the formation of exclusively the primary and secondary phosphines I had so long endeavoured to produce, the secondary process (action of the alcohols upon phosphonium iodide) furnishes only the

* Phil. Trans. 1850, p. 93; 1851, p. 357.

tertiary phosphines and the quartary phosphonium compounds previously known, but which may be much more easily and plentifully obtained by the new method. Phosphonium iodide has thus become a general agent for the production of the alcohol derivatives of phosphoretted hydrogen.

The formation of the several groups of phosphines by means of phosphonium iodide is represented by the following equations, in which the reaction is assumed to be accomplished in the methyl series:—

Primary Phosphines.

$$2CH_3I+2(H_3P.HI)+ZnO=2[(CH_3)H_2P.HI]+ZnI_2+H_2O.$$

Secondary Phosphines.

$$2CH_3I+H_3P.HI+ZnO=(CH_3)_2HP.HI+ZnI_2+H_2O.$$

Tertiary Phosphines.

$$3(CH_3.HO)+H_3P.HI=(CH_3)_3P.HI+3H_2O.$$

Quartary Phosphonium Compounds.

$$4(CH_3.HO)+H_3P.HI=(CH_3)_4PI+4H_2O.$$

II. Primary and Secondary Methylic Derivatives of Phosphoretted Hydrogen.

Owing to the superior interest attached to the monocarbon compounds, I was induced, in the first place, to test the new reactions in the methyl series.

Phosphonium iodide, methylic iodide, and zinc oxide act upon one another with remarkable facility. Two molecules of each of the iodides and one molecule of zinc oxide were found to be appropriate proportions; the ordinary zinc-white of commerce may be employed.

Since it was desirable to procure at once the new compounds in sufficient quantity for a thorough examination, the digestion-tubes received considerable charges. If these tubes have a capacity of from 120 to 150 cubic centims., 70 or 80 grms. of the agents involved in the process may be digested without danger. It is not, however, desirable to pass these limits. The compound first introduced is the phosphonium iodide, then follows the zinc oxide, which is compressed into a solid layer in order to prevent the methyl iodide, lastly poured in, rapidly to come in contact with the phosphonium compound. In the presence of zinc oxide the two iodides act upon each other even at the common temperature; and without the precaution just indicated, it would be difficult to draw out and seal the tubes. Before being heated, the tubes must be strongly agitated in order to produce a thorough mixture of the three substances. As regards the digestion, I have been often satisfied to work at the temperature of boiling water; after six or eight hours' exposure in a water-bath, the transformation is generally complete. If the tubes be heated to 150° in an air-bath, not more than four hours are required. On cooling, the digestion-tubes are found to contain a white crystalline solid; they invariably hold a good deal of compressed gas, so that some precaution is necessary in opening them before the blowpipe. The gases generally issue with a loud report and a long

flame; explosions, however, are but rarely met with. The proportions of the charge are so selected, that supposing the reaction to be complete, no other products except methylphosphonium iodide and zinc iodide should be produced. But the presence of some unchanged phosphonium iodide together with the escape of its constituents, more especially of phosphoretted hydrogen, prove at once that compounds more highly methylated must be formed. Experiment shows, however, that monomethyl- and dimethylphosphine exclusively are generated. These two substances are easily separated from the crude product of the reaction, which, by means of a bent wire, may be removed in one piece from the tube.

The separation of monomethylphosphine and dimethylphosphine and the preparation of the two bodies in a state of purity are based upon the observation that the salts of the former base are easily and thoroughly decomposed by water, whilst those of dimethylphosphine, more especially in the presence of free acid, may be considerably diluted without undergoing any change, but are immediately decomposed by addition of a fixed alkali. The crude product of the reaction is therefore consecutively treated with water and strong alkali, the former disengaging the methylphosphine, which, being gaseous, is collected in concentrated hydriodic acid, the latter liberating the dimethylated phosphine, which, being liquid at the common temperature, may be readily condensed by an appropriate cooler. The two bodies being powerfully acted upon by the oxygen of the air, the whole process is to be conducted in an apparatus filled with hydrogen.

Methylphosphine.

$$CH_5P = \left.\begin{matrix} CH_3 \\ H \\ H \end{matrix}\right\} P.$$

Methylphosphine is a colourless transparent gas of a most overwhelming odour.

Both by cooling and by pressure the gas may be condensed into a colourless liquid floating upon water, and boiling from platinum at $-14°$ under a pressure of 0·7585 metre. The experiment was made with from 60 to 70 grms. of methylphosphine condensed in one operation, the boiling-point remaining constant till the last drop had distilled. In studying the behaviour of the new gas under the influence of increased pressure, I have availed myself of the beautiful compression-apparatus constructed by Gustav Magnus. At 0° $1\frac{3}{4}$ atmosphere was sufficient to start the liquefaction; under a pressure of $2\frac{1}{2}$ atmospheres the gas was perfectly liquid, its purity being thus satisfactorily established. At 10° liquefaction commenced, and was completed under a pressure of $2\frac{1}{4}$ and 4 atmospheres respectively; at 20°, lastly, under a pressure of 4 and $4\frac{1}{2}$ atmospheres.

The volume-weight of methylphosphine gas was easily determined by allowing a small tube with a weighed quantity of the iodhydrate to rise into a graduated cylinder filled with mercury and inverted over the mercurial trough and subsequently introducing some concentrated solution of

soda which decomposed the salt. By observing the volume of the gas disengaged, all the data for fixing the volume-weight were given. In this manner the number 24·35 was found, the theoretical value being 24.

Methylphosphine is nearly insoluble in water; if the water contain air, part of the gas disappears, but only in consequence of oxidation, clearly indicated by the formation of white clouds. If the gas stand over water into which air can penetrate from without, the gas after some time is perfectly absorbed. Methylphosphine gas is rather soluble in alcohol even at the ordinary temperature, but more especially at temperatures approaching its point of liquefaction; at 0° one volume of alcohol of 95 per cent. absorbs not less than twenty volumes. At the ordinary temperature ether dissolves but little; the solvent power increases, however, rapidly by cooling the liquid. At 0° one volume of ether is capable of dissolving seventy volumes of methylphosphine.

The methylated phosphorus base attracts oxygen with great avidity; on mixing the gas with air, white clouds are formed at once; but detonation does not take place at the common temperature. If methylphosphine be required of absolute purity, the gas must be allowed to escape from the apparatus until a small quantity collected over mercury remains perfectly transparent. The nature of the more immediate products of oxidation remains to be investigated. When gently heated in contact with air, methylphosphine takes fire. A glowing match, and even a glass rod just heated to scarcely visible redness, at once inflame the gas. In contact with chlorine and bromine or nitric vapours, it burns with a brilliant flame.

By its union with acids, methylphosphine gives rise to a series of well-defined salts distinguished by the remarkable property of being decomposed by water. On this property is based the preparation of the body in a state of purity. The salts bleach vegetal colours like chlorine.

Of the salts, two only have as yet been more closely examined, the chlorhydrate and the iodhydrate.

Chlorhydrate.—If a current of methylphosphine gas be conducted into strong fuming hydrochloric acid, it is perfectly absorbed; no crystals, however, are separated; but on mixing the two gases, they are at once condensed to beautiful, well-formed, four-sided plates. In certain reactions with organic chlorides, which are accomplished in ethereal solution, and which I hope to describe more minutely to the Society hereafter, the salt is deposited in splendid large four-sided tables, often having a centimetre in diameter. The chlorhydrate is so volatile that it passes over even with the vapour of ether. Analysis was performed by the method often adopted for demonstrating the composition of sal-ammoniac in lectures. By allowing equal volumes of methylphosphine and hydrochloric gases to meet over mercury, both entirely disappear with formation of a crystalline deposit. Hence the salt contains

$$CH_6PCl = (CH_3)H_2P, HCl.$$

The solution of the chlorhydrate in concentrated hydrochloric acid furnishes with platinum perchloride a beautifully crystallized orange-red platinum salt.

Iodhydrate.—Of all the salts of the base, this is the one most readily obtained; it separates in bulky crystals when a current of methylphosphine is passed into the most concentrated iodhydric acid. If a solution of the gas in somewhat less concentrated iodhydric acid be mixed with ether, the whole liquid solidifies to a mass of iridescent plates. By washing with ether, pressing, and sublimation in a current of dry hydrogen, the salt may be readily obtained in a state of purity. Analysis led to the formula

$$CH_6 PI=(CH_3) H_2 P, HI.$$

The *sulphate* I have not yet seen in the solid state; it is, however, readily formed by bringing the phosphorus base in contact with concentrated sulphuric acid. The gas is absorbed without the acid colouring. On addition of water, methylphosphine is again liberated. The *sulphite* is a white amorphous mass which is formed when methylphosphine and sulphurous acid gases are collected together over mercury.

With carbonic acid and sulphuretted hydrogen methylphosphine gas may be mixed without any condensation taking place. Sulphur, carbon bisulphide, and chlorocarbonic ether, when placed in contact with methylphosphine, give rise to the formation of new compounds, which will be the subject of a special communication.

Among the products M. P. Thenard* describes, in the short notice of his researches on the action of methylic chloride upon calcium phosphide, is an oily compound boiling at 250° containing $C_2 H_6 P$ (perhaps phosphoric kakodyl), which, when treated with water, splits into an acid and a gaseous body. The latter, to which M. P. Thenard assigns the formula $C H_5 P$, is said to combine with either one or two volumes of chlorhydric gas. It can scarcely be doubted that the phosphoretted gas discovered by M. Thenard is identical with the body the chemical history of which I have endeavoured to sketch in this paper. There are certainly still some discrepancies to be explained, such as the observation just alluded to, that M. Thenard's phosphorus body combined with chlorhydric acid in two proportions.

The somewhat complex reaction which gives rise to the formation of methylphosphine, when generated by means of methylic chloride and calcium phosphide, and indeed the difficulties attending this process, which appear to have prevented M. Thenard from pursuing his researches, have hitherto deterred chemists from entering more thoroughly into the investigation of this remarkable compound. By the method described in this paper, methylphosphine may be readily and abundantly obtained in a state of perfect purity, so that its further examination will present no difficulty.

* Compt. Rend. vol. xxv. p. 892.

Dimethylphosphine.

$$C_2H_7P = \left.\begin{matrix} CH_3 \\ CH_3 \\ H \end{matrix}\right\} P.$$

The method of preparing dimethylphosphine has been already stated. It is a transparent colourless liquid, which, when protected from the atmosphere, may be preserved without change. It is lighter than water, in which it is insoluble; alcohol and ether, on the other hand, dissolve it with facility. Its boiling-point is 25°.

Dimethylphosphine is remarkable for the avidity with which it attracts oxygen, and which is infinitely superior to that of the monomethylated base. In contact with the air, it instantaneously takes fire and burns with a powerfully luminous phosphorus flame. If the hydrogen atmosphere in which it is prepared contain only traces of air, the presence of which is at once indicated by the formation of white fumes, violent and by no means dangerless explosions are occasionally experienced in working with this compound, even if great care be employed.

Dimethylphosphine easily unites with acids; all the salts are exceedingly soluble. The solution of the chlorhydrate furnishes with platinum perchloride a fine crystalline salt. The base also unites with sulphur and carbon bisulphide. The compounds thus formed are not yet investigated; but it may even now be remarked that they essentially differ from those produced by trimethylphosphine; more especially in the deportment of the two bases with carbon bisulphide, a marked discrepancy is observed. Dimethylphosphine in this case produces no crystalline compound similar to those which are formed by the tertiary phosphines; so that the absence of trimethylphosphine among the products of the action of methyl iodide on phosphonium iodide may be readily demonstrated.

The inflammability and the low boiling-point of dimethylphosphine render it difficult to work with this body except in the midst of winter. The examination of its numerous products of decomposition, which promises to be fruitful in results, is therefore as yet but little advanced. Hitherto I have studied somewhat more in detail only the products of oxidation of the methyl bases, which I beg leave to describe to the Society in a special paper.

III. Products of Oxidation of the Methylated Phosphines.

When determining phosphorus in several substances which in the course of the new researches on the phosphines had to be examined, it was found that these bodies, and especially the members of the methyl series, resist with remarkable energy the action of even the most powerful oxidizing agents.

If the phosphoretted bodies were heated according to the method of Carius, it happened sometimes, especially when the digestion in sealed tubes was conducted according to the earlier directions with nitric acid not per-

fectly concentrated and at moderate temperatures, that the liquid taken from the tube and treated in an appropriate manner with magnesian salts gave no precipitate whatever. If, on the other hand, strongest fuming nitric acid be employed at high temperatures, phosphoric acid is certainly formed; but only when the reaction takes place at the very extreme temperatures recommended by Carius* in his more recent paper, is the whole quantity of phosphorus made precipitable by magnesian salts.

It appeared of interest to submit to a closer examination the products of oxidation that are formed by the action of nitric acid on the primary and secondary phosphines at moderate temperatures, more especially since some of these bodies have probably passed already through the hands of M. Paul Thenard when engaged in his remarkable but, unfortunately, unfinished researches on this subject. Experiment showed that nitric acid gives rise to new acids of great stability and comparatively little volatility, and thus a very simple method of estimating phosphorus in this whole group of compounds at once suggested itself. It was only necessary to dissolve the substance under examination, according to circumstances, either in strong hydrochloric or in nitric acid, to mix the liquid slowly with fuming nitric acid, to treat the solution after evaporation with excess of sodium carbonate, and finally to dry and fuse the mass in a porcelain crucible; in this way the oxidation of the phosphorus is easily and perfectly accomplished. All the phosphorus estimations necessary in these researches have been performed in this manner.

Experiments in the Methyl Series.

Monomethylphosphinic acid.—In order to obtain the product of oxidation of methylphosphine in appropriate quantity, a slow stream of the gas was directed into fuming nitric acid. It would have been unnecessary in this case to employ the phosphine gas in a state of purity; it was sufficient to make use of the methylphosphine as it is delivered from the crude product of the action of methyl iodide on phosphonium iodide and zinc oxide by treatment with water. This gas always contains small quantities of phosphoretted hydrogen, which ignite in contact with the fuming acid, and easily give rise to small explosions. As the methylphosphine becomes purer, these become more seldom and at last quite cease. Invariably, however, in consequence of these detonations, more or less phosphoric acid is found amongst the products of oxidation.

To get rid of the nitric acid, the solution is several times evaporated to dryness on a water-bath, the residue dissolved in water, and the liquid boiled with lead oxide for the purpose of separating the phosphoric acid; a lead-salt is thus formed which is insoluble in water but dissolves in acetic acid, leaving an appreciable residue of lead phosphate. This solution is freed from lead by means of sulphuretted hydrogen, and from acetic acid by repeatedly evaporating, when the new body remains as an oily liquid which, on cooling, solidifies to a crystalline mass resembling spermaceti. The

* D. Chem. Ges. Ber. 1870, p. 697.

crystals thus produced, which could not be obtained in distinct forms, are hygroscopic but not deliquescent. They readily dissolve in water, the solution restoring litmus red to blue, and possessing, moreover, an agreeable sour taste. They are also soluble in alcohol, less so in ether; the alcoholic solution, however, is not precipitated by ether. The stability of this body is quite remarkable; that it is not altered by fuming nitric acid is evident from the manner of its preparation; but even by repeated evaporation with *aqua regia* not the slightest change is produced.

The new compound melts at 105° C.; it is volatilized, at least for the greater part, without decomposition; when strongly heated, it evolves an inflammable gas, a small residue of phosphoric acid mixed with more or less carbon remaining behind.

Analysis showed that methylphosphine, by treatment with nitric acid, fixes three atoms of oxygen, the composition of the new body being represented by the formula

$$CH_5\,PO_3=(CH_3)\,H_2\,PO_3.$$

The new substance is a well-marked acid; I will designate it by the name placed at the head of the paragraph, i. e. *monomethylphosphinic acid*, or more briefly *methylphosphinic acid.* It forms two series of salts, the composition of which is represented by the formulæ

$$(CH_3)\,HM\,PO_3 \text{ and } (CH_3)\,M_2\,PO_3.$$

The primary (acid) salts are produced by the action of metallic carbonates, or by incomplete saturation with the free bases. For the preparation of the secondary (neutral) salts, the acid must be completely saturated by the free bases; they can, however, also be obtained by means of carbonates, if the latter, as is the case with the alkaline carbonates, are capable of fixing the carbonic acid which is liberated.

The primary methylphosphinates have an acid, the secondary salts an alkaline reaction; these latter are soluble and only little inclined to crystallize. The ammonium-salts lose ammonia by evaporation, leaving the acid behind. Amongst the metallic salts, especially the primary ones, many are insoluble, or soluble only with difficulty.

Silver Methylphosphinate.—If the acid be saturated with silver oxide and the solution evaporated to the consistence of a syrup, the primary salt separates from the solution in beautiful white needles, which, in contact with water and even with alcohol, are readily converted into the secondary salt with separation of the free acid.

The salt obtained by means of silver oxide and purified by washing with water gave numbers which showed that it consisted of the nearly pure secondary compound. In order to obtain this salt quite pure, the solution of the acid was accurately neutralized by ammonia and precipitated by silver nitrate. It is a white amorphous precipitate, nearly insoluble in water, having the composition

$$CH_3\,Ag_2\,PO_3.$$

Lead Methylphosphinate.—If an aqueous solution of methylphosphinic

acid be boiled with an insufficient quantity of lead oxide, the primary and secondary salt are formed at the same time; the latter as a white amorphous heavy powder which collects at the bottom of the hot liquid, the former crystallizing from the liquid as it cools in beautiful, long, lustrous, white needles. On washing with water, the salt is decomposed like the silver compound, gradually forming the secondary salt and free acid; indeed analysis of the washed crystals gave numbers lying between those required for the primary and secondary salts. The secondary salt may be obtained, however, in a state of purity if the barium-salt presently to be described be decomposed by lead acetate. It is a precipitate almost insoluble in water, but soluble in acetic acid; its composition is

$$(CH_3)\,Pb\,PO_3, \text{ or perhaps more correctly } (CH_3)_2\,Pb_2\,P_2\,O_6.$$

Barium Methylphosphinate.—This is obtained by boiling the acid with barium carbonate, evaporating the solution to the consistence of a syrup, and precipitating by alcohol. It is a white powder consisting of microscopic needles easily soluble in water. The aqueons solution, even on slow evaporation, yields no crystals, but dries to a gummy mass. Analysis showed the salt to be the primary compound

$$C_2\,H_8\,Ba\,P_2\,O_6 = (CH_3)_2\,H_2\,Ba\,P_2\,O_6.$$

Methylphosphinic acid has the same composition as methylphosphorous acid, but it is only necessary to compare the above statements with what is known respecting the latter compound in order to see that they constitute two absolutely different bodies. Methylphosphorous acid is an uncrystallizable ephemeral compound, being decomposed, even at a gentle heat, into phosphorous acid and methyl alcohol; it cannot possibly be mistaken for the extremely stable derivative of methylphosphine, which may even be distilled without undergoing any decomposition.

Dimethylphosphinic Acid.—By this name I designate an acid which is produced by the action of nitric acid on the secondary methyl base. In preparing this body it is convenient to start from the chlorhydrate of dimethylphosphine. A solution of this salt is readily obtained when the crude product of the action of phosphonium iodide on methyl iodide, after the expulsion of methylphosphine by water, is distilled with alkali, the dimethylphosphine thus disengaged being passed into hydrochloric acid. If this solution be mixed with fuming nitric acid, a powerful reaction ensues, causing the liquid to boil, and fumes of nitrous acid copiously to be evolved. In order to free the strongly acid solution from nitric acid, it is repeatedly evaporated with hydrochloric acid, and then heated for some time on the water-bath to expel as far as possible also this latter acid. To get rid of the last traces of hydrochloric acid, the liquid is saturated with silver oxide and the solution, filtered from the silver chloride, precipitated by sulphuretted hydrogen. The solution agein evaporated on the water-bath gradually solidifies to a white paraffine-like mass of crystals, which, in contact with the air, are apt to become slightly brown; they are very soluble in

water, alcohol, and ether. These solutions have a decidedly acid reaction. The crystals melt at 76°; at a higher temperature they are volatilized without decomposition; indeed the distilled product shows the same melting-point as the undistilled acid. Dimethylphosphinic acid is less suitable for analysis than the mono-compound. It appeared sufficient to fix its composition by the examination of the silver-salt. This analysis proved the acid to be represented by the formula

$$C_2H_7PO_2=(CH_3)_2HPO_2.$$

It is thus seen that dimethylphosphine, when treated with nitric acid, fixes not three atoms of oxygen, like the monomethylated base, but only two. Dimethylphosphinic acid forms only one series of salts, having the general formula

$$C_2H_6MPO_2=(CH_3)_2MPO_2.$$

Silver Dimethylphosphinate.—This salt is obtained by saturating the crude acid still retaining hydrochloric acid with silver oxide, evaporating the filtered solution, and precipitating the concentrated liquid with absolute alcohol. The salt presents itself in the form of fine felted white needles extremely soluble in water, but very slightly so in ether and absolute alcohol. Its composition is represented by the formula

$$C_2H_6AgPO_2=(CH_3)_2AgPO_2.$$

Barium Dimethylphosphinate.—By boiling the solution of the pure acid with an excess of precipitated barium carbonate, a neutral liquid is obtained, which, when evaporated on the water-bath, dries up to a transparent varnish. In contact with a hard body this clear varnish becomes opaque, and shows inclination to crystallize. It is soluble also in alcohol.

Lead Dimethylphosphinate.—The preparation is conducted in the same manner as that of the barium salt, only that oxide instead of carbonate is employed. In its properties it resembles the barium salt: the varnish dissolves in a small quantity of water; the solution becomes turbid, however, by the addition of a larger quantity. Several lead determinations showed excess of lead above that contained in the normal dimethylphosphinate, a result which cannot surprise if the tendency of lead to form basic salts and the absence of all properties warranting the purity of the compound be considered.

It is of some interest to compare the behaviour of phosphoretted hydrogen under the influence of powerful oxidizing agents with that of its several methylated substitution-products. Phosphoretted hydrogen, on treatment with concentrated nitric acid, fixes four atoms of oxygen, becoming converted into *tribasic* orthophosphoric acid; methylphosphine similarly treated combines with only three atoms of oxygen, forming *dibasic* methylphosphinic acid. Under the same conditions dimethylphosphine appropriates not more than two atoms, giving rise to *monobasic* dimethylphosphinic acid. Lastly, trimethylphosphine fixes but one atom of oxygen, the product of the reaction being trimethylphosphine oxide, observed some years ago by

Cahours and myself; this body is *no longer capable of forming saline compounds.* We thus arrive at the following series:—

$$\begin{array}{rcl} H_3P+O_4 & = & H_3PO_4. \\ (CH_3)H_2P+O_3 & = & (CH_3)H_2PO_3. \\ (CH_3)_2HP+O_2 & = & (CH_3)_2HPO_2. \\ (CH_3)_3P+O & = & (CH_3)_3PO. \end{array}$$

By examining this series it is observed at once that all the bodies here described are derived from phosphoric acid, the oxides generated from the methylated phosphines being orthophosphoric acid, the hydroxylic groups of which are successively replaced by methyl.

Orthophosphoric acid .... $\left.\begin{array}{l} HO \\ HO \\ HO \end{array}\right\} PO.$

Methylphosphinic acid .... $\left.\begin{array}{l} CH_3 \\ HO \\ HO \end{array}\right\} PO.$

Dimethylphosphinic acid ...... $\left.\begin{array}{l} CH_3 \\ CH_3 \\ HO \end{array}\right\} PO.$

Trimethylphosphine oxide ...... $\left.\begin{array}{l} CH_3 \\ CH_3 \\ CH_3 \end{array}\right\} PO.$

This symmetrically constituted series does not stand alone; indeed orthoarsenic acid forms the starting-point of a perfectly analogous group of compounds, which are obtained, however, by processes different from those yielding the phosphorus bodies. The substance corresponding to methylphosphinic acid is arsenmonomethylic acid, discovered by M. Baeyer; that analogous to dimethylphosphinic acid is the well-known kakodylic acid of M. Bunsen; finally, trimethylarsine oxide has been obtained by M. Cahours when submitting trimethylarsine to the action of oxidizing agents.

Orthoarsenic acid.............. $\left.\begin{array}{l} HO \\ HO \\ HO \end{array}\right\} AsO.$

Arsenmonomethylic acid........ (Methylarsinic acid) $\left.\begin{array}{l} CH_3 \\ HO \\ HO \end{array}\right\} AsO.$

Kakodylic acid................ (Dimethylarsinic acid) $\left.\begin{array}{l} CH_3 \\ CH_3 \\ HO \end{array}\right\} AsO.$

Trimethylarsine oxide .... $\left.\begin{array}{l} CH_3 \\ CH_3 \\ CH_3 \end{array}\right\} AsO.$

The formation of methyl- and dimethylphosphinic acid is thus seen to illustrate again the unmistakable analogy of the two elements, phosphorus

and arsenic, already traced in so many directions. I hope that the continuation of these researches will still further elucidate this similarity. There can be no doubt that the several members of the kakodyl series from which arsenmonomethylic and kakodylic acids have been produced will soon be represented amongst the derivatives of phosphorus. The discovery, too, of the primary and secondary arsines, the oxidation of which, as is obvious from the results described in this note, must yield the same acids, will probably not have long to be waited for.

IV. Primary and Secondary Ethylic Derivatives of Phosphoretted Hydrogen.

After what has been elicited by the investigation of the methyl compounds, there could be but little doubt as to the phenomena to be observed by repeating the experiments in the ethyl series. Nevertheless the study of the ethyl compounds presented an interest of its own. In the first place, it was desirable experimentally to generalize the new method by applying it to different groups; again the properties likely to be possessed by the ethylphosphines, and more particularly their higher boiling-points, appeared to promise that the experimental difficulties of this inquiry would be materially diminished by working in the ethyl series.

The formation of the ethylphosphines by means of ethyl iodide, phosphonium iodide, and zinc oxide takes place with the same facility and precision as that of the methylated compounds. The reaction, however, proceeds but slowly at the temperature of boiling water. Exposure of the tubes to a temperature between 140° and 150° C. for six or eight hours is sufficient for the transformation. The product of the reaction in this case, exactly as in that of the methyl series, contains the primary and secondary bases only. Their separation and preparation in a state of purity is carried out in exactly the same manner as that of the corresponding methyl compounds.

Ethylphosphine.

$$C_2H_7P = \left.\begin{matrix} C_2H_5 \\ H \\ H \end{matrix}\right\} P.$$

Colourless, transparent, mobile liquid, powerfully refractive, lighter than water, in which it is insoluble. It is easily soluble in alcohol and ether. These solutions are without any action on vegetal colours. Ethylphosphine boils constantly at 25°, and is thus seen to possess exactly the same boiling-point as dimethylphosphine (p. 227), with which it is isomeric.

$$\left.\begin{matrix} CH_3 \\ CH_3 \\ H \end{matrix}\right\} P = \left.\begin{matrix} C_2H_5 \\ H \\ H \end{matrix}\right\} P = C_2H_7P.$$

The odour of this compound is overwhelming; it strongly recalls that of the formonitriles, producing more especially the same sensation of bitterness on the tongue and to the very depth of the throat. Odour and taste

are, however, obviously, in consequence of the volatility and oxidability of the compound, far less persistent. The vapour of ethylphosphine bleaches cork like chlorine; very peculiar, too, is its action on caoutchouc, which in contact with it becomes translucent, losing its elasticity. Chlorine, bromine, and nitric acid inflame the compound. Ethylphosphine combines with sulphur and carbon bisulphide, though far less energetically than triethylphosphine; nor are the compounds thus produced crystalline, like the corresponding derivative of triethylphosphine; as yet they are but imperfectly studied.

Like the monomethylated base, ethylphosphine unites with chlor-, brom-, and iodhydric acids to saline compounds. The solution of the chlorhydrate yields with platinum perchloride a double salt, crystallizing in fine crimson-red needles, which resemble freshly prepared chromic acid. The most beautiful salt of ethylphosphine is the iodhydrate. It forms white four-sided tables, which, in a current of hydrogen, may be sublimed even at the temperature of boiling water. The aspect of the substance forcibly recalls that of ordinary sal ammoniac. Analysis showed that this salt possesses the composition assigned to it by theory, viz.:—

$$C_2 H_8 PI = (C_2 H_5) H_2 P . HI.$$

The iodhydrate easily dissolves in water, but not without being entirely decomposed. In dry air the crystals of the salt are permanent; but even when breathed upon they are altered, the decomposition being indicated by the powerful odour emanating from the salt, which, when dry, is perfectly inodorous. When a crystal is thrown upon water, it is seen to disappear with evolution of gas. Alcohol dissolves the iodhydrate, but only with partial decomposition; in ether it is insoluble. The only solvent in which the salt was found to be soluble, though likewise but sparingly, is concentrated iodhydric acid. Addition of ether to this solution causes the salt to separate in large, well-formed tables, having often a length of 1 centimetre; they are generally very thin, their surface presenting magnificent iridescence.

Diethylphosphine.

$$C_4 H_{11} P = \left.\begin{matrix} C_2 H_5 \\ C_2 H_5 \\ H \end{matrix}\right\} P.$$

Transparent, colourless, perfectly neutral, mobile liquid, floating upon water, in which it is insoluble, powerfully refracting light. It boils constantly at 85°, *i. e.* 60° higher than the primary base. The odour is penetrating and most persistent, very different from that of ethylphosphine, distantly resembling that of triethylphosphine. The diethylated compound attracts oxygen with far more energy than the primary base; and more than once have I seen this compound bursting into flame on opening a bottle. Diethylphosphine combines with sulphur and carbon bisulphide; these combinations, like those of the monoethylated base, are liquids.

The secondary phosphines readily dissolve in acids. The salts, as far as my experience goes, are difficult to crystallize, with the exception of the iodhydrate. The solution of the chlorhydrate gives, with platinum perchloride, a fine platinum salt, crystallizing in orange-red prisms, which are, however, easily changed. It is interesting to perceive that the salts of diethylphosphine resist the action of water, whilst those of the monoethylated base are readily decomposed; the deportment of the phosphines is thus seen to afford an instructive illustration of the increased basicity the phosphoretted molecule acquires with the number of ethyl groups it has incorporated.

Products of Oxidation of the Primary and Secondary Ethylphosphines.

It is my intention to study in detail the products of oxidation of ethyl- and diethylphosphine, which, according to special circumstances, appear to vary to a considerable extent. Hitherto I have examined only the terminal compounds. These are perfectly analogous to the products similarly obtained from the methylated bases; they need not therefore be more than cursorily mentioned.

Ethylphosphinic Acid.—Preparation, appearance, and properties of the body obtained by the action of nitric acid upon ethylphosphine resemble in every respect the acid similarly obtained from methylphosphine, and described in a previous communication (comp. p. 228). The ethyl compound is likewise exceedingly soluble in water, alcohol, and ether; it fuses at 44°, and may be distilled without decomposition. The formula

$$C_2\,H_7\,PO_3 = (C_2\,H_5)\,H_2\,PO_3$$

was fixed by the analysis of a silver-salt. This salt was formed by saturating the free acid partially with silver oxide, and precipitating the concentrated liquid by alcohol. It is an amorphous, yellowish powder, insoluble in water and alcohol, the composition of which is represented by the formula

$$(C_2\,H_5)\,Ag_2\,PO_3.$$

Diethylphosphinic Acid.—On treating diethylphosphine with nitric acid, all the phenomena present themselves which are observed in the corresponding experiment in the methyl series. The acid produced, however, has been observed as yet in the liquid state only; it refuses to solidify even at a temperature of $-25°$. The composition of the acid,

$$C_4\,H_{11}\,PO_2 = (C_2\,H_5)_2\,H\,PO_2,$$

was established by the analysis of a silver compound. The latter was obtained by nearly neutralizing the acid by silver oxide and precipitating the evaporated liquid by alcohol. The silver diethylphosphinate contains

$$C_4\,H_{10}\,Ag\,PO_2 = (C_2\,H_5)_2\,Ag\,PO_2.$$

The products of oxidation of the ethylated phosphorus bases are thus proved to be perfectly analogous to the group of methyl bodies previously described (p. 232), as seen by glancing at the following Table:—

Orthophosphoric acid........ $\left.\begin{matrix} HO \\ HO \\ HO \end{matrix}\right\} PO.$

Ethylphosphinic acid........ $\left.\begin{matrix} C_2H_5 \\ HO \\ HO \end{matrix}\right\} PO.$

Diethylphosphinic acid...... $\left.\begin{matrix} C_2H_5 \\ C_2H_5 \\ HO \end{matrix}\right\} PO.$

Triethylphosphinic oxide .... $\left.\begin{matrix} C_2H_5 \\ C_2H_5 \\ C_2H_5 \end{matrix}\right\} PO.$

V. Aromatic Phosphines.

The well-defined results which the easy and handy use of phosphonium iodide, as a source of phosphorus compounds, has furnished in the methyl and ethyl series, and, as I shortly intend to communicate to the Society, also in the propyl, butyl, and amyl series, very naturally created the wish of extending these researches also to the aromatic phosphines. It appeared especially worthy of interest to study an aniline with phosphorus in the place of nitrogen,—in other terms, phenyl phosphine and, indeed, the whole series of phenylated phosphorus bases. I have instituted many experiments in the hope of obtaining these bodies, but as yet without success. Considering the remarkable inactivity of benzol chloride and analogous benzol compounds under the influence of ammonia, I could scarcely hope to form phenylphosphine by acting on phosphonium iodide with benzol chloride. Nevertheless the experiment was made; but, although tried under varying conditions, I have not been able to observe the generation of phosphorus bases in this process. The benzol chloride is reduced to benzol, which itself is then no further changed, even by raising the temperature, as the interesting researches of M. Baeyer have already proved.

But even the action of phosphonium iodide on phenol, from which, looking at the experience gathered in the methyl and ethyl series, I was fairly entitled to hope that at least the tertiary and quartary compounds would emerge, gave rise to changes very different from those anticipated. The singular phosphorus bodies generated in this reaction claim further examination. New processes, different from those hitherto followed, must therefore be devised for the production of phenylphosphine.

Exactly as in the formation of phenylphosphine, I have hitherto failed in that of phosphoretted toluidine. On the other hand, the preparation of a phosphorus base corresponding to benzylamine presents no difficulty. Considering that benzyl chloride is easily converted into benzylamine by the action of ammonia (as the researches of Cannizzaro and Limpricht have shown), it could not possibly be doubted that by causing benzyl

chloride and phosphonium iodide to meet under appropriate circumstances, an aromatic phosphorus base would be obtained.

Benzylphosphine.

$$C_7\,H_9\,P = \left.\begin{matrix} C_7\,H_7 \\ H \\ H \end{matrix}\right\} P.$$

For the preparation of this body it is not necessary to employ the benzyl chloride in its pure state. It suffices to operate with toluol chlorinated whilst hot, which boils between 150° and 180°. The substances acting on one another are employed in the same proportions which, in the methyl and ethyl series, are known to yield satisfactory results, namely 2 mol. of benzyl chloride, 2 mol. of phosphonium iodide, and 1 mol. of zinc oxide. A digestion of six hours' duration at 160° is sufficient for the formation of benzylphosphine. When the reaction is finished the digestion-tubes contain a white mass of crystals, which is generally forced out by the phosphoretted hydrogen escaping when the tubes are opened. When the product of the reaction was distilled with the vapour of water, an oily liquid, heavier than water, possessing an extremely characteristic persistent odour, passed over. This was separated from the water by means of a separating funnel, dried by allowing it to stand on caustic potass, and submitted to fractional distillation in a current of hydrogen. It commenced boiling a few degrees above 100°; the mercury then rose rapidly to 180°, at which temperature a large quantity of a colourless, powerfully refractive liquid distilled. That passing between 180° and 190° was collected apart from the earlier distillate, consisting chiefly of toluol (which is regenerated from the benzyl chloride by the phosphonium iodide). The liquid boiling at 180° is benzylphosphine; the residue in the retort contains dibenzylphosphine and other products.

Purified by a second distillation in a current of hydrogen, benzylphosphine is found to have the constant boiling-point 180°. In contact with the air the aromatic phosphorus base attracts oxygen with such avidity that the thermometer rises to 100° and more, and thick white clouds are formed. Benzylphosphine is insoluble in water, but easily so in alcohol and ether. The aromatic phosphorus base shares the characteristic property of the other primary phosphines, viz. that of forming a crystallizable iodhydrate. This is obtained by mixing the phosphine with fuming iodhydric acid, when it falls as a white and, apparently, amorphous mass. The insolubility of the iodine compound presents an easy method of recovering any benzylphosphine that may have passed over in the first distillate containing toluol. The white precipitate of benzylphosphine iodhydrate dissolves on warming in iodhydric acid, and forms, as the solution cools, white needles, often more than a centimetre long, which, in contact with water, are decomposed into the acid and base. By washing with dry ether and drying in a stream of hydrogen at 100°, the iodhydrate may

easily be obtained in a state of purity for analysis. In this experiment, occasionally, well-formed tables are produced of considerable dimensions and great beauty. The salt has the composition—

$$C_7\,H_{10}\,PI = (C_7\,H_7)\,H_2\,P.HI.$$

Benzylphosphine combines likewise with concentrated chlorhydric and bromhydric acids. I have not, however, been able to obtain these compounds in crystals. The chlorhydrate gives with platinum perchloride a yellow insoluble precipitate.

Benzylphosphine is formed according to the equation—

$$2C_7\,H_7\,Cl+2[H_3\,P.HI]+Zn\,O=2[(C_7H_7)\,H_2\,P.HI]+Zn\,Cl_2+H_2O.$$

This equation, however, shows only one phase of the reaction, in which at the same time several other substances are formed.

Dibenzylphosphine.

$$C_{14}\,H_{15}\,P=\left.\begin{matrix}C_7\,H_7\\C_7\,H_7\\H\end{matrix}\right\}P.$$

This compound is found in the liquid remaining in the retort after the distillation of benzylphosphine. By long standing, especially in the presence of solid alkali, this fluid solidifies to a soft mass of crystals, which are collected on a linen filter, in order to free them, by pressing, as much as possible from adhering liquid. The still strongly coloured crystals are then dissolved in alcohol and treated with a little animal charcoal. The colourless liquid thus obtained deposits on cooling beautiful white crystals of the new compound. By repeated crystallization from boiling alcohol, dibenzylphosphine is obtained in a perfectly pure state. Thus prepared, the phosphine forms large brilliant needles, mostly grouped in stars or glistening tufts, perfectly tasteless and colourless, which are insoluble in water, but dissolve, though sparingly, in boiling alcohol. In ether they are nearly insoluble. The crystals melt at 205°; at a higher temperature they are volatilized, but not without partial decomposition. With the entrance of the second benzyl group, the basic characters, which in the monobenzylphosphine are still distinctly perceptible, have entirely disappeared. Dibenzylphosphine does not dissolve in acid, nor have I succeeded in obtaining a platinum salt. In this respect the aromatic secondary phosphine essentially differs from the analogous bodies in the ethyl and methyl series, which are well-marked bases. The absence of basic properties cannot, however, be looked upon as strange, since even in the secondary aromatic amines we find the tendency to form saline compounds very nearly effaced. The difference of dibenzylphosphine from the corresponding terms in the methyl and ethyl series becomes obvious, moreover, by its behaviour with oxygen; for whilst dimethyl- and diethylphosphine take fire on contact with air at the ordinary temperature, oxygen is without any action, even at an elevated temperature, on the dibenzylated phosphorus

base. As dibenzylphosphine yields no compounds, I was limited to the analysis of the body itself. This analysis led to the formula—

$$C_{14} H_{15} P.$$

The formation of dibenzylphosphine takes place according to the equation—

$$2 C_7 H_7 Cl + H_3 P.HI + ZnO = (C_7 H_7)_2 HP.HI + ZnCl_2 + H_2 O.$$

Benzylphosphine and dibenzylphosphine are not the only phosphoretted products of the action of benzyl chloride on phosphonium iodide. The mother-liquor of dibenzylphosphine contains yet another phosphorus body. The idea naturally suggested itself that it might be tribenzylphosphine; but, notwithstanding many efforts, I have not succeeded in obtaining this compound. The mother-liquor of dibenzylphosphine consists, for the greater part, of a viscous substance, soluble in alcohol but insoluble in water, which is precipitated by lead-salts. This substance tenaciously adheres to a small quantity of a crystallizable body, which most probably is no other than dibenzylphosphine. All attempts to obtain this glutinous substance (which appears to possess acid properties) in a condition fit for analysis have hitherto failed.

In conclusion I may be allowed most warmly to thank Messrs. F. Hobrecker and E. Mylius for the untiring perseverance with which they have (the former in the earlier stages, the latter more recently) assisted me in these researches.

II. "On some Heterogenetic Modes of Origin of Flagellated Monads, Fungus-germs, and Ciliated Infusoria." By H. Charlton Bastian, M.D., F.R.S., F.L.S., Professor of Pathological Anatomy in University College, London. Received February 15, 1872.

The "pellicle" that forms on infusions of organic matter which are exposed to the air is composed for the most part of a dense aggregation of *Bacteria* of various sizes and shapes imbedded in a more or less abundant, pellucid, gelatinous material. Very frequently there are also a variable number of intermixed *Vibriones* and more or less characteristic *Torulæ*. The *Bacteria* in this layer are mostly placed vertically to the surface, so that an examination of it under the microscope generally reveals the appearance of a stratum densely studded with small though tolerably uniform granules. On attempting to remove a portion of this pellicle, it is found to constitute a more or less coherent membrane.

It is now a well-known fact that when two or more Amœbæ chance to come into close contact with one another, they may fuse so as to constitute a larger individual of the same kind, which afterwards creeps about and seizes food as its component parts had previously done. Such a process must be classed under the head of Homogenetic Biocrasis; for, although

separate living units fuse to form a new individual, the process is one of mere fusion, and the product is similar in kind although necessarily larger than its components.

Similar mutual attractions, however, may be exerted by other living units when brought into close contact with one another, and the result may be the formation of an aggregate in which considerable molecular changes are compelled to take place. The products resulting from such a fusion may be quite different from the originally fused units; whilst they will differ at different times according to the precise nature and number of the units which enter into combination. Such processes are frequently to be observed taking place in various parts of the "proligerous pellicle." It is in this way, in fact, that those phenomena occur which make the name "proligerous pellicle" suitable for the scum that forms on organic infusions. The processes themselves come under the head of Heterogenetic Biocrasis.

The first person who actually described the microscopical appearances characterizing the evolution of higher organisms from the pellicle was M. Pineau. This he did in 1845, in a memoir entitled "Recherches sur le Développement des Animalcules Infusoires et des Moisissures"*. More precision, however, was given to the subject in 1859, by M. Pouchet, when, in his 'Hétérogénie,' he described the mode of origin of some of the organisms which had formed the objects of Pineau's investigations as well as of some different organisms.

Although some of these observations have not been recorded with all the details which might have been desired, yet I have satisfied myself that the statements made by Pineau and Pouchet are substantially correct. The observations of the latter have, moreover, been confirmed by MM. Joly and Musset, M. Pennetier, and others.

Nearly two years ago I described in 'Nature' † the mode of origin of certain corpuscular organisms, and of some Fungus-germs, from differentiated portions of the pellicle of hay-infusions. These observations I have since repeatedly confirmed; and I now wish to describe other allied processes, and the means by which I am enabled to obtain, almost at will, either animal or vegetal forms from certain *embryonal areas* which are produced in the pellicles of similar infusions.

The characters of the pellicles that form on different hay-infusions of the same strength differ notably, according to the temperature of the water with which the infusions have been made; and, to a less extent, according to the mean atmospheric temperature to which they are subsequently exposed. If the infusion has been prepared with very hot water (140° F. and upwards), only a thin and somewhat tough pellicle will form, secondary changes will take place in it very slowly, and they will lead only to the

* See Ann. des Sc. Nat. (Zoologie), t. iii. p. 182 and t. iv. p. 103.

† 'Nature,' 1870, no. 35, pp. 172, 173.

evolution of products of a certain kind. When prepared with moderately hot water (120° F.), or with cold water (60°–70° F.), the pellicles which are produced become thicker and thicker, and continue for a long time to be soft and pulpy. The changes that may take place in a pellicle of the latter kind are very varied, and they may give rise to a multiplicity of organic forms.

For a long time my observations were carried on upon infusions made with hot water, and they were also conducted during the winter months, so that the secondary changes which I was able to observe in the pellicle were neither varied nor numerous *. That which is to follow concerning my own observations has been learned from an investigation of the pellicles which form on filtered hay infusions prepared both with warm and with cold water.

In all cases, and at whatsoever temperature the infusion may have been prepared, the earliest change which takes place in the pellicle is such as I have previously described †. In certain portions of it (altogether irregular in size, shape, and distribution) the aggregated *Bacteria* begin to form around themselves a certain amount of pellucid gelatinous matter, in which they become imbedded. This change may be well seen in pellicles made with hot water, because such areas continue (more especially when the atmospheric temperature is low) for several days without undergoing much alteration. The *Bacteria* in them are slightly separated from one another, rather larger in size, and irregularly placed with regard to the direction of their long axis. Such areas are freely intermixed with other less altered portions in which the *Bacteria* are densely packed, even smaller than natural, and apparently not separated by any pellucid material. Any of the modified areas may after a time undergo changes very similar to those which I described in 'Nature' as resulting in the production of fungus-spores.

On the other hand, a totally different fate may occasionally await such modified areas. Thus, in a strong infusion prepared with water at a temperature of about 120° F., the pellicle was found to be abundant and pulpy; and on the second day areas of the kind above described were most

* During this time I was also working at the subject of Archebiosis, and I had not then ascertained that even in this part of the investigation infusions are more efficacious if prepared with moderately hot (120°–130° F.) rather than with very hot water. They answer better when made with warm water (at the temperature above named) than with cold water, because they can thus be obtained in a more concentrated state. And seeing that in this kind of experiment the fluids have afterwards either to be boiled or otherwise superheated (before or after closure of the flasks), the slight increase in temperature during the preparation of the infusions becomes of less consequence. But in studying Heterogenesis, and with the view *of witnessing all the higher changes* which may take place in a pellicle, the organic infusions or *macerations must be made with cold water*, and subsequently filtered.

† 'Nature,' no. 35, 1870.

marked and numerous*. The contained *Bacteria* very soon became notably larger and distinctly loculated—each loculus containing two or three granules; whilst the jelly-like material was so abundant that every *Bacterium*† was distinctly isolated from its fellows. These particular areas were watched for several days, and were not found to have any tendency to undergo segmentation, although myriads of Monads had been formed in adjacent portions of the pellicle, as well as Fungus-germs which had vegetated into mycelial filaments and bore numerous heads of spores similar to those of a small variety of *Penicillium glaucum*. The *Bacteria* included within these areas seemed to possess too much inherent vigour to lose their own individuality—a supposition which was confirmed by their great increase in size and subsequent development. On the fourth and fifth days many were seen which had grown out into minute filaments, resembling what is commonly regarded as *Leptothrix*, although they also possessed all the characteristics of a miniature fungus-mycelium.

Thus, then, we may have modified areas in which the contained units flourish and grow, whilst still preserving their own individuality; or we may have pellucid areas, persisting as such for a certain time, whose units at last undergo a process of molecular fusion and regeneration leading to the production of a segmenting embryonal area from which brown Fungus-germs are produced‡. And, lastly, there may be pellucid areas which, almost as soon as they are formed, begin to undergo those changes whereby they are converted into true embryonal areas.

Many variations exist in the character of these areas in different cases, some of which I will now attempt to describe, as I have lately had an opportunity of watching all sorts of transitional conditions.

The pellicle which formed on a filtered maceration of hay during frosty weather (when the temperature of the room in which the infusion was kept was rarely above 55° F., and sometimes rather lower than this) presented changes of a most instructive character. On the third and fourth days the pellicle was still thin, although on microscopical examination all portions of it were found to be thickly dotted with embryonal areas.

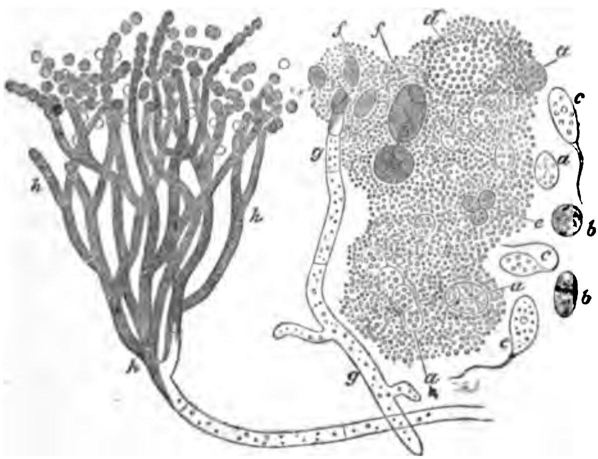
* The daily atmospheric temperature being about 62° F.

† The corpuscular appearance of some of these bodies was so marked that they might, perhaps, more appropriately be spoken of as *Torulæ*.

‡ During this process the contained *Bacteria* disappear, and a whitish refractive and homogeneous protoplasm is produced in the place of the jelly and its contained granules. If we turn to the account given of the origin of the "germinal membrane" in the ova of higher animals, we may be struck by the similarity of the phenomena. Müller says (Baly's Translation, vol. i. p. 9):—"It appears, indeed, that the germinal membrane is formed by the attraction and aggregation of the globules of the yolk; but all parts developed in this germinal membrane are produced by solution of these globules, and conversion of them into a matter in which no elementary particles can be distinctly recognized, and of which the molecules must at any rate be beyond comparison more minute than the globules of the yolk and germinal membrane." The subsequent development of blastodermic cells from this mass also agrees closely with what occurs in our embryonal areas.

Nearly all of them were very small; but a few areas of medium size were intermixed*. The smallest were not more than $\frac{1}{4000}$ of an inch in diameter, and these separated themselves from the pellicle as single corpuscles; slightly larger areas broke up into two or three corpuscles; and others, larger still, into 4–10 corpuscles. In most of these small areas, the corpuscles were formed with scarcely any appreciable alteration in the refractive index of the matter of which they were composed: this simply became individualized, so that the corpuscles separated from the surrounding pellicle and from their fellows, still presenting all the appearance of being portions of the pellicle, and exhibiting from 4–10 altered *Bacteria* in their interior. In some cases the products of segmentation soon developed into actual flagellated Monads in a manner presently to be described; whilst in others they seemed to remain for a

Fig. 1.—*Simplest Mode of Development of Monads and Fungi from the Pellicle.* (× 1670.)



a, *a*. First stage of differentiation of separate and aggregated corpuscles.
b, *b*. Such corpuscles in more refractive condition, developing into Monads.
c, *c*. Fully developed Monads.
d. Larger area in first stage of differentiation.
e. Refractive corpuscles which will develop either into Monads or Fungi.
f, *f*. More refractive corpuscles which give birth to mycelial filaments as in *g*, and ultimately expand into a form of *Penicillium* (*h*).

longer period in the condition of simple motionless corpuscles. Other solitary corpuscles or small areas began to form in the pellicle in precisely the same manner, though they speedily assumed a highly refractive and homogeneous appearance. Why some should undergo such a change

* In these medium-sized areas segmentation was accompanied by the production of homogeneous and highly refractive protoplasm.

and not others, seems quite impossible to say. One can only assert the fact, and add that these highly refractive ovoid corpuscles were, for the most part, more prone to produce Fungus-germs than Monads. Many of them soon grew out into dissepimented fungus-filaments, which rapidly assumed the *Penicillium* mode of growth. The spores, which were abundantly produced in terminal chaplet-like series, were, however, small, homogeneous, spherical, and colourless.

On several occasions I have seen Monads produced in this way, by direct and immediate separation from the pellicle; though, as M. Pineau had stated, on other occasions they may be seen to arise in groups, in which they first appear as aggregations of motionless corpuscles. The solitary mode of origin is that which has been described by M. Pouchet; and although the details given by him are not very full, so far as they go they are in accordance with my own observations. M. Pouchet, for instance, describes the flagellum as being closely applied to the body, and motionless for a time. This I have also found to be the case. I have, moreover, on one or two occasions, been able to watch all the transitions from the mere motionless corpuscle to the flagellated Monad; just as, on other occasions, I have watched almost similar corpuscles develope into Fungus-germs.

Sometimes the flagellum is seen attached to corpuscles which still display almost unaltered *Bacteria* imbedded in their substance: generally, however, the corpuscles which separate from the pellicle in this comparatively unaltered condition, undergo certain slow changes before the flagellum is developed. The contained *Bacteria* become more and more indistinct, whilst the general substance of the corpuscle becomes rather more refractive, so as to produce ordinary protoplasm. Corpuscles about $\frac{1}{5000}''$ in diameter are often very obscurely granular and quite motionless. They grow, however, and when they have attained the size of $\frac{1}{4000}''$ in diameter they frequently begin to exhibit slow undulating alterations in outline, and tend to assume an ellipsoidal form. One specimen, $\frac{1}{3300}''$ in diameter, was seen without a flagellum, but slowly alternating between the spherical and ellipsoidal forms. Suddenly, at one extremity of the ellipsoid, a series of rapid contractions and protrusions of its substance were observed, and when they ceased, a motionless filament was seen bent around one side of the body. Three minutes afterwards a vacuole appeared for the first time at the opposite extremity of the ellipsoid. The corpuscle remained almost motionless for twenty-five minutes, merely exhibiting very slight changes in outline; after thirty minutes the first slow bendings of the flagellum were seen; and after thirty-five minutes the whole organism began to exhibit slow semi-rotations, at intervals of a minute or two. After forty minutes the movements were pronounced and of a starting character, dependent upon sudden contractions of portions of the body of the organism rather than upon movements of its flagellum. After fifty-five minutes, the corpuscle unfortunately became hidden, owing to its having floated underneath a portion of the pellicle. How far the rapidity of the evolution of the

flagellum, and its subsequent movements, were impaired by the glare of artificial light to which the organism was subjected, cannot be said. Certainly, however, the flagellum seems to be thrown out much more rapidly in other cases. Speaking of simple organisms of this kind, Dr. T. R. Lewis says*:—"Frequently a succession of pseudopodia are seen projected in a wave-like manner, as if lashing the fluid." And, again, of other similarly active animalcules he says:—"Sometimes one flagellum is seen, a posterior one, at others an anterior one also, both being retractile at will; and another may be darted forth out of any portion of its body." Again, where tailed "zoospores" are produced from Algæ or from such Fungi as *Achlya* and *Cystopus*, they are also evolved most rapidly—two hours often sufficing for the entire production of a brood of such flagellated Monads from the segmentation of a mass of formless protoplasm.

Monads, indeed, are frequently produced from the "pellicle" in precisely the same manner as that by which they arise within the terminal chambers of certain Algæ or Fungi—that is to say, they result from the segmentation of a mass of homogeneous protoplasm†. The steps of this process I will now describe.

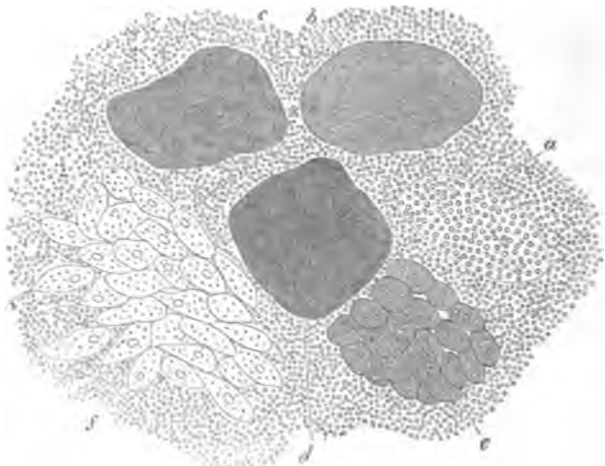
An infusion of hay was made with water at a temperature of about 120° F., and maintained at this heat for three hours. After filtration, four or five ounces of the fluid were poured into a wide-mouthed bottle, and placed under a small bell-jar. When the infusion was examined at the expiration of three days, it was found to be quite turbid, and covered by a moderately thick pellicle. On removing portions of this and submitting it to microscopical examination, the fluid around was found to contain multitudes of very active specimens of *Monas lens*, having an average length of $\frac{1}{3300}''$. The pellicle itself was mostly composed of medium-sized *Bacteria*, though here and there were areas, of different dimensions, in which the units had more the appearance of embryo *Torulæ*. But, contrasting with the very pale fawn-colour of the evenly granular pellicle, there were numerous areas of a whitish colour, refractive, and more or less homogeneous. These areas differed very much in shape and size; some were not more than $\frac{1}{1000}''$, whilst others were as much as $\frac{1}{150}''$ in diameter. Their shape was wholly irregular. Careful examination with a $\frac{1}{16}''$ and a $\frac{1}{32}''$ immersion-objective made it easy to recognize such transitions as are depicted in fig. 2. As in the instances previously recorded, the first appreciable stage in the formation of an embryonal area in the pellicle was a local increase in the amount of gelatinous material between the units of this portion of the pellicle, so that they became more distinctly separated from one another than in adja-

* Report on the Microscopic Objects found in Cholera Evacuations &c. (Calcutta, 1870), pp. 26 & 33.

† In both cases, also, it happens that the products of segmentation are sometimes motionless and sometimes active units.

cent parts. Gradually these particles became less sharply defined, and at last scarcely visible, in the midst of a highly refractive protoplasmic mass which began to exhibit traces of segmentation.

Fig. 2.—*Segmentation of Embryonal Areas into Monads.* ($\times$ 1670.)



a. First stage of differentiation.
b. Second stage; area almost homogeneous and refractive.
c. First traces of segmentation.
d. Segmentation more complete; units highly refractive.
e. Units less refractive, forming tailless corpuscles.
f. Fully developed Monads derived from such corpuscles.

Masses of this kind were seen which had been resolved by such a process of segmentation into a number of spherical corpuscles about $\frac{1}{4500}''$ in diameter. These were at first highly refractive, though they gradually became rather less so, and revealed the presence of two or three minute granules in their interior. In other adjacent areas, a number of densely packed, pliant, and slightly larger corpuscles were seen actively pushing against one another. When they separated they were found to be active ovoid specimens of *Monas lens* about $\frac{1}{3300}''$ in length, and provided with a vacuole and a rapidly lashing flagellum. On the fourth day the number of embryonal areas throughout the pellicle had increased, and the specimens of *Monas* existed in myriads in the infusion. They were tolerably uniform in size, though some were notably smaller than the average, owing to the fact that they were products of a recent fission, all the stages of which were watched on many occasions*. On the sixth day many of

* It took place mostly in a longitudinal, though occasionally in a transverse direction. I have never seen the whole process occupy less than twenty minutes.

the Monads had much increased in size, some of the larger of them measuring $\frac{1}{1800}''$ in length. Others had lost their flagellum, and were existing in the form of ovoid or rounded corpuscles, which were motionless, though still provided with a vacuole, and now also with a solid nucleus about $\frac{1}{20000}''$ in diameter (fig. 3, *b, c, d*). All stages were seen, between the ovoid corpuscle $\frac{1}{3300}''$ in length and a much larger Amœba of the kind just described, which was either motionless or else, at intervals, exhibited slowly evolved and blunt protrusions at its periphery.

In other specimens the most easy and rapid alternations were seen between the shape and mode of locomotion which pertain to Monads and those which are characteristic of Amœbæ. Monads which had been previously in active motion would at times come to a state of rest, develop two or three vacuoles in their interior, and behave in all respects like an Amœba, save for the presence of the now languidly moving flagellum. After remaining in this state for a variable time, some of them would just as abruptly cease to display the amœboid movements, the extra vacuoles would disappear, the shape of the Monad would be resumed, and with it the lashing movements of the flagellum, which again gives rise to the rapidly darting gyrations of the organism. Whilst in the amœboid state the changes in shape were moderately rapid; though two or three organisms were watched, one portion of which remained rounded and apparently attached to the glass, whilst the opposite extremity threw out and retracted comparatively long processes with lightning-like rapidity—some of them being filiform, like the ordinary persistent flagellum*.

On the seventh day thousands of the motionless spheroidal Amœbæ were seen, which had much increased in size. They were now as much as $\frac{1}{1350}''$ in diameter, and displayed one or more vacuoles (fig. 3, *d*). Each one contained a distinct nuclear particle, though there was an almost complete absence of granules, the body substance being quite pellucid. Some organisms of the same kind, though rather smaller, contained the ordinary granules in their interior and also exhibited slow amœboid movements; whilst many Monads of the same size and general appearance were seen exhibiting amœboid changes of form, though they had not yet lost their almost motionless flagellum.

On the eighth day there were myriads of active Amœbæ around every portion of the pellicle which was examined; they were, in fact, at this period, almost as numerous as the Monads. Great numbers also existed in the spherical motionless condition.

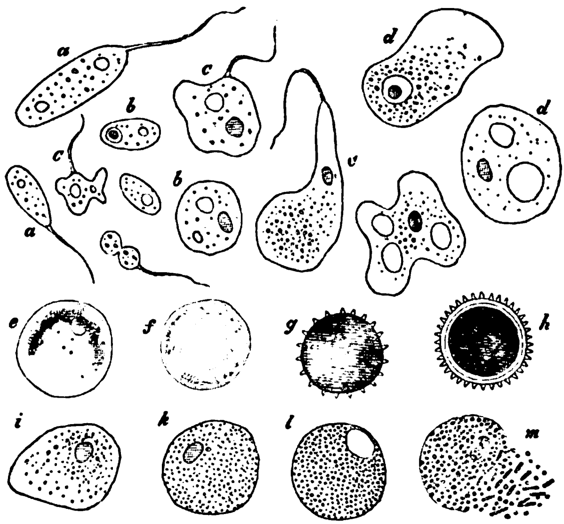
On the ninth day the pellicle began to assume a brownish colour on the surface, owing to the enormous development of minute brown fungus-spores†. Portions of the pellicle were also separating and beginning to shrink, whilst

* The rapidity with which such processes were emitted was similar to what was noticed at p. 245.

† Very similar to those represented in fig. 4, *e*.

many of the spherical Amœbæ were undergoing changes destined to result in encystment.

Fig. 3.—*Phases in the Life-history of Monads and Amœbæ.* (×1670.)



a–d. Representing the stages by which Monads became converted into Amœbæ.
e–h. Stages by which such Amœbæ became encysted.
i–m. Stages by which other Amœbæ became resolved into Bacteria.

On the tenth day, similar though more advanced changes were seen. Although the majority of the Amœbæ were still active and polymorphic, hundreds of them were becoming encysted, and the different stages of the process were quite obvious. They were these:—The previously spherical Amœbæ lost their vacuoles, the granules almost wholly disappeared, and the body generally became slightly refractive, the nucleus being still visible. After a time the nucleus became invisible, and the whole substance of the organism assumed a homogeneous and highly refractive appearance—so that when it was examined a little beyond the focal distance it looked almost like an oil-globule. There was a decided condensation, also, of the outer layer of protoplasm, this being the first trace of the cyst-wall. Subsequently the cyst-wall became more and more manifest, whilst the size of the sphere slightly diminished, and assumed a faintly brownish tinge. From the surface of the developing cyst there were a number of very short ray-like projections (fig. 3, *g*). In the last stage, whilst the cyst-wall became more developed and the projections more obvious, the whole exterior envelope assumed a decidedly brown colour, and the contained protoplasmic mass,

which had again become less refractive, distinctly separated from the cyst-wall *.

As the virtues of this infusion seemed to be getting exhausted, on the same (tenth) day I transferred a portion of the pellicle to the surface of a new weak infusion of hay, which had been previously boiled. On the following day the Monads were found to have increased very much in size, and so also had many of the Amœbæ. Several large ovoid Monads on measurement were found to be as much as $\frac{1}{100}''$ in length; they had, in fact, become nearly twice as long as the largest of those which had existed in the old infusion.

Five days afterwards (sixteenth day), when another portion of this transferred pellicle was examined, all the Monads were found to have disappeared, with the exception of a few which were in a motionless state, and were apparently about to be converted into Amœbæ. These latter organisms existed in teeming myriads: a portion of them had become encysted, whilst of the rest about one half were active, and the others, though not encysted, were almost motionless and more or less granular. On further examination, it was found that the granular Amœbæ (fig. 3, *i–m*) were organisms in a dying state, and that the contained particles were new living units which gradually developed into *Bacteria*. All the stages of this development were to be seen. Thus there were a considerable number of languid Amœbæ which merely displayed a slight increase in the customary number of minute particles situated near or around the nucleus. There were others in which these minute granules, were more numerous; and others still, quite motionless and spherical, which were densely packed with minute particles throughout their whole substance—these particles being motionless and less than $\frac{1}{100000}''$ in diameter. In many of such Amœbæ a clear vacuole was still to be seen. In other organisms the particles had become very slightly larger, whilst the protoplasmic substance in which they had been produced had become fluid, and the particles were to be seen in active movement within the attenuated film constituting the outer layer of the old organism whose nucleus was still visible. When reduced to this condition, trembling movements of the whole mass were seen, owing to the resultant agitations produced by the contained units. Soon the attenuated outer membrane gave way, and, when the contained units were liberated, they at once exhibited very active movements of progression, after the fashion of minute *Bacteria*. The surrounding fluid was, in fact, crowded with similarly minute and active *Bacteria*, and with others slightly larger, which had evidently been produced in this manner.

Such was the fate that overtook those Amœbæ which lived latest in the

* In the course of the next few days myriads of the Amœbæ had undergone this kind of change, which seems to follow quite naturally as soon as the activity of their vital processes becomes diminished. It is the extraordinary molecular activity and constant change of shape of the Amœba which tends to prevent the earlier occurrence of this primary differentiation.

solution. Changes of an unhealthy nature seemed to have been so suddenly induced that the organisms did not possess sufficient energy even to undergo the process of encystment. Their own molecular movements (those which pertain to the ordinary life of the Amœbæ) being so languid, other retrograde changes were initiated, leading to the birth of new particles throughout their substance. *Bacteria*, in fact, were generated by a most typical process of secondary evolution.

The changes which have been thus observed constitute a very remarkable series. The simplest living units (*Bacteria*) first swarm in the infusion*; these become aggregated at the surface so as to form a "proligerous pellicle," in which embryonal areas gradually appear: as a result of segmentation in these embryonal areas, specimens of *Monas lens*, $\frac{1}{3300}''$ in diameter, more or less suddenly make their appearance; they increase in size, occasionally assume an amœboid appearance for a time, and are ultimately transformed into real Amœbæ. The transition is effected by the loss of the flagellum, the appearance of vacuoles in their interior, and the simultaneous manifestation of polymorphism and a creeping mode of progression; at the same time a nuclear corpuscle develops in the interior, and the whole animal grows considerably. At last the Amœbæ gradually cease to exhibit their characteristic movements, whilst they become more or less spherical and motionless. Ultimately a firm bounding membrane is produced, and they pass into the encysted condition, in which, although slightly smaller in size, they constitute spherules $\frac{1}{1550}''$ in diameter. On the removal of some of this pellicle to the surface of a fresh infusion, the Monads and Amœbæ greatly increased in size; all the Monads gradually became converted into Amœbæ, and some of these at first went through the ordinary process of encystment, though at last (on account of some more sudden change in the fluid) they seemed suddenly to lapse into a morbid state. They were apparently unable to encyst themselves; and not being capable of continuing as Amœbæ, there sprang up in their interior a teeming progeny of new units (*Bacteria*), the production of which occasioned the final dissolution of the organisms in which they were evolved.

Other changes, however, took place in this same infusion which deserve to be chronicled. On the sixth day there were seen scattered throughout those portions of the pellicle intervening between the embryonal areas a multitude of solitary spherules, varying in size from mere specks $\frac{1}{30000}''$ in diameter, or less, to bodies $\frac{1}{5000}''$ in diameter. They were colourless, quite motionless, and appeared to be solid and almost homogeneous masses of plasma rather than vesicular bodies (fig. 4, *a*). There were merely faint indications of granules in their interior, and no evidence of a differentiated outer membrane. None of them seemed to be undergoing processes of self-division,

* Although I do not, in the present communication, touch upon the mode of origin of these simplest living units, this question has been pretty fully considered in my 'Modes of Origin of Lowest Organisms,' 1871.

and each appeared to have grown up in the situation in which it was seen*. These corpuscles gradually became more numerous, on to the tenth day, though they underwent no appreciable change except a slight increase in size. On the eleventh day, in the portion of the pellicle which had been transferred to the fresh hay-infusion, many of these stationary bodies, like the Monads and active Amœbæ, were found to have increased to such an extent as to have doubled their transverse measurement. They had also developed a distinct nucleus in their interior (of a ring-like character), vacuoles appeared and disappeared at intervals, and at the same time they exhibited slow and slight amœboid changes in outline (fig. 4, *g*, *h*, *i*); they were, in fact, now obviously converted into sluggish Amœbæ. On the seventeenth day many of them were recognized in the pellicle, scattered amongst the already-described encysted Amœbæ. They had again become motionless and slightly contracted in dimensions; whilst their outer layer was condensed, but not decidedly cyst-like. Many of the smaller sizes were also seen. Seven days afterwards (twenty-fifth day), when another portion of the transferred pellicle was examined, it was found to be densely studded throughout with thousands of encysted Amœbæ, the great majority of which were of the first variety and were pretty uniform in size and appearance. But interspersed amongst them were a considerable number of the imperfectly encysted Amœbæ, of different sizes (*b*). Here and there, however, some of them (now mostly about $\frac{1}{1500}''$ in diameter) presented an unusual appearance. They had assumed a faint brown hue throughout their whole mass, and segmentation had gone on within so as to produce a number of units, whose shape seemed irregular, owing to their being so densely packed (*c*). Other masses were seen in which considerable growth had taken place—these being nearly twice the size and irregular in outline (*d*), though still of a faint brown colour, and composed of a mass of densely packed units, which were held together by an almost invisible bounding membrane. And, lastly, in other places small aggregations of brownish spores were seen (*e*), which had been liberated by the solution of this very attenuated membrane—the separate spores being tolerably thick-walled, bilocular bodies, $\frac{1}{4000}''$ in length by $\frac{1}{12000}''$ in breadth. An examination on subsequent days showed many other of the amœboid bodies breaking up in a similar manner into these brownish, biloculated Fungus-germs.

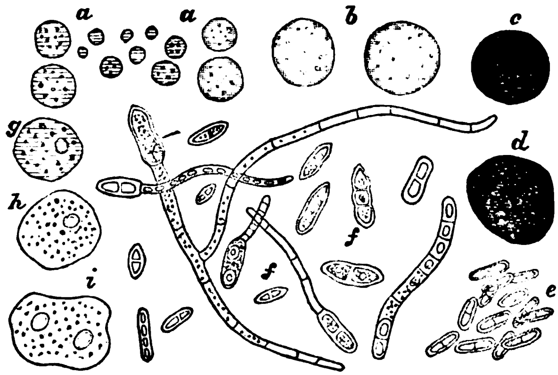
But, strange to say, brown Fungus-germs of an almost similar character had previously presented themselves on the surface of the original infusion, although they had arisen in quite a different manner—by a process of Archebiosis.

In the original infusion, when (on the tenth day) the Amœbæ commenced encysting themselves, portions of the pellicle began to sink to the bottom

* These bodies were evidently quite different from *Monas* and its amœboid derivatives, all of which shrivelled very much when mounted in glycerine-jelly, though the corpuscles which I have just been describing were not appreciably changed.

of the vessel. Three or four days later it was found that the portions of the surface of the fluid which had thus been left uncovered were coated by a delicate, brownish film, which, when examined, displayed appearances

Fig. 4.—*Similar Organisms segmenting into brown Fungus-germs or growing into Amœbæ.* (× 1670.)



a, a. Motionless corpuscles of various sizes.
b. Similar corpuscles much increased in size.
c, d. Segmentation of such corpuscles into brown Fungus-germs.
e. Appearance of germs when liberated.
f, f. Germination of almost similar spores.
g, h, i. Gradual conversion of other corpuscles into Amœbæ when transferred to another fluid.

similar to what I have elsewhere represented. An almost invisible and thin gelatinous stratum existed (a kind of formative membrane), in which every intermediate stage could be detected, between the most minute particle and a brownish, thick-walled, biloculated fungus-spore. The smaller bodies were colourless, solid-looking, and highly refractive; and they seemed much more like mere dead concretions * than living things. All were motionless. Gradually, however, they became less refractive, grew more and more vesicular, and at last assumed a faint brown tint. Although most of them remained as bilocular bodies, others grew and underwent further segmentation, so as to produce tri- and quadrilocular bodies, or "septate spores." During all stages of growth, some of them seemed to undergo an occasional process of fission. They were watched for many days; but as the germs displayed no tendency to develop †, some of them were immersed in a little syrup upon a glass slip, protected by a covering glass, and then set aside in a damp, air-tight, developmental chamber. After about ten days

* Such as are represented in fig. 43 of a work about to be published, entitled "The Beginnings of Life." Other forms, closely allied to *Sarcina*, appeared in some ammonic tartrate solutions, and are figured in Appendix A of the work above mentioned.

† This has been very frequently observed on other occasions (see p. 255).

the germs were found to have become more colourless, to have budded and multiplied, and in many cases to have formed elegant mycelial filaments, such as are represented in fig. 4, *f, f*.

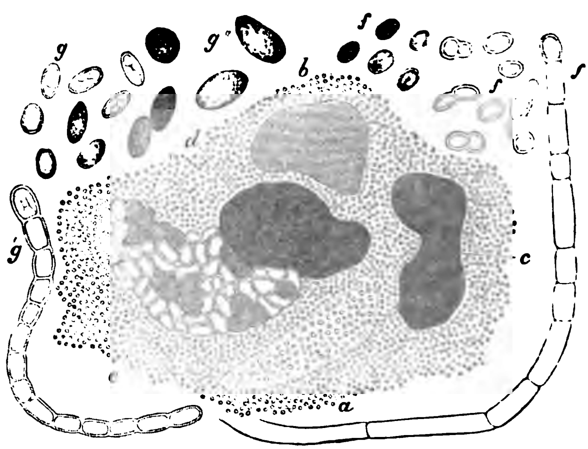
These latter observations are interesting in many respects. It is remarkable, for instance, that germs of precisely the same appearance should arise after such different methods—by origin and growth in a formative membrane in one case, and as the result of the segmentation of a partially encysted Amœba in another case. Then, again, it is extremely interesting to find that these parental Amœbæ had, to all appearances, arisen by a process of Archebiosis, although at one stage of their development they were almost indistinguishable from other Amœbæ seen in the same infusion, which had resulted immediately from the transformation of flagellated Monads, and mediately as products of a process of segmentation occurring in an embryonal area. So that whether we have to do with Fungus-germs or with Amœbæ, their forms are occasionally so intimately associated with the matter from which they have been derived, that similarity may ultimately be met with between organisms whose actual modes of origin have been most diverse.

These amœboid corpuscles which grew up in the midst of the pellicle were peculiar in many respects. In their very early stages it was quite impossible to say whether they were going to develop into Fungus-germs or into Amœbæ; ultimately, however, they seemed to lean more towards the latter mode of development, although the activity which they displayed in this phase of their existence was extremely slight. Finally, we find them, after encystment, undergoing a process of segmentation, by which they give rise to a colony of brown Fungus-germs, in precisely the same manner as that by which the *Protomyxa* of Haeckel gives origin to flagellated Monads which subsequently assume the characters and mode of locomotion of Amœbæ *. This evidence, in addition to other facts previously known, tends to show that the transition from the Amœba to the Monad, or the reverse, may be paralleled by a similar interchangeability between the form and mode of growth of an Amœba and that peculiar to a Fungus; so that either form may at times result from one and the same living matter when it undergoes internal modifications, with or without being subjected to new conditions. This position is still further strengthened by the facts which I have now to record.

* Just as Amœbæ may arise either by Archebiosis or by segmentation of pre-existing living matter (in embryonal areas), with or without passing through the Monad phase of existence, so may Fungus-spores arise by either of these methods. There is also much evidence to show that Monads may arise directly by a process of Archebiosis. They have been found in sealed flasks in which no pellicle was present, even by M. Pasteur; and in some of my experiments with closed flasks (the details of which have not yet been published) the Monads that were found must also have had the primary mode of origin. Some of the new-born specks of living matter which were evolved within one of these closed flasks, previously heated to 270°–275° F., seemed to have grown into Fungus-germs, some into minute Amœbæ, and others into active Monads.

A few days after having made the infusion, the changes in which have just been described, I prepared another with a portion of the same sample of hay. This second infusion, however, was made with water at a temperature of 158° F., which was maintained at this heat for two hours. After filtration it was placed in a similar vessel, and allowed to stand side by side with the other infusion. On the third day, embryonal areas of various shapes and sizes were seen in the firm pellicle which had formed upon the surface*. These areas were distinguished by their whitish, refractive appearance from the slightly fawn-colour of the contiguous unaltered pellicle. Particles of some kind were obscurely seen within the refractive protoplasm, and on the following day many of the areas, which had increased in number, showed signs of commencing segmentation. This process went on

Fig. 5.—*Segmentation of Embryonal Areas into Fungus-germs.* (× 1670.)



a. First stage of differentiation.
b. Area almost homogeneous and refractive.
c. First stage of segmentation.
d. Area showing more complete segmentation.
e. Area in which homogeneous refractive products are being converted into brownish vesicular Fungus-germs.
f, f, f. One form of germ in different stages of development.
g, g′, g″. Another form of germ in different stages of development.

comparatively slowly, and two or three days elapsed before the segmentation was completed. But at last some of the areas were wholly resolved into a number of colourless, homogeneous, and highly refractive spherules, about $\frac{1}{5000}''$ in diameter. Some areas seemed to remain in this condition for two or three days longer, whilst in others the products of segmentation began to undergo change almost before it was completed. In each case, however, the modification was of the same kind, and consisted in a gradual

* The daily temperature being about 60° F.

diminution in the refractiveness of the separated elements, and their assumption of a more distinctly vesicular character, whilst they simultaneously acquired a faint brown colour. They were thus converted into unmistakable Fungus-germs, although they showed very little tendency to germinate; and it was not until after repeated examination that a few of them were found growing out into filaments such as are represented in the figure. Occasionally, in the same pellicle, the embryonal areas broke up into products of a somewhat different character (fig. 5, *g*, *g''*). The segments were slightly larger, whilst they gradually assumed a deeper brown colour and a more compound character. These elements also grew at this stage, and underwent processes of division after the fashion of Lichen-gonidia, and in a manner similar to what I had observed on a previous occasion*. These germs also exhibited very little tendency to germinate, though on one or two occasions they were seen to have grown out into short chain-like filaments, such as are represented in fig. 5 *g'*.

During all the period in which the embryonal areas were breaking up into these corpuscles, which soon assumed the form of brown Fungus-germs, not a single Monad or Amœba was to be seen in the solution; and yet it had been standing during the whole time side by side with the other infusion which was prepared at a temperature of 120°–125° F. Facts of this kind have been observed on several other occasions with great constancy; so that one may safely state that Fungus-germs or Monads and Amœbæ may be procured at will, by simply regulating the amount of heat at which the infusion is prepared. The Monad and the Amœba represent more highly animalized modes of existence, which are only able to manifest themselves in infusions in which the organic matter has not been too much deteriorated by the influence of heat. Such deterioration seems to manifest itself by altering the developmental potentialities of the primary forms of living matter evolved in the infusion†.

Experience has shown me that, if an infusion has been heated for a time to 212° F., the pellicle which forms on its surface very frequently never gives rise to an embryonal area. If the infusion has been prepared at a temperature of 149°–158° F., the embryonal areas which form will give origin

* See 'Nature,' 1870, no. 35, p. 173. fig. 3.

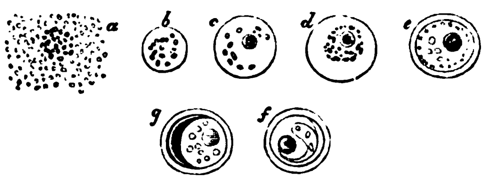
† Seeing that the Monads and the Fungus-germs are produced, not from invisible germs, but from the segmentation of large embryonal areas, every stage of whose formation can be accurately traced, this seems the only possible explanation. If the opponent of Evolution contends, in answer to one set of experiments with heated fluids and closed flasks, that Monads are met with because their germs are capable of resisting a temperature of 275° F., he cannot now contradict himself by saying that embryonal areas, formed on infusions which have been prepared at a temperature of 158° F., do not yield Monads because such a temperature is destructive to their germs. Neither is it open to him to say that Fungus-germs (derived from embryonal areas) do not appear in infusions prepared at 212° F. because such heat is destructive to them, when at the same time he vehemently contends, in answer to other experiments, that similar Fungus-germs are not hindered from developing after exposure to such a temperature, or to others which are much higher.

to Fungus-germs; whilst in a similar infusion prepared at 120°–130° F., the embryonal areas, which seem at first to be in all respects similar, break up into actively-moving Monads. It remains for us to see what changes may take place in the pellicle that forms on an infusion or maceration prepared with cold water (60°–70° F.).

Before passing to a description of these phenomena, however, I will describe the mode of origin of the embryos of some organism whose real nature is unknown, the final stages of its development not having been traced. So far as they were seen, the stages were of a very positive character.

I have observed these early stages in two different infusions; but in each case, after a certain stage of development had been achieved, no further progress seemed to be made for about two days; and then the pellicle unfortunately broke up and sank to the bottom. The arrest of development may therefore have been due in both cases to some morbid quality of the pellicle itself. These organisms were observed in the middle of the month of April (1869), in an infusion of turnip-leaves, which had been prepared fourteen days previously. All stages of development could be seen in different parts of the pellicle. The new organism first manifested itself by the presence (in a uniformly granular layer) of an aggregation of 8–20 larger and more refractive particles; these were then gradually marked off from the surrounding granules by a thin but distinct bounding line, whilst the contained granules increased in size. At a later stage the containing sphere

Fig. 6.—*Mode of Origin and Development of an Embryo of uncertain nature.* (×800.)



was seen to have grown larger (fig. 6, *c*), and the granules had assumed a crescentic arrangement; whilst on their concave side there was a tolerably large refractive globule, about $\frac{1}{12000}''$ in diameter, which exhibited the most distinct oscillations and more or less extensive to-and-fro movements in the otherwise clear central space. In other specimens this central spherule had become even larger, and the granules had closed round it more equally, so as to leave a broad space between the central mass and the thin walls of the containing sphere (*d*). The measurements in this stage were found to be as follows:—containing sphere $\frac{1}{2000}''$, central nuclear-like body $\frac{1}{10000}''$, and surrounding mass of granules $\frac{1}{5000}''$ in diameter. Afterwards the central nuclear-like body and the granular mass seemed to become lighter in colour,—the former still exhibiting its slow oscillating movements; whilst the latter had much increased in size, so as more nearly

to fill the delicate cyst in which it was contained. Then the outlines of the embryo gradually became more defined; three or four other, rather large granules appeared in the neighbourhood of the nucleus; and one crescentic portion of the embryo-mass presented a smooth, glistening, and homogeneous appearance. No later stages were traced; and though no movements of the embryo as a whole were seen (only movements of the nucleus), there could not be the shadow of a doubt that these bodies represented organisms of some kind, which were developing, not from ova, but by means of changes taking place in the very elements of the pellicle itself.

The intermediate connecting links between Flagellated Monads on the one hand, and such Ciliated Infusoria as *Paramecium* and *Kolpoda* on the other, are undoubtedly such forms as those which were included by Dujardin in his genus *Enchelys*. They are scarcely larger than many Monads; they possess the same simple structure, having no trace of an oral aperture, though, like the Monads, they display an internal vacuole, and like them also they may or may not possess a simple nuclear particle. They present the same variations in form which are to be met with amongst Monads; and they differ from them only by the presence of vibratile cilia over most of their body, instead of the possession of a single flagellum. They are, moreover, not unfrequently met with in large numbers in situations in which Monads abound.

Pineau says he has watched the development of organisms of this kind in a pellicle which formed on an infusion of isinglass. The first stages were altogether similar to those which he has described as having taken place in the evolution of *Monas lens**. Corpuscles were seen to separate from the embryonic aggregations without a flagellum, though they continued to increase in size, and soon developed a vacuole and nuclear particle in their interior. As they enlarged, they gradually assumed an oval form, though still remaining motionless and devoid of cilia. At last, with very little further increase in size, cilia were developed†, and the organisms gradually displayed the appearance and locomotory powers which have been attributed by Dujardin to the form which he named *Enchelys ovata*‡.

The organisms previously mentioned have nearly all been minute; and it has therefore been somewhat difficult to trace their early stages. These difficulties, however, gradually vanish when we come to the investigation

* Ann. des Sc. Nat. 1845 (Zool.), p. 183.

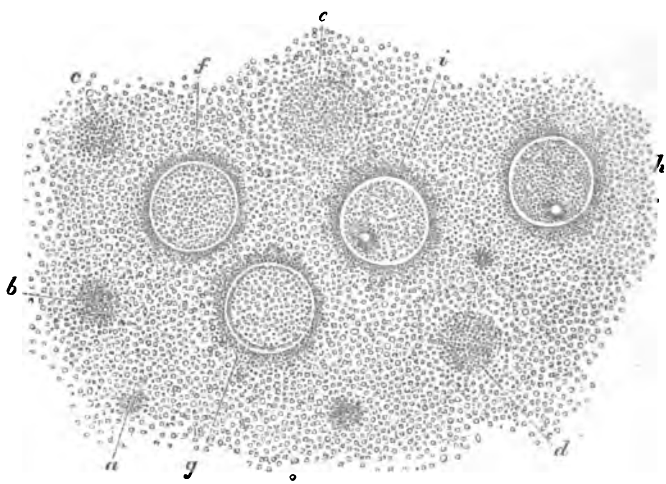
† The apparition of cilia is known to be quite sudden in the development of the spore of *Vaucheria*, and also to be sudden during the development of some Infusoria, as Cienkowski and others have observed.

‡ To another similar solution of isinglass M. Pineau, mindful of the results recorded by Dutrochet, added a few drops of vinegar; and he says:—"Il ne s'y développa un seul animalcule: mais en revanche elle se couvrit, comme je m'y attendais, d'un forêt de moisissures."

of the mode of origin of such larger organisms as *Paramecium* and *Kolpoda*. Although their most remarkable mode of origin was fully described and figured by M. Pouchet more than twelve years ago, yet, unfortunately, many of our leading biologists have preferred to repudiate his statements and rely upon their own notions concerning credibility and the mode in which living matter ought to conduct itself, rather than adequately investigate the subject for themselves.

According to Pouchet, the stages in the evolution of *Paramecium viride* were as follows :—The pellicle, which was at first uniform and evenly gra-

Fig. 7.—*Mode of Origin of* Paramecia *from the Pellicle*: after Pouchet. (×400.)



nular, after a short time changed in aspect here and there, owing to a concentration of its granules at tolerably equal distances into small, more or less rounded aggregations, which soon became surrounded and defined by a clear border, suggestive of a resemblance to the *zona pellucida* of higher animals. The next change which took place was, that the granules, which had been at first more densely aggregated towards the centre, disseminated themselves uniformly through the ovum, whilst at the same time the simple clear zone thickened into a distinct membrane. At this stage the whole egg appeared somewhat lighter and more transparent than the surrounding pellicle. Soon after this, differentiation still proceeding, the mass of enclosed granules gradually became converted into a real embryo, which manifested its existence by slow movements—at first by simple oscillations in the mass of granules, and afterwards by regular uniform movements of revolution of the whole contents within its enveloping membrane, similar to those of many other embryos. The slightest shock at this stage immediately arrested the gyration. Then, after a time, a pale spot appeared

amongst the granules in some part of the embryo, the alternate contraction and dilatation of which soon showed that it was the contractile vesicle of the infusorium. After a time the embryo began to exhibit movements of quite a different kind (sudden and irregular), no longer checked, but rather increased by slight shocks from without. In one of these sudden plunges the thin enveloping membrane was ruptured, and there entered into the aquatic world a free-swimming and ciliated infusorial animalcule having the characteristics of the species above mentioned.

Such is the marvellous story; and the description of other observers is substantially similar. In the particular observation of which M. Pouchet gives the details *, the first rudiments of the eggs began to make their appearance in the pellicle of an infusion of hay on the second day; on the third day the ovules were distinctly circumscribed, spherical, and $\frac{1}{800}''$ in diameter; on the fourth day there was no increase of size, the investing membrane could scarcely be recognized—although there was a distinct gyration of the embryo within it, and in those which were most advanced the contractile vesicle could already be discovered; on the fifth day the embryos were found to be of the same size, though slightly greenish in colour, and their movements were more irregular and jerking. At this stage the animalcule had assumed a pyriform shape, fine cilia could be seen on some parts of its surface, and the contractile vesicle was most obvious in the midst of minute and densely packed greenish granules. After a few hours more, the buccal cleft, fringed with longer cilia, became obvious, and also the so-called nucleus in the centre of the body. The embryos had by this time somewhat increased in size, so that, after an interval of a few more hours, fully developed specimens of *Paramecium viride*, $\frac{1}{450}''$ in diameter, were swimming about in the solution.

These observations of M. Pouchet have been repeated by him over and over again. He has thus seen different forms of *Paramecium* arise in the pellicle; and at other times, by steps essentially similar, *Kolpodæ* have made their appearance. The difference between these two forms is indeed quite trivial and unimportant, and wholly unworthy, even from the old point of view, of being regarded as a generic mark of distinction.

These observations of M. Pouchet have been confirmed by MM. Joly and Musset, M. Pennetier, and others. The former observers declare† that they have watched the evolution of specimens of *Kolpoda cucullus* in a pellicle that formed on water in which the contents of a hen's egg were allowed to macerate. In this pellicle there appeared, as they say, "en vertu d'une sorte de cristallisation vitale," the spherical masses of granules constituting "les œufs spontanés" of Pouchet; and these in their turn, after a period in which the usual rotation of the embryos within the egg-membrane was observed, gave origin to specimens of the organism above mentioned. On the removal of the first pellicle, it was succeeded by another, in which similar developmental phenomena were repeated.

* 'Hétérogénie,' p. 394.

† See Compt. Rend. 1860, t. li. p. 934.

I have also myself, quite recently, watched with the greatest interest all the stages of such a process, terminating in the evolution of fine specimens of *Paramecium*, and am most pleased to be able to bear my testimony to the general accuracy of M. Pouchet's description. Up to this period I had never seen a single *Paramecium* or other specimen of the larger ciliated Infusoria in any of my hay-infusions, these having all been prepared either with warm or with hot water. But about ten days previously, on re-reading M. Pouchet's description of the mode of evolution of these organisms, it struck me that I had failed to see these phenomena, owing to my never having made any infusions with cold water. I therefore at once prepared such a maceration, and two or three days afterwards wrote to M. Pouchet on the subject. In the reply which he was kind enough to address to me, he said:—" Jamais, jamais vous ne renconterez *un seul* infusoire cilié dans une expérience faite à l'eau chaude..... Il faut pour cela opérer sur des macérations faites à froid ; alors vous obtiendrez facilement le phénomène de développement des œufs spontanés des Paramécies, dans les membranes proligères qui se seront formées d'abord "*.

On the evening of the day on which I received this letter, I again examined the thick pellicle which had formed on the maceration of hay; and, much to my delight, I found it studded with thousands of embryo *Paramecia*, whilst others were free and active in the infusion. It was, therefore, a most significant fact that they should have been met with on the very first occasion that a cold maceration had been employed† ; whilst not a single *Paramecium* had ever been seen before in any of the many hay-infusions kept in the same place‡, although several of them had even been made with water whose temperature was not more than 125°–130° F.— and, therefore, not high enough to have killed any embryos which might have chanced to preexist in the infusion previously to its filtration.

The maceration was at the time covered by a thick pellicle, which had become brown on its upper surface. Its under layers, however, were still soft and pulpy. When a small portion of it was transferred to a microscope-slip, and gently compressed by the covering-glass so as to flatten it out into a thinner layer, the granular membrane was observed to be pretty

* M. Pouchet has been in the habit of using one part by weight of ordinary dry hay to about forty parts of water, and then allowing the maceration to stand for two or three hours before filtering off the clear liquid.

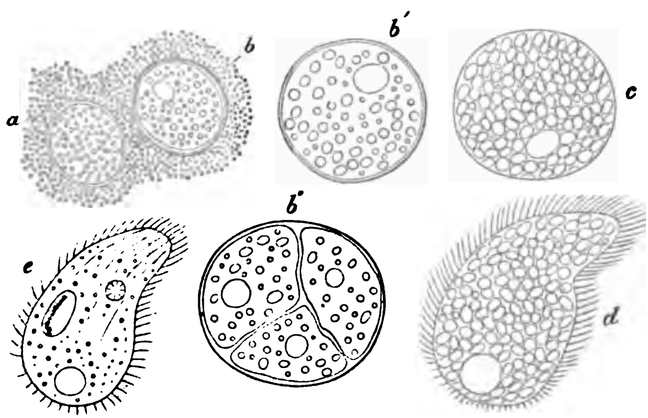
† Owing to the coldness of the weather (the daily temperature of the room being scarcely above 60° F.), they did not make their appearance in the pellicle till more than fourteen days, although with a daily temperature of 75° F. they are said by M. Pouchet to begin to make their appearance on the third or fourth day. I had examined the pellicle of my maceration from time to time during the first week, but did not look at it subsequently for several days—not, in fact, until the day on which I received M. Pouchet's letter. During the first week the pellicle had become very thick and pulpy, but, the weather being rather colder at this time, it was principally giving birth to various kinds of Fungus-germs.

‡ Beneath a bell-jar in my study.

thickly studded with the most distinct egg-like bodies, varying in size from $\frac{1}{800}''-\frac{1}{350}''$ in diameter. What struck me more than any thing was the extreme distinctness with which almost all the phenomena described by M. Pouchet were to be seen. There could be little room for doubt with such objects before one.

The only difficulty was to make out exactly what was the nature of the first change by which the egg-like body became differentiated from the surrounding substance of the pellicle. I laboured under some disadvantages from having to examine an old and somewhat opaque pellicle; but after the most careful and repeated observations with reference to this point, I have been led to adopt an opinion slightly different from that of M. Pouchet. Instead of small concentrations of granules occurring which gradually increased in size and at last became enclosed by a bounding membrane, it seemed to me that the differentiation took place after a manner essentially similar to that by which an ordinary "embryonal area" is formed*. The small embryos did not appear to represent the earlier stages of large embryos; and it seemed rather that spherical masses of the pellicle of different sizes began to undergo molecular changes, which terminated in the production of *Paramecia* of a correspondingly different bulk.

Fig. 8.—*Mode of Origin of* Paramecia. (×800.)



a. First stage of differentiation.
b. Later stage, in which a vacuole has appeared.
b′. Similar stage of much larger embryo.
b″. Another embryo, which has segmented into four (only three parts visible).
c. Later stage: embryo filled with large particles and revolving within its cyst.
d. *Paramecium* after it emerges from its cyst.
e. *Nassula*-like form into which many afterwards passed.

* On the other hand, the embryos of unknown organisms which were seen to form in the infusion of turnip-leaves did seem to develop in a manner remarkably similar to the embryos of *Paramecium viride*, as described by M. Pouchet.

Just as in the previously described embryonal areas masses of different sizes began to exhibit signs of change, so also here spherical portions of the pellicle, differing within the limits above mentioned, began to undergo other heterogenetic changes. This was first indicated by an increased refractiveness of the area (especially when seen a little beyond the focal distance); and almost simultaneously a condensation of its outer layer appeared to take place, whereby the outline became sharply and evenly defined*. At this stage an actual membrane was scarcely appreciable, and the substance of the embryo (when examined at the right focal distance) scarcely differed in appearance from the granular pellicle of which it previously formed part.

So far as it could be ascertained, the individual embryos did not increase in size, although they went through the following series of developmental changes. The contained matter became rather more refractive, and the number of granules within diminished considerably, whilst new particles after a time seemed gradually to appear in what was now a mass of contractile protoplasm. These new particles were at first sparingly scattered, though as they were evolved they continued to grow into biscuit-shaped bodies, which sometimes attained the size of $\frac{1}{10000}''$. All sizes were distinguishable; and many of them moved slowly amongst one another, owing to the irregular contractions of the semifluid protoplasm in which they were imbedded. Gradually the number of homogeneous biscuit-shaped particles increased, and at last a large vacuole slowly appeared in some portion of the embryo (fig. 8, *b*); it lasted for about half a minute, disappeared, and then, after a similar interval, slowly reappeared. Much irregularity, however, was observed in this respect. The next change that occurred was the complete separation of the embryo from the cyst which it filled, and the commencement of slow axial rotations. These rotations gradually became more rapid, though they were not always in one direction. The embryo became more and more densely filled with the large biscuit-shaped particles (*c*); and at last the presence of cilia could be distinctly recognized on one portion of the revolving embryo. Then, as M. Pouchet stated, the movements grew more and more irregular and impulsive, so that after a time the thin wall of the cyst was ruptured, and the embryo emerged as a ciliated and somewhat pear-shaped sac provided with a large contractile vesicle at its posterior extremity.

Sometimes the embryo-mass at an early stage of its evolution divided into two or four bodies, each of which developed within the cyst into a perfect embryo; and in place of exhibiting a regular rotation, they rolled and tumbled over one another in the most irregular manner. On one occasion I saw a cyst containing two embryos and four spherical Monads about $\frac{1}{5000}''$ in diameter, the latter having apparently resulted from the

* The first changes seem to take place rather rapidly, judging from the great difficulty of recognizing the earlier stages. It was almost impossible to find an area which was not already bounded by a delicate outer layer.

fission of some smaller portion of the embryo-mass. Sometimes it was the largest embryos which were observed to undergo this process of fission, though the phenomenon was by no means confined to them *.

On emerging from the cyst, all the embryos, although differing somewhat in size, were of the same shape. This closely corresponded with the description given of *Paramecium kolpoda* in Pritchard's 'Infusoria,' namely:—"Ovate, slightly compressed; ends obtuse, the anterior attenuated and slightly bent like a hook." Cilia existed over the whole body, though they were largest and most numerous about the anterior extremity. No trace of an actual buccal cleft could be detected; and (except in the posterior portion of the body, where a large and very persistent vacuole was situated) the organism was everywhere densely packed with the large, homogeneous, biscuit-shaped particles. For many days these most active Infusoria seemed to undergo little change, though afterwards the number of the contained particles gradually began to diminish, whilst the body became more and more regularly ovoid, and a faint appearance of longitudinal striation manifested itself, more especially over its anterior half. At the same time a very faint and almost imperceptible mass ("nucleus") began to appear near the centre of the organism; and when examined with a magnifying power of 1670 diameters, a lateral aperture (mouth) $\frac{1}{8000}''$ in diameter was seen, which was fringed by short active cilia, arranged like the spokes of a wheel. These peculiarities correspond very closely with those of an embryo *Nassula*. Very many were seen with similar characters; and multitudes existed in all conditions intermediate between this stage and that of the simpler organism which first emerged from the cyst. No further stages, however, could be watched, as at this time some change took place in the infusion which proved fatal to all the free Infusoria and also to the multitudes of embryos which were at the time developing in the pellicle; these became more minutely granular and opaque, their movements ceased, and the cyst-wall grew thicker. This phase of development disappeared, therefore, almost as suddenly and mysteriously as it had appeared. The cysts were examined from time to time for many weeks afterwards, but they seemed to undergo no further change †.

It will, of course, be seen that the phenomena which I have described

* Partial desiccation has a strong tendency to induce such fission, as I found by the frequency with which it occurred when the water had in great part evaporated from specimens placed in a developmental chamber. Fission of *Penicillium*-filaments (into conidia), and also of encysted *Euglenæ*, have several times been seen under similar circumstances.

† In a maceration which was subsequently made during very cold weather, when the temperature of the room, even during the day, was rarely higher than 53° F., large Amœbæ, visible to the naked eye, some of which were $\frac{1}{150}''$ in diameter, were produced from the pulpy under portions of the pellicle. They formed great masses of living granular jelly of the simplest description—too large to move as a whole, though fluxes of portions of their semifluid body-substance were continually taking place in different directions.

as taking place in the "proligerous pellicle" may be watched by all who are conversant with such methods of investigation. We do not require to call in the aid of the chemist; we need exercise no special precautions; the changes in the pellicle are of such a kind that they can be readily appreciated by any skilled microscopist.

Just as I have supposed that living matter itself comes into being by virtue of combinations and rearrangements taking place amongst invisible colloidal molecules*, so now does the study of the changes in the "pellicle" absolutely demonstrate the fact that the visible new-born units of living matter behave in the manner which I have attributed to the invisible colloidal molecules. The living units combine, they undergo molecular rearrangements, and the result of such a process of heterogenetic biocrasis is the appearance of larger and more complex organisms; just as the result of the combination and rearrangement between the colloidal molecules was the appearance of primordial aggregates of living matter. Living matter is formed, therefore, after a process which is essentially similar to the mode by which higher organisms are derived from lower organisms in the pellicle on an organic infusion. All the steps in the latter process can be watched; it is one of synthesis—a merging of lower individualities into a higher individuality. And although such a process has been previously almost ignored in the world of living matter, it is no less real than when it takes place amongst the simpler elements of not-living matter. In both cases the phenomena are essentially dependent upon the "properties" or "inherent tendencies" of the matter which displays them.

The Society then adjourned over the Easter Recess to Thursday, April 11.

Presents received March 7, 1872.

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"On the Elimination of Alcohol." By A. Dupré, Ph.D., Lecturer on Chemistry at Westminster Hospital. Communicated by William Odling, M.B., F.R.S. Received November 16, 1871*.

Previous to the year 1860 it was the generally received opinion that the greater portion of any alcohol taken was oxidized in the system, and only a small fraction eliminated unaltered. In that year, however, Messrs. Perrin and Lallemand published an elaborate memoir on the subject, in which they maintained that all, or at least nearly all, the alcohol taken is eliminated unaltered. This opinion was soon very generally adopted, notwithstanding the fact that Messrs. Perrin and Lallemand never succeeded in recovering, from the excretions, more than a very small fraction of the alcohol consumed, although very large doses were frequently given. However, the missing alcohol was easily accounted for as loss, occasioned by its ready volatility.

Soon after Dr. Anstie took up the subject, and, on the strength of numerous qualitative experiments, arrived at the conclusion, which he was the first to publish in this country, that the originally received opinion was correct, viz. that a small portion only of any alcohol taken is eliminated unaltered. After this, Dr. Thudichum and the author in this country, and Ichulinus abroad, undertook a number of quantitative experiments which proved that a minute fraction only of the alcohol taken is eliminated through the kidneys. Owing to these researches, general opinion gradually reverted to the original notion.

Quite recently, however, Drs. Parkes and Wollowicz have published several valuable memoirs on the action of brandy, alcohol, and wine on the human body, in which they adopt, at least partially, the views of Messrs. Perrin and Lallemand. But here also no quantitative results are given which will account, even approximately, for the amount of alcohol supposed to be eliminated. Drs. Parkes and Wollowicz believe that the elimination of alcohol may go on for 5 or 6 days after the last dose of alcohol has been taken; and they express the opinion that Dr. Anstie and the author, by assuming that elimination practically ceases after 24 hours, had considerably underestimated the amount actually eliminated. The author has thereby been induced once more to investigate this subject.

Assuming, then, for the sake of argument, that all the alcohol is eliminated, and that such elimination takes 10 days†, it would follow that if a certain quantity of alcohol be taken daily, the amount eliminated would increase from day to day until, from the 10th day onward, the quantity

* Read January 25, 1872. (For abstract see p. 107.)

† This seems the very utmost limit that can be conceded. In the numerous experiments of Dr. Bence Jones and the author, on the passage of substances into and out of the tissues, it was found that the elimination of lithium, for example, was complete in 5 to 6 days, while all elimination of quinine ceased after 2 days. In neither case did any of the substance remain behind.

eliminated daily would equal the daily consumption; in other words, the quantities which would be eliminated, if this theory were correct, might be measured by ounces instead of by grains, and even the most ordinary processes of analysis could not fail to yield considerable quantities of alcohol. The argument loses nothing in force if it be contended that elimination lasts longer than 10 days; for, however long it may last, if the alcohol diet is continued, a time must come when elimination and consumption would be equal. Obviously, however, there is a tolerably narrow limit given to the time during which elimination can be presumed to last; for the alcohol, if not eliminated, must accumulate in the body, and would speedily rise to a proportion totally incompatible with life itself. The experiments recorded in the following pages were guided by the foregoing considerations.

Analytical processes employed.—Since, in the experiments of Messrs. Perrin and Lallemand, the alcohol not recovered is accounted for as loss, the author has thought it of importance to give the analytical processes employed in greater detail than might otherwise be considered necessary. It will thus be shown how much of the alcohol present can be recovered, and how much may reasonably be accounted for as loss. Alcohol is eliminated through four channels,—kidneys, lungs, skin, and bowel.

Examination of the Urine.—The urine to be examined is rendered slightly acid by dilute sulphuric acid; some tannin is added to prevent frothing, and about $\frac{1}{3}$ of it is distilled over. This first distillate is now made slightly alkaline by caustic potash or soda, and redistilled; this second distillate is once more acidified by sulphuric acid, and again distilled, $\frac{1}{3}$ being driven over each time. If these three distillations reduce the amount of liquid to about 20 cub. centims., it may at once be submitted to oxidation. If, however, the amount of urine employed was so large that this third distillate amounts to much more than 20 cub. centims., the redistillation must be repeated until the quantity is brought down to 20 cub. centims. Less than three distillations should, however, in no case be made. All distillations should, moreover, be conducted with an apparatus in which both retort and receiver are connected air-tight with the condenser, the receiver being furnished with a safety-tube closed by a globule of mercury. The final distillate is now put into a small assay flask, a suitable amount of bichromate solution is added, the flask is closed by a well-fitting caoutchouc-stopper tied down, and heated for one hour in a water-bath. The flask is then taken out, cooled, opened, and the excess of bichromate left is reduced by zinc. (The bichromate solution is made by dissolving 147 grammes of potassium bichromate in water, adding 200 grammes of strong sulphuric acid, and making up the solution to 1400 cub. centims. Ten cub. centims. of this solution will oxidize nearly 0·2 gramme alcohol into acetic acid. After a little practice, it is easy to judge by the colour of the liquid in the flask whether or no sufficient bichromate had been added.) When all the bichromate is reduced, the green solution

is transferred to a small retort, some sulphuric acid is added, and the acetic acid present is distilled over. To do so effectually, the distillation is continued almost to dryness, some water is then added to the residue, and the distillation resumed; this is repeated three times. The distillation may be conducted over an Argand burner, and sometimes it is advisable to place pieces of tobacco-pipe into the retort to avoid bumping. In the distillate the acetic acid is now estimated by a standard solution of decinormal soda, 1 cub. centim. of which neutralizes 0·006 grm. acetic acid, and indicates therefore 0·0046 grm. of alcohol. If ordinary care has been used, no trace of sulphuric acid will be present in the distillate; but if the distillation has been carried too far, so that the residue in the retort has become dry, traces of sulphuric acid may have passed into the distillate. In such case the acidity of the distillate is carefully estimated, and the neutral solution is evaporated to dryness on a water-bath. To the dry residue an amount of deci- normal sulphuric acid is added slightly in excess of the quantity of deci- normal soda used, and the resulting solution is once more evaporated on the water-bath. During this second evaporation all the acetic acid is driven off. The acidity of the residue is now determined as before, and will be found equal only to the excess of deci- normal acid taken, in case nothing but acetic acid (or other volatile acid) was present in the distillate. If, however, any sulphuric acid had passed over, the acidity of this residue will be found greater than the excess of deci- normal acid added, and this surplus is the measure of the sulphuric acid contained in the distillate. If this then is subtracted from the total acidity originally found, the rest will be the acetic acid. If an appreciable amount of volatile acid is present, which to a great extent may be judged of by the colour of the bichromate solution after the heating, the disturbing effect of any sulphuric acid having passed over may also be neutralized in the following manner. The distillate is accurately divided into two equal parts; the acidity of the one half is estimated directly, the other half is evaporated on a water-bath, and the acidity of the residue left is determined. The difference between these two determinations will give the volatile acid present in one half of the distillate. Lastly, the acetic acid may be estimated by neutralizing the distillate by pure barium carbonate, filtering, washing, &c., evaporating the solution to dryness, and weighing the barium acetate left. The above process yields accurate results, even with very small quantities of alcohol; thus in two check experiments the amount of alcohol taken was 0·1 and 0·025 gramme, the acetic acid obtained neutralized 20·1 cub. centims. and 5·5 cub. centims. of deci- normal soda, corresponding respectively to 0·0924 and 0·0253 gramme of alcohol.

Examination of Breath.—By help of a suitable mouthpiece the breath is blown, through a wide tube containing chloride of calcium, into a bag placed between light-pressure boards (such a bag as is used in the administration of laughing-gas, and holding from 4 to 5 cubic feet). From this bag it passes into a flask containing water, thence into a Liebig's con-

denser in connexion with a receiver, and finally escapes into the air. The experiment is conducted as follows:—The bag being empty, the water in the flask is heated to boiling, and the breath is blown into the apparatus for a quarter to half an hour. The breath is first deprived of most of its moisture by the chloride of calcium, and next enters the bag dry, or nearly so. From the bag it passes, at considerably reduced speed, through the flask, where it becomes mixed with steam, into the condenser. In this last the steam is condensed, and with it the greater part of the alcohol vapours present. The alcohol will therefore be found in the distilled water collected in the receiver. When the breath has been blown into the apparatus during the desired length of time, the blowing is discontinued, the tube is closed by a clamp, and the air collected in the bag is allowed to pass gradually through the retort and condenser. The chloride of calcium is now dissolved in water, the solution is added to the distillate collected, and the alcohol therein contained is estimated by repeated distillations, oxidation, &c., as described in the case of urine. If it is desired to blow for a greater length of time than half an hour, it is necessary, with a bag of the above size, to blow it up twice, each time of course allowing all the air collected to pass out through the retort and condenser. At first it is difficult to keep up the ordinary rate of respiration while blowing into the bag; but with a little practice this becomes easy, and a bag holding about 4 cubic feet will then suffice for half an hour's breathing. At the end of this time about 4 cubic feet of air should be in the bag, while 2 cubic feet have passed through the apparatus; the bag will then take one hour more to become empty. The bag has thus to serve both as a reservoir and regulator, reducing the velocity of the air-current to about one third. The desired velocity is readily obtained by a proper adjustment of the weights on the pressure-boards. The boiling of the water in the flask is regulated so as to give about half a litre distilled water for every 12 cubic feet of air passing through. The breath was blown through the apparatus for half an hour. The following quantitative experiments, made under precisely the same conditions as the experiments with the breath, will serve to show that the greater part of the alcohol carried by the air is condensed with the steam. A given quantity of alcohol was evaporated in a current of air, which was afterwards passed through the apparatus at the same speed as the breath. In the distillate obtained, the alcohol was estimated as described. The amount of air taken was 12 cubic feet, equivalent to about one hour's breathing, and necessitated the filling of the bag twice.

First experiment.—Amount of alcohol evaporated 0·004 gramme; acetic acid obtained neutralized 0·67 cub. centim. d. n. soda, equivalent to 0·031 gramme alcohol.

Second experiment.—Amount of alcohol evaporated 0·008 grm.; acetic acid obtained neutralized 1·25 cub. centim. d. n. soda, equivalent to 0·0058 grm. alcohol.

Third experiment.—Amount of alcohol evaporated 0·0415 grm.; acetic

acid obtained neutralized 6·05 cub. centim. d. n. soda, equivalent to 0·0278 grm. alcohol.

In the first two experiments $\frac{3}{4}$, in the last $\frac{2}{3}$ of the alcohol contained in the 12 cubic feet of air was recovered in the distillate. In a blank experiment, in which air only was blown through the apparatus, no trace of volatile acid was obtained.

Examination of Alvine Discharges.—These are stirred up with water, the mixture is distilled &c., and the alcohol in the final distillate is estimated as described. In the experiments recorded in the following Tables, the alvine discharges were not examined, previous experiments having convinced the author that, even in cases where very large quantities of brandy are taken for some length of time, the amount of alcohol eliminated by the bowel is extremely small.

Examination of Cutaneous Exudations.—No attempt was made to estimate the amount of alcohol eliminated by the skin. Qualitatively, however, the fact that traces of alcohol are thus eliminated may be shown as follows:—Part of the body is enclosed for several hours in an air-tight covering; at the end of that time the perspiration collected is washed off with clean water, the distillate from which is then tested with bichromate and strong sulphuric acid. Dr. Anstie has made numerous experiments in this manner, which show conclusively that the amount so eliminated is always extremely minute. The actual amount eliminated might perhaps be estimated, with tolerable exactness, by enclosing an ascertained fraction of the entire surface of the body in an air-tight bag, through which a current of dry air is passed. In this air the alcohol is then determined exactly as in the case of the breath.

By means of the method just described, two series of experiments on the elimination of alcohol were made, and, for the sake of absolute certainty, the author conducted them in his own person.

First Series.—Having first abstained absolutely, for a space of 10 days, from all alcoholic drinks or other articles of food containing alcohol, the urine was collected on the 11th day, and the breath blown through the apparatus for half an hour. On the 12th day, and on each of the twelve succeeding days, 112 cub. centims. of brandy* were taken daily (28 cub. centims. at 1 P.M., 56 cub. centims. at 6 P.M., and 28 cub. centims. at 11 P.M.). The urine was collected between the hours of 3 P.M. one day and 3 P.M. the following day, on 1st, 6th, and 12th day of the brandy diet, and 4 P.M. on the same above-mentioned days. Lastly, the urine was collected during the 5 days following the cessation of the brandy diet. The analytical results obtained are arranged in the following Table.

* The brandy contained 43·47 per cent. by weight of absolute alcohol.

TABLE I.—Left off taking alcohol February 26, 1871; first cub. centim. of brandy taken March 8th at 6 P.M.

Date.	Cubic centimetre deci-normal soda neutralized by acetic acid obtained from		Amount of alcohol, corresponding to this acid, discharged in 24 hours, in grammes, through		Amount of absolute alcohol taken in the 24 hours.
	½ hour's breath.	24 hours' urine.	Breath.	Urine.	
March 8	0·03	0·67	0·0083	0·0031	None.
„ 9	0·05	9·44	0·0138	0·0434	52·16
„ 14	0·05	7·80	0·0138	0·0359	52·16
„ 20	0·04	5·00	0·0110	0·0230	52·16
„ 21	......	0·64	....	0·0029	None.
„ 22	......	0·29	....	0·0013	„
„ 23	......	0·40	....	0·0018	„
„ 24	......	0·50	....	0·0023	„
„ 25	......	0·45	....	0·0021	„

Total amount of absolute alcohol taken during the twelve days 625·92 grms.

Total amount of absolute alcohol discharged by the kidneys during the same twelve days, 0·3984 grm., taking the daily elimination at 0·0332 grm., the mean of that on the 1st and 12th day.

Total amount of absolute alcohol eliminated by the lungs, taking the amount discharged at 3 P.M. on the 14th as representing the mean elimination during the day, and adding ⅓ for loss, as shown by the control experiments, 0·2064 grm.

TABLE II.—56 cub. centims. brandy (26·08 grms. absolute alcohol) taken 10 A.M. March 29th.

Period of elimination.	Alcohol eliminated by breath during 1 hour		Alcohol eliminated by breath during entire period.	Quantity of urine discharged in cub. centims.	Alcohol eliminated in the urine.	
	Yielded $\overline{A}$, which neutralized cub. centim. deci-normal soda.	Equivalent to grammes of alcohol.			$\overline{A}$ obtained neutralized cub. centim. deci-normal soda.	Equivalent to grammes of alcohol.
First 3 hours.........	6·9	0·03174	0·09522	570	36·36	0·16720
Second 3 hours	0·3	·00138	·00414	92	·63	·00290
Third 3 hours	0·25	·00115	·00345	180	·45	·00207
Fourth 3 hours	......	....	....	120	·05	·00023
Next 12 hours	.. ...	....	....	350	·36	·00166
1st day following....	0·25	·00115	·02760	900	·40	·00184
2nd day following...	0·25	·00115	·02760	1050	·46	·00212

Second series.—Discontinued the use of alcohol, in any shape, on March the 20th. On March the 29th, at 10 A.M., took 56 cub. centims. brandy (same brandy as in previous experiments). Urine collected for every 3 hours up to the 12th, from the 12th to the 24th hour, and during the next succeeding 2 days. The breath was passed through the apparatus, for ten minutes at a time, in every half hour during the first 9 hours, and during 1 hour (between 2 and 4 P.M.) on the 2 days following. The results are arranged in Table II. p. 273.

Total amount of absolute alcohol eliminated through the kidney during the 3 days 0·1780 grm.; more than $\frac{8}{10}$ of this amount was eliminated during the first 3 hours.

Total amount of absolute alcohol eliminated through the lungs during the 3 days (adding $\frac{1}{3}$ for loss) 0·2336 grm. In both cases all the volatile acid obtained during the 3 days is calculated as alcohol.

An examination of Table I. shows that, even after 10 days' total abstinence, a substance is eliminated by the kidneys, and apparently also in the breath, which, when distilled and oxidized, yields a volatile acid (the acid has the smell of acetic acid). An opportunity was therefore taken to examine the urine of a gentleman, a teetotaler, who had only once in his life, and that two years previously, taken some spirituous liquor. On treating this urine in the usual manner, for the detection and estimation of alcohols, an amount of volatile acid was obtained from 1 day's urine which neutralized 0·5 cub. centim. deci- normal soda. The experiment was twice repeated with different days' urine with the same result. The smell of the volatile acid in this case also was that of acetic acid. We must therefore look upon this substance, whatever it may be, which yields the volatile acid as a normal constituent of urine. The elimination of alcohol must, then, be considered at an end as soon as the proportion of volatile acid obtained sinks to the normal amount.

Leaving, then, the nature of this substance out of consideration for the present, we arrive at the following conclusions:—

1st. The amount of alcohol eliminated per day does not increase with the continuance of the alcohol diet; therefore all the alcohol consumed daily must of necessity be disposed of daily; and as it certainly is not eliminated within that time, it must be destroyed in the system.

2nd. The elimination of alcohol following a dose or doses of alcohol is completed 24 hours after the last dose has been taken.

3rd. The amount of alcohol eliminated, in both breath and urine, is a minute fraction only of the amount of alcohol taken*.

A consideration of Table II. leads substantially to the same conclusions. Here, a single dose having been taken, elimination had ceased to be per-

* Quite recently I have examined the urine of a woman suffering from ascites, who at the time of the experiment took 12 ounces of brandy (38 per cent. by weight of absolute alcohol) daily, and had done so during a period of six weeks. Two days' urine yielded 0·0366 grm. of acetic acid, equivalent to 0·02806 grm., or 0·44 grain alcohol.

ceptible; that is, the amount of volatile acid yielded on oxidation had sunk to the normal amount 9 hours after the dose had been taken. The proportion of alcohol eliminated in this second experiment, although still small, is, however, considerably higher than it was in the first; but this is most likely owing to the different conditions under which the experiment was made. The two ounces of brandy were taken within a very short space of time and early in the morning, no other food being taken at the same time. In consequence of this, the brandy had a considerable diuretic effect during the first few hours, within which, as will be seen, more than $\frac{9}{10}$ of the total proportion was eliminated.

It has been shown in the foregoing that urine, even after 10 days of total abstinence, when treated as for the estimation of alcohol, yields some volatile acid which, as judged by the smell, is acetic acid. A similar substance was also found in the urine of a teetotaler; and a preliminary experiment having shown that at least the greater part of this substance passed over with the first portions of distillate, a somewhat larger quantity of the same urine was obtained and examined. The total quantity employed amounted to 180 ounces, being the greater part of 10 days' urine. To avoid decomposition, the daily portion of urine was at once acidified slightly, and $\frac{1}{3}$ of it distilled; this distillate was rendered alkaline and redistilled. At the end of the 8 days, all these distillates were mixed, acidified, and again distilled. This third portion was now twice distilled over freshly ignited animal charcoal, after which the distillations were repeated until the quantity of liquid was reduced to 10 cub. centims., care being taken that never less than $\frac{1}{3}$ was driven over. All the distillations were, moreover, conducted with the usual precautions of having the receiver closed by a mercury valve. These 10 cub. centims. showed the following properties:—

Specific gravity at 15°·5 C., 0·9996 water at the same temperature taken as unity. Vapour-tension in Geissler's vaporimeter equivalent to 0·88 per cent. by weight of alcohol.

3·593 of it, when oxidized by bichromate &c., yielded an acid distillate which, when neutralized by barium carbonate, filtered and evaporated, gave 0·0192 grm. barium salt; this barium salt, on decomposition with sulphuric acid, gave 0·0176 grm. of barium sulphate, and contained therefore 53·88 per cent. of barium; pure barium acetate contains 53·72 per cent. barium. The acid vapours expelled had the smell of acetic acid. Another portion readily gave the emerald-green reaction with bichromate and strong sulphuric acid, and finally they readily gave the iodoform test, viz. when treated with iodine and an alkali, a yellow glittering precipitate was produced, which, under the microscope, consisted of golden-coloured six-sided plates, sometimes single, sometimes united into stars in the manner of snow-crystals.

The author having again abstained from the use of alcohol since May the 16th, the urine was collected from May the 29th to June the 10th

(with the exception of June the 4th and 5th), amounting altogether during the 10 days to 360 oz. This urine was treated exactly as the previous sample, and the amount of distillate finally collected was also 10 cub. centims; these 10 cub. centims. possessed a specific gravity of 0·9988 at 15°·5 C.

In Geissler's vaporimeter they showed a vapour-tension equivalent to 1·7 per cent. by weight of alcohol; 3·588 grms. of it, when oxidized &c., gave 0·0307 grm. barium salt, yielding 0·0278 grm. barium sulphate, and contained therefore 53·24 per cent. of barium: here also the smell of the escaping acetic acid was unmistakable. The volatile acid obtained from another portion of these 10 cub. centims. gave with ferric chloride distinctly, though but feebly, the well-known reaction of acetic acid. Finally, they gave readily the iodoform test, as well as the green reaction, with bichromate and strong sulphuric acid.

On June the 22nd and 23rd the urine was again collected and examined, no alcohol having been taken since May the 16th. The urine of the 22nd yielded an amount of acid neutralizing 0·53 cub. centim. deci- normal soda, the volatile acid produced from the distillate of the urine on the 23rd neutralizing 0·55 cub. centim. of the same soda. Lastly, the urine was collected on June the 26th, 27th, and 28th, no alcohol whatever having been taken since May the 16th. The urine was repeatedly distilled, as usual, the final distillate amounting to 5 cub. centims.; these 5 cub. centims. readily gave the iodoform test, as well as the green reaction, with bichromate and strong sulphuric acid.

It appears, therefore, that a substance is found in the urine after six weeks' total abstinence, and even after an abstinence of two years, which gives the reactions ordinarily employed for the detection of small quantities of alcohol. Since it is impossible to assume that any elimination of alcohol, due to alcohol which has been taken, could go on for a period of six weeks, not to speak of two years, we must conclude that this substance is a normal constituent of human urine, or at least may be obtained from it by distillation with dilute acid &c. At first the author inclined to the belief that this substance is actually ethylic alcohol, although the very small quantities dealt with did not allow of its separation. The final distillate obtained is, however, evidently a mixture; and it would therefore be unsafe to rely solely on the above test as a sufficient demonstration of the presence of alcohol, more particularly as the proportion of alcohol, as calculated from the specific gravity, differs widely from that derived from the vapour-tension, and neither agree with the proportion as calculated from the amount of acetic acid obtained by oxidation. Moreover, the distillate yields the iodoform test far more readily than would correspond to its alcoholic strength as calculated by any of the above processes, and the appearance of the precipitate also differs somewhat from that produced in pure dilute alcohol. However, while still engaged in the examination of this substance, the author learned that M. Lieben, to whom we owe the introduc-

tion of the iodoform test, had already discovered the presence of a volatile substance in human urine, as well as in that of various animals, which gives the iodoform test. Working on larger quantities of urine, he has arrived at the conclusion that this substance is not alcohol. M. Lieben also has failed to isolate and identify the substance, owing to the very small quantity present in the urine; he thinks, however, that it may be one of the odoriferous constituents of the urine. According to the author's experience this cannot, however, be the case, since, first, the quantity of substance yielding the iodoform does not seem to be diminished by distillation over animal charcoal, whereas the urinous odour is thus almost entirely removed; secondly, the urinous odour of the distillate, in case no animal charcoal was used, is not destroyed by heating with the bichromate solution, which nevertheless produces acetic acid; thirdly, a somewhat similar substance seems present in the breath. It might be, however, that the substance giving the iodoform test and that yielding the acetic acid are two different compounds; this must be left to future researches to decide.

In conclusion, it may not be uninteresting to point out that the quantity of substance which yields the acetic acid apparently falls below the normal proportion just after the effect of a dose or doses of alcohol has passed off; after which it gradually rises again to the normal standard. A somewhat analogous effect was observed by Dr. Bence Jones and the author, in their research on the passage of quinine into and out of the tissues &c., to follow the administration of quinine. In this case the natural fluorescence of the extracts from the tissues, due to the presence of a substance resembling quinine, and therefore called animal quinoidine by the discoverers, frequently fell below the normal standard just after the effect of the quinine had passed off, gradually rising again to the normal proportion. A closer study of this relation might perhaps throw considerable light on the physiological action of alcohol both in health and in disease.

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April 11, 1872.

The EARL OF ROSSE, D.C.L., Vice-President, in the Chair.

The following Letter was read:—

Whitehall, 23rd February, 1872.

Sir,—I have had the honour to lay before the Queen the loyal and dutiful Address of the President, Council, and Fellows of the Royal Society on the occasion of the illness of His Royal Highness the Prince of Wales.

I have to inform you that Her Majesty was pleased to receive the Address very graciously. I am, Sir, your obedient Servant,

(Signed) H. A. Bruce.

W. Sharpey, Esq., M.D., Secretary to the Royal Society, Burlington House, W.

The following communications were read :—

I. "Contributions to the History of the Opium Alkaloids."—Part V. By C. R. A. Wright, D.Sc., Lecturer on Chemistry in St. Mary's Hospital Medical School. Communicated by Prof. G. G. Stokes, Sec. R. S. Received March 9, 1872.

§ 1. *On the Polymerides of Codeia.*

In Part IV. of these researches reasons have been adduced for the following general conclusions, viz. that codeia and morphia are capable of forming polymerides (with the elimination of methyl in the case of codeia in some instances), which yield derivatives containing certainly not less than C_{68}, and probably not less than C_{136} (C_{72} and C_{144} in the case of those codeia derivatives where methyl has not been eliminated). Experiments now in progress tend to show that the formulæ of codeia and morphia are really double of those formerly ascribed to these bases, *i. e.* are $C_{36}H_{42}N_2O_6$ and $C_{34}H_{38}N_2O_6$ respectively, the proof of which is (as will be shown in a subsequent communication) that the first products of the action of hydrochloric acid on these bases appear to contain chlorine and carbon in the proportions C_{36} and Cl, C_{34} and Cl respectively, instead of C_{18} and Cl, C_{17} and Cl. It might be anticipated, therefore, that intermediate polymerides might be formed containing respectively :—

Morphia series.		Codeia series.	
Monomorphia..	$C_{34}H_{38}N_2O_6$	$C_{36}H_{42}N_2O_6$	.. Monocodeia.
Dimorphia	$C_{68}H_{76}N_4O_{12}$	$C_{72}H_{84}N_4O_{12}$	.. Dicodeia.
Trimorphia....	$C_{102}H_{104}N_6O_{18}$	$C_{108}H_{126}N_6O_{18}$	.. Tricodeia.
Tetramorphia..	$C_{136}H_{152}N_8O_{24}$	$C_{144}H_{168}N_8O_{24}$	.. Tetracodeia.

In the case of codeia these anticipations have been verified.

In order to obtain these supposed polymerides before their further alteration by secondary reactions, the action of acids other than the hydracids was examined. Acetic acid seemed a probable agent for this purpose; but no appreciable quantity of any thing different from ordinary codeia was obtained after sixty-four hours' digestion at 100° of one part of this base with three parts of glacial acetic acid. On precipitation of the product by Na_2CO_3 in large excess, extraction with ether, and agitation of the ethereal extract with H Cl, a crystalline mass was obtained which developed a smell of acetic acid on standing in contact with a slight excess of H Cl; but on analysis this gave numbers agreeing with those required for codeia hydrochlorate, and from it nothing different from codeia could be obtained; probably therefore only a trace of acetyl-codeia was formed.

The action of phosphoric acid, however, was found to lead to the desired result without the formation of bye-products beyond colouring-matters formed by the high temperature employed; by heating codeia with 3 parts of glacial phosphoric acid and 5 of water for several hours at 100°, no per-

ceptible change is produced. The same result follows on boiling for twelve hours (boiling-point 105°) with an inverted condenser attached to prevent loss of water by evaporation; but if the boiling-point be allowed to rise slowly from evaporation, the mixture being very gently boiled in a long-necked flask, the product gradually acquires the power of giving an immediate amorphous precipitate with $Na_2\,CO_3$; no large amount of new substances are, however, formed until the boiling-point has risen to about 200°, beyond which point the evaporation cannot safely be pushed. The viscid chestnut-coloured liquid, while still hot, is dissolved in boiling water and allowed to cool; nothing separates on cooling: when cold, the liquid is nearly neutralized by caustic soda, and then precipitated with $Na_2\,CO_3$; the precipitate is collected on filters, drained from mother-liquors, dissolved in weak $H\,Cl$, and reprecipitated by $Na_2\,CO_3$, to get rid of traces of unaltered codeia mechanically retained; finally, the drained precipitate is exhausted with ether. The ethereal solution yields on agitation with $H\,Cl$ a crystalline hydrochlorate, which may be purified by solution in water, fractional precipitation with $Na_2\,CO_3$, and repetition of the ether process, and finally by recrystallization of the resulting hydrochlorate.

The portion of the first $Na_2\,CO_3$ precipitate insoluble in ether is dissolved in $H\,Cl$, and fractionally precipitated by $Na_2\,CO_3$, to remove colouring-matters as much as possible: the last precipitate, after thorough washing and drying, forms a light buff-coloured amorphous powder that does not soften at 100° when perfectly dry, but clots to a resinous mass if heated in the water-bath while still moist; it is soluble in alcohol, is precipitated from this solution on addition of ether, and yields salts that have no vestige of crystalline characters.

Both the crystalline and the non-crystalline hydrochlorates yield on analysis numbers identical with those required for codeia hydrochlorate; for the reasons developed in the subsequent sections, they are regarded as respectively di- and tetracodeia.

The filtrate from the original $Na_2\,CO_3$ precipitate contains much unaltered codeia; by extracting with ether and agitation of the extract with excess of phosphoric-acid solution, a mixture of phosphates is obtained, from which a further quantity of each polymeride is obtainable by simply boiling down the liquid till the boiling-point reaches 200°.

The hydrochlorate of tetracodeia obtained as above described forms a brownish brittle tar, not fusible at 100° when dry; dried at 100° it yields the following numbers:—

Specimen A.	0·325	grm.	gave	0·773 CO_2	and	0·186 H_2O.
„ B.	0·3145	„	„	0·732	„	0·185 „
	0·1215	„	„	0·0495 $Ag\,Cl$.		

	Calculated.		Found. A.	Found. B.
C_{144}	1728	64·38	64·87	63·48
H_{176}	176	6·56	6·36	6·54
N_8	112	4·17		
O_{24}	384	14·30		
Cl_8	284	10·59		10·08
$C_{144}H_{168}N_8O_{24}$, 8H Cl	2684	100·00		

The free base gave the following numbers:—

0·3095 grm. gave 0·818 CO_2 and 0·190 H_2O.

	Calculated.		Found.
C_{144}	1728	72·24	72·08
H_{168}	168	7·02	6·82
N_8	112	4·68	
O_{24}	384	16·06	
$C_{144}H_{168}N_8O_{24}$	2392	100·00	

In appearance and most physical properties, tetracodeia and its salts bear a great resemblance to chloro- and bromo-tetracodeia; and they further agree in that all yield a blood-red colour on warming with silver nitrate and nitric acid, or with nitric acid alone; it differs from chloro-tetracodeia in that the aqueous solution of the hydrochlorate does not precipitate on the addition of strong H Cl, the salt being apparently as soluble in diluted H Cl as in water; also the free base does not oxidize so readily. In all respects tetracodeia agrees with the description given by Anderson of his "amorphous codeia" * obtained by the action of sulphuric acid on codeia. On comparison with the product obtained by Anderson's process, no essential differences could be detected between the two substances, except that the phosphoric-acid product was somewhat darker in tint, owing no doubt to the presence of colouring-matters from the higher temperature employed in its production.

The hydrochlorate of dicodeia obtained as above described crystallizes with $3H_2O$ for every C_{18} contained, this water of crystallization being wholly lost at 100° and partially by standing over sulphuric acid.

2·163 grms. of crystals dried on filter-paper lost at 100° 0·295 grm.
Actual loss .... = 13·63 per cent.
Calculated for........ $C_{72}H_{84}N_4O_{12}$, $4HCl + 12H_2O$ = 13·86 "
2·012 grms. of crystals that had stood three days over SO_4H_2 lost, at 100°, 0·172............ = 8·54 "

* Anderson, Ed. Phil. Trans. xx. [1] 57.

Dried at 100°, these crystals gave these numbers:—

0·306 grm. gave 0·719 CO_2 and 0·182 H_2O.
0·3135 „ 0·742 „ 0·194 „
0·229 „ 0·098 AgCl.

	Calculated.		Found.	
C_{72}	864	64·38	64·08	64·54
H_{88}	88	6·56	6·61	6·88
N_4	56	4·17		
O_{12}	192	14·30		
Cl_4	142	10·59	10·60	
$C_{72}H_{84}N_4O_{12}$, 4HCl	1342	100·00		

Na_2CO_3 throws down from the solution of the hydrochlorate white amorphous flakes that do not oxidize spontaneously in the air. Dried at 100°,

0·2965 grm. gave 0·7765 CO_2 and 0·189 H_2O.

	Calculated.		Found.
C_{72}	864	72·24	71·43
H_{84}	84	7·02	7·08
N_4	56	4·68	
O_{12}	192	16·06	
$C_{72}H_{84}N_4O_{12}$	1196	100·00	

If the solution of the hydrochlorate be concentrated, the addition of Na_2CO_3 solution throws down tarry globules consisting of a mixture of the base and its hydrochlorate, the salt being sparingly soluble in the NaCl solution formed by the decomposition.

Dicodeia and its salts do not yield a blood-red colour with NO_3H, only a slight orange tint; Fe_2Cl_6, also $SO_4H_2+K_2Cr_2O_7$, give no colour-reactions.

In general properties, and in the fact that the water of crystallization possessed by the hydrochlorate is lost at 100°, dicodeia bears a great resemblance to the "isomer of codeia" obtained by Drs. Matthiessen and Armstrong by the action of diluted sulphuric acid on codeia*. On comparison with the product obtained by Armstrong's process, no difference whatever was discernible provided the hydrochlorate obtained by the action of sulphuric acid &c. were several times recrystallized. The crude hydrochlorate contains, besides the dicodeia salt, the hydrochlorate of another polymeride which differs from dicodeia hydrochlorate in that it is non-crystalline, drying up to a gummy, extremely hygroscopic and deliquescent substance; it yields a blood-red colour with NO_3H, and with $SO_4H_2+K_2Cr_2O_7$ a very evanescent purplish red; Fe_2Cl_6 gives no coloration at first,

* Chem. Soc. Journ. [2] ix. 56.

but on standing, a reddish purple tinge appears, gradually becoming more intense. Na_2CO_3 throws down an amorphous white precipitate, which is soluble in ether and but little changed by exposure to air. From these properties, which seem to be analogous in some respects to dicodeia, in others to tetracodeia, the base is considered to be intermediate between these two polymerides, *i. e.* to be *tricodeia.* The crude hydrochlorate of dicodeia obtained by Armstrong's process furnished on recrystallization mother-liquors which, on standing over SO_4H_2 for several weeks, gradually deposited crystals, and finally became a crystalline mass wetted with a viscid non-crystalline liquid: by gentle pressure in filter-paper the liquid portion was separated from the crystals, which were found to be only dicodeia hydrochlorate; and finally the treacly hydrochlorate of tricodeia was extracted from the papers by water. On repetition of the treatment over SO_4H_2, no crystals were obtained even after several weeks' standing; at 100° a brittle, gummy, hygroscopic substance was obtained, of which

0·309 grm. gave 0·730 CO_2 and 0·191 H_2O.
0·208 „ 0·0895 AgI.

	Calculated.		Found.
C_{108}	1296	64·38	64·43
H_{132}	132	6·56	6·87
N_6	84	4·17	
O_{18}	288	14·30	
Cl_6	213	10·59	10·64
$C_{108}H_{126}N_6O_{18}$, 6HCl	2013	100·00	

§ 2. *Action of Hydrochloric Acid on the Polymerides of Codeia.*

(*a*) *Tetracodeia.*—Tetracodeia hydrochlorate was boiled for six hours with a large excess of strong HCl; no perceptible evolution of methyl chloride took place; and on examining the resulting product no change was found in the ratio of carbon to chlorine. Hence no substitution of Cl for OH had taken place, and apparently no action at all had ensued.

(*b*) *Tricodeia.*—Tricodeia hydrochlorate was heated to 100° for 1¼ hour with a large excess of strong HCl; on adding water to the product, a tarry substance was precipitated, whereas the original tricodeia hydrochlorate is readily soluble in dilute HCl: precipitated by Na_2CO_3 and the precipitate exhausted with ether, a viscid non-crystalline hydrochlorate was obtained on agitation of the ethereal extract with HCl. The reactions of this product appear to be identical with those of tricodeia, excepting that the reddish purple tinge with Fe_2O_6 appears instantaneously instead of only after standing a short time. Dried at 100°,

0·3070 grm. gave 0·756 CO_2 and 0·185 H_2O.
0·2480 „ 0·1150 AgCl.

	Calculated.		Found.
C_{108}	1296	68·03	67·16
H_{120}	120	6·30	6·69
Cl_6	213	11·18	11·48
N_6	84	4·41	
O_{12}	192	10·08	
$C_{108}H_{114}N_6O_{12}, 6HCl$	1905	100·00	

Hence this product has been formed by the reaction

$$C_{108}H_{126}N_6O_{18}, 6HCl = 6H_2O + C_{108}H_{114}N_6O_{12}, 6HCl,$$

and has the composition of a polymeride of "apocodeia." From the great similarity observed between this product and "apocodeia" made by Matthiessen and Burnside's process*, it appears probable that the product of the action of zinc chloride on codeia is a mixture of bodies of general formula $(C_{18}N_{19}NO_2)_n$, nHCl, in which the derivative where $n=6$ greatly predominates: experiments on the action of zinc chloride on morphia now in progress in conjunction with Herr L. Mayer indicate that mixtures are obtained in this case also.

(*c*) *Dicodeia.*—When pure dicodeia hydrochlorate is heated to 100° for one hour with a large excess of HCl, a change is produced expressible by the equation

$$C_{72}H_{84}N_4O_{12}, 4HCl + HCl = C_{72}H_{83}ClN_4O_{11}, 4HCl + H_2O,$$

which shows that the formula of this polymeride contains at least C_{72}. Na_2CO_3 throws down from the product a voluminous white precipitate, which differs in appearance slightly from that of dicodeia and turns green by exposure to air; ether dissolves this precipitate, and on agitation with HCl a viscid hydrochlorate is obtained which does not crystallize, but dries up to a gum. Fe_2Cl_6 gives a brown-purple tint, NO_3H a blood-red, and $K_2Cr_2O_7 + SO_4H_2$ a lighter blood-red, none of which reactions occur with the original dicodeia. Dried at 100°,

0·3200 grm. gave 0·737 CO_2 and 0·189 H_2O.
0·3260 „ 0·172 AgCl.

	Calculated.		Found.
C_{72}	864	63·50	62·82
H_{87}	87	6·39	6·56
Cl_5	177·5	13·04	13·06
N_4	56	4·12	
O_{11}	176	12·95	
$C_{72}H_{83}ClN_4O_{11}, 4HCl$	1360·5	100·00	

* Proc. Roy. Soc. vol. xix. p. 71.

§ 3. *Action of Hydriodic Acid and Phosphorus on the Polymerides of Codeia.*

(*a*) *Dicodeia.*—When pure dicodeia is dissolved in a large excess of strong hydriodic acid (55 per cent. HI) and heated, together with a piece of phosphorus, to ebullition until the boiling-point rises to 120°, methyl iodide is given off and a considerable quantity of phosphoric acid formed. The product, filtered through asbestos and precipitated with water, yields snow-white flakes that become yellow by exposure to air, and melt to a colourless oil at 100° when moist, although they do not fuse at that temperature when thoroughly dried. Dried at 100°,

0·3155 grm. gave 0·5620 CO_2 and 0·1460 H_2O.
0·1895 „ 0·1190 AgI.

	Calculated.		Found.
C_{136}	1632	48·45	48·58
H_{161}	161	4·78	5·14
I_9	1143	33·94	33·92
N_8	112	3·33	
O_{20}	320	9·50	
$C_{136}H_{153}IN_8O_{20}, 8HI$	3368	100·00	

Hence this substance is formed by the reaction

$$2C_{72}H_{81}N_4O_{12}, 4HI + 17HI = I_8 + 4H_2O + 8CH_3I + C_{136}H_{153}IN_8O_{20}, 8HI.$$

The physical properties of this substance are almost identical with those of the bodies of analogous constitution (containing C_{136}) formerly obtained from both codeia and morphia (Part IV.). Carbonate of sodium throws down a precipitate almost insoluble in ether, showing that polymerization to the tetra series has taken place; agitated with a large bulk of ether, this precipitate furnishes an extract which, on agitation with dilute nitric acid and boiling with $AgNO_3$ and NO_3H of the nitrate thus obtained, yields a precipitate of AgI, showing that iodine is contained in the precipitated base. The substance itself, boiled with $AgNO_3$ and HNO_3, produces a deep orange-colour, intermediate in tint between the blood-red produced by the derivatives of polymerized $C_{17}H_{19}NO_3$, and the deep yellow of those of polymerized $C_{17}H_{21}NO_3$, a result confirmatory to some extent of the formula deduced from the analysis, this being capable of representation as

$$8(C_{17}H_{20}NO_3) + 9HI - 4H_2O.$$

From this it appears pretty evident that the formulæ hitherto attributed to the tetra bases (containing $C_{68} - C_{72}$) are only half the true ones, which contain $C_{136} - C_{144}$.

(*b*) *Tetracodeia.*—On treating tetracodeia in the same way and continuing the ebullition until the temperature reaches 130°, a brown syrupy liquid is finally obtained, which yields, on filtration through asbestos and

precipitation with water, a yellow brittle tar not fusible at 100° when quite dry; methyl iodide is produced in quantity during the action, but only traces of phosphoric acid, and this probably by atmospheric oxidation. Dried at 100° the tar gave these numbers:—

0·3660 grm. gave 0·621 CO_2 and 0·149 H_2O.
0·5520 „ 0·363 AgI.

	Calculated.		Found.
C_{136}	1632	46·31	46·27
H_{158}	158	4·48	4·53
I_{10}	1270	36·04	35·54
N_8	112	3·18	
O_{22}	352	9·99	
$C_{136}H_{150}I_2N_8O_{22}, 8HI$	3524	100·00	

Hence this substance is formed by the reaction

$$C_{144}H_{168}N_8O_{24}, 8HI + 10HI = 8CH_3I + 2H_2O + C_{136}H_{150}I_2N_8O_{22}, 8HI.$$

NO_3H and $AgNO_3$ give a blood-red coloration with this product, showing, as the analytical numbers indicate, that it is derived from polymerized $C_{17}H_{19}NO_3$, and not from polymerized $C_{17}H_{20}NO_3$, or $C_{17}H_{21}NO_3$.

The foregoing results show that the methyl group in codeia is unaltered during the polymerization to dicodeia and to tetracodeia, and furnishes another proof of the conclusion come to in Part IV. § 2, that the addition of H_2 for C_{17}, when HI and P act on morphia or codeia, takes place *before* and not after the final polymerization; even polymerization to dicodeia could not precede this addition of H_2, as the product obtained from that polymeride has only H added on for C_{17}.

The following formulæ show clearly the difference in the action of hydriodic acid and phosphorus on codeia and its polymerides:—

Alkaloid.	Temperature.	Formula of product.
Codeia	100°	$8(C_{17}H_{19}NO_3 + H_2) + 12HI.$
„	....110°–115°	$8(C_{17}H_{19}NO_3 + H_2) + 12HI - 4H_2O.$
„	....up to 130°	$8(C_{17}H_{19}NO_3 + H_2 - O) + 12HI - 4H_2O.$
Dicodeia....	up to 120°	$8(C_{17}H_{19}NO_3 + H) + 9HI - 4H_2O.$
Tetracodeia..	up to 130°	$8(C_{17}H_{19}NO_3) + 10HI - 2H_2O.$

From which it is clear that dicodeia is intermediate between tetracodeia and ordinary codeia. From the fact that the basic 4HI for $8(C_{17})$ are added on in the case of the first product *before* the elimination of $4H_2O$, as in the second substance in the list, it may be inferred that the action is not a true substitution of iodine for hydroxyl; analogous facts have been observed in the chlorinated substances obtained by the action of HCl on codeia and morphia, the first action being apparently a direct *addition* of the elements of HCl, the subtraction of the elements of H_2O taking place at a later stage.

§ 4. *Action of Sulphuric Acid on Codeia and its Polymerides.*

The results detailed in the previous sections show that the action of sulphuric acid on codeia is to polymerize it with the formation of di-, tri-, and tetracodeia, the substances obtained by Armstrong and by Anderson by this means being identical with the first and last of these bases. It appears probable that tetracodeia may be formed by the further polymerization of dicodeia, whereas it would seem as though tricodeia were not likely to be obtained from dicodeia; on the other hand, it is possible that tetracodeia is directly produced from codeia, and that it could not be formed from dicodeia. To settle this point, pure dicodeia was heated to very gentle ebullition with sulphuric acid diluted with its own bulk of water for five hours, the operation being conducted in a long-necked flask, so that no appreciable concentration by evaporation took place. At the end of this time the dicodeia was wholly converted into a base, of which ether dissolved only traces, and hence no tricodeia was formed. After precipitation by Na_2CO_3 and drying, the free base was dissolved in alcohol and fractionally precipitated by ether. If the alcoholic solution be nearly free from water, the ether throws down solid amorphous flakes; but if 10 or more per cent. of water be present, the ether precipitate is a tarry fluid containing water, alcohol, and the base. Flakes of tetracodeia were thus obtained identical in all respects with that obtained by the action of phosphoric acid; a trace of some product of the further action of sulphuric acid appeared to be present, however, as the free base turned slightly green on drying, without, however, absorbing so much oxygen as to make any appreciable difference in its composition. Dried at 100°, 0·221 grm. gave 0·583 CO_2 and 0·142 H_2O.

	Calculated.		Found.
C_{144}	1728	72·24	71·94
H_{168}	168	7·02	7·14
N_8	112	4·68	
O_{24}	384	16·06	
$C_{144}H_{168}N_8O_{24}$	2392	100·00	

If the action of sulphuric acid be pushed further than this point, a smell of SO_2 is perceptible, and the product obtained rapidly oxidizes on precipitation by Na_2CO_3 and exposure to air. Nothing fit for analysis was obtained from the product, which probably is formed by the dehydration, oxidation, and possibly demethylization of tetracodeia.

§ 5. *On the Physiological Action of the foregoing Polymerides. By* REGINALD STOCKER, *M.B., Pathologist in St. Mary's Hospital Medical School.*

An aqueous solution of the hydrochlorate of codeia and its polymerides was in each case employed, being subcutaneously injected into adult cats (a

dog being also employed in a few experiments), quantities equivalent to 0·1 grm. of the anhydrous salt being used in each experiment. Four cats were employed, several trials being made with each animal, and three or four days being allowed to intervene between each experiment, so that the effects of one dose had entirely passed away and the animal entirely recovered before the administration of another dose. The main results observed were as follow:—

Codeia.—Four experiments. In each instance dilated pupils; cerebral congestion (determined by ophthalmoscopic examination), and much increased reflex excitability (epileptic convulsions in one case); salivation and purging in two cases; vomiting not produced in any case.

Dicodeia.—Two experiments. In each instance vomiting; fundus of eye not congested; pupil dilated in one case.

Another experiment with a dog (full-grown she-terrier) produced salivation and purging without vomiting; no cerebral congestion.

Tricodeia.—Three experiments. In each case salivation (profuse) and dilated pupils; no cerebral congestion; in one case slight excitement, in the others purging and depression: vomiting produced in one of these two latter instances, micturation in the other.

Tetracodeia.—Four experiments. In each case profuse salivation, micturation, and depression; dilated pupils in three instances, and lachrymation in two; in one case vomiting and purging, in another increased reflex excitability with an occasional convulsion (cat was weak and not in good condition); slight hypnotism in two cases.

In two experiments with the dog, salivation and depression only were produced.

From these results it would appear that codeia produces cerebral congestion and increased reflex excitability without vomiting; whilst di- and tetracodeia produce profuse salivation and some depression, with vomiting in several instances, no evidence of cerebral congestion and but little of increased reflex excitability being noticeable.

§ 6. *Conclusions.*

The foregoing results suggest the probability of other bases being capable of forming similar polymerides. In anticipation of this result experiments are in progress with morphia.

Hesse has shown* that by the action of HCl on thebaine there are produced two isomerides of that base, one forming crystalline salts, one amorphous salts; not improbably these are respectively dithebaine and tetrathebaine.

Matthiessen and Foster have shown† that cotarnine occurs in crystals of the formulæ $C_{12}H_{13}NO_3+\frac{1}{2}H_2O$ and $C_{12}H_{13}NO_3+H_2O$; and the

* Ann. Chem. Pharm. vol. clxiii. p. 47.

† Proc. Roy. Soc. vol. xvii. p. 342 (5).

Reagent &c.	Codeia.	Dicodeia.	Tricodeia.	Tetracodeia.
Alcohol	Soluble	Soluble	Soluble	Soluble.
Ether	Soluble	Soluble	Soluble	Insoluble.
Character of base.	Crystalline, stable in the air.	Amorphous, stable in the air.	Amorphous. Very slowly oxidizes while moist.	Amorphous. Very slowly oxidizes while moist.
Character of hydrochlorate.	Crystallizes with $2H_2O$ for C_{18}, not lost at 100°.	Crystallizes with $3H_2O$ for C_{18}; lost at 100°, and partially at lower temperatures.	Non-crystalline, extremely deliquescent.	Non-crystalline, deliquescent.
Ferric chloride ...	Nil	Nil when pure ...	No colour at first, reddish purple on standing.	Reddish-purple colour immediately.
Nitric acid	Light orange	Light orange	Blood-red	Blood-red.
Potassium dichromate and sulphuric acid.	Nil	Nil	Evanescent red ...	Evanescent red.
Sodium carbonate and solution of hydrochlorate.	No immediate precipitate, crystals on standing.	Instantaneous amorphous precipitate but little soluble in excess.	Same as dicodeia.	Same as dicodeia.
Caustic potash and solution of hydrochlorate.	Oily precipitate, if concentrated, becoming crystalline on standing. Not markedly soluble in excess.	Oily precipitate, if concentrated, not becoming crystalline; more dilute solutions give a white amorphous precipitate soluble in large excess.	Same as dicodeia.	Same as dicodeia.
Action of hydrochloric acid, not pushed to extreme.	Product contains Cl for C_{36}; further action contains Cl_2 for C_{36}.	Product contains Cl for C_{72}.	H_2O removed for C_{18}; no basic Cl contained in product.	Nil.
Action of hydriodic acid, in conjunction with phosphorus, not pushed to extreme.	Polymerizes with elimination of CH_3 for C_{18}, forming bases derived from $(C_{17}H_{21}NO_3)_n$, H_2 being added on for C_{17} in product.	Polymerizes with elimination of CH_3 for C_{18}, forming bases derived from $(C_{17}H_{20}NO_3)_n$, H being added on for C_{17} in product.	....	CH_3 eliminated for C_{18}; product derived from $(C_{17}H_{19}NO_3)_n$, no H being added on, but simply I substituted for OH.
Action of sulphuric acid, not pushed to extreme.	Polymerizes, forming successively di-, tri-, and tetracodeia.	Polymerizes, forming tetracodeia.	....	Nil. Further action probably dehydrates and oxidizes.
Formula inferred from above properties and reactions.	$C_{36}H_{12}N_2O_6$.	$C_{72}H_{84}N_4O_{12}$...	$C_{108}H_{126}N_6O_{18}$.	$C_{144}H_{168}N_8O_{24}$.
Physiological action of 0·1 grm. of anhydrous hydrochlorate subcutaneously injected into adult cats.	Extreme hypersensitiveness and cerebral congestion, dilatation of pupils; no diarrhœa; no vomiting in any instance.	No hypersensitiveness nor cerebral congestion; dilatation of pupils; vomiting in every instance. With a dog pro[illegible] fuse diarr[illegible] without vom[illegible]ing.	Hypersensitiveness scarcely marked, vomiting in som[illegible] instances, in [illegible] others salivatio[illegible] [illegible] defæcation [illegible]	No hypersensitiveness; vomit[illegible] [illegible] salivation, [illegible]

writer's observations indicate that the former substance is more stable than the latter, which rapidly becomes more or less coloured; not improbably these two forms are polymerides, the first being $C_{24} H_{26} N_2 O_6 + H_2 O$, the second $(C_{12} H_{13} NO_3)_n nH_2 O$. Opianic acid*, on heating, furnishes an anhydride of formula $C_{40} H_{38} O_{19}$; this tends to show that the formula of this acid is not less than $C_{20} H_{20} O_{10}$; not impossibly, therefore, the formula of narcotine may be double that usually ascribed to it, and the dimethylnornarcotine, methylnornarcotine, and nornarcotine of Matthiessen may be derivatives not of ordinary narcotine, but of its polymerides.

The different modifications of the cinchona alkaloids are not impossibly polymerides of one another.

The Table (p. 288) exhibits the principal differences between codeia and the polymerides above described.

II. "Researches on Solar Physics."—III. By Warren De La Rue, D.C.L., F.R.S., Balfour Stewart, LL.D., F.R.S., and Benjamin Loewy, F.R.A.S. Received March 12, 1872.

(Abstract.)

The authors present in this paper the third instalment of the determination of the areas and heliographic positions occupied by the sun-spots observed by the Kew photoheliograph, comprising the years 1867, 1868, and 1869. They announce that the fourth and last instalment is in active progress, and will be preceded by the final discussion of the whole ten-yearly period, during which the photoheliograph has been at work. This final discussion will contain the determination of the astronomical elements of the sun on the basis of photographic observations; and this work, they anticipate, will not only settle the question of rotation for a considerable time to come, but will also throw light upon many points which have only recently been brought under the consideration of scientific men. The results in general, they believe, will prove the superiority of photographic sun-observations over previous methods. The second question which will be discussed is the distribution of sun-spots over the solar surface. The facts already brought out indicate that the progress of the inquiry may lead to some definite laws which regulate the distribution; there appear to exist centres of great activity on the sun, and the different solar meridians seem to have various but definite intervals of rest and activity. In conclusion the authors point out the necessity of devoting in future greater attention to the study of the faculæ, and express a hope of seeing photographic sun-observations carried on in this country on a more extended system, connecting from day to day solar phenomena with terrestrial meteorology and magnetism.

* Proc. Roy. Soc. vol. xvii. p. 341, § III. (Matthiessen and Wright).

III. Corrective Note by Messrs. De La Rue, Stewart, and Loewy to their Paper "On some recent Researches in Solar Physics &c." *

The erroneous date given in our paper for one of Professor Wolf's maxima has already been corrected by us, and we give in the subjoined little Table the corrections of the few numerical data which are necessitated by the error of fixing the date of maximum at 1846·6 instead of 1848·6.

Professor Wolf's ratio $\frac{A}{B}$ (p. 86).

	Erroneous figures given previously.		Differences.	Corrected figures.		Differences.
I.	1·265	Mean 2·093	−0·728	1·265	Mean 1·548	+0·283
II.	2·615		+0·522	1·478		+0·073
III.	2·400		+0·307	1·900		+0·352

The differences derived from our own results are respectively +0·061, −0·107, and +0·047, that is, they are still much smaller, and agree singly better with the mean, than if Professor Wolf's ratio were adopted; hence our conclusion is quite unaffected by this correction.

The remark made by us with reference to this maximum (*vide* p. 85) will remain in force even with the corrected date. We stated there that this particular maximum showed alone an appreciable difference from the dates fixed by ourselves, for it will be found that Professor Wolf's date differs still by about $\frac{3}{4}$ of a year from ours.

IV. "The Action of Oxygen on Copper Nitrate in a state of tension." By J. H. Gladstone, Ph.D., F.R.S., and Alfred Tribe, F.C.S. Received March 14, 1872.

In our experiments on the action between copper and nitrate of silver in solution, we frequently noticed that the tips of the silver crystals became red, as though coated with a thin layer of metallic copper.

This apparent deposition of a positive on a more negative metal of course raised our curiosity, and led us to look closely into the circumstances under which it occurred. We found that it took place only when the nitrate of silver was exhausted, and only on those silver crystals which remained in metallic connexion with the copper. We found, too, that the cupreous coating formed most readily where air had the freest access, and, in fact, that it would not form at all in vessels from which oxygen was excluded, nor on those white crystals which were far below the surface of the liquid, though they might be in immediate contact with the copper plate. When an inverted jar was filled with nitrate-of-copper solution and silver crystals resting on branches of copper, and the liquid was displaced by oxygen gas, it was found that the tips of the crystals became red, and

* *Suprà*, p. 82.

the solution gradually filled the jar again by the absorption of the gas. In the same way the oxygen was absorbed from air, or from its mixtures with hydrogen or carbonic anhydride.

This action was further studied by employing plates of the two metals instead of copper covered with silver crystals. When the two plates, connected by a wire, were partially immersed in an ordinary aqueous solution of copper nitrate, it was found that a slight yellowish deposit made its appearance speedily all over the silver plate, and went on increasing for a day or two, while at the air-line there was a thicker deposit, which gradually grew and extended itself a little below the surface. This deposit changed from yellowish to red, and under the microscope presented a distinctly crystalline appearance.

Thinking that this slight crust all over the silver plate was due to air dissolved in the solution itself, we took advantage of the reaction to prepare copper nitrate absolutely free from dissolved oxygen. An ordinary solution of the salt mixed with some silver nitrate was placed in a narrow cylinder, with a long piece of copper-foil arranged somewhat spirally, so as to retain the deposited silver on its surface, and allowed to rest for twenty-four hours. The solution thus obtained was exposed to the action of the conjoined copper and silver plates; but even after some hours there was no dimming of the lustre of the silver plate, except at the air-line, which was sharply defined. The same solution, shaken for some time in the air, produced a yellowish deposit on the white metal in three minutes.

The colour and general appearance of this crust, together with its formation only where oxygen can be absorbed, showed that it was not metallic copper, but the suboxide. This was further proved by the action of dilute sulphuric acid, which resolves it at once into red metallic copper and copper sulphate. There is also another curious reaction, which can only be properly observed under a microscope. When treated with a solution of silver nitrate, this cupreous deposit does not give the ordinary crystals of the white metal; in fact it is only slowly acted upon; but presently there shoot forth thin threads of silver, which run through the liquid, often twisting at sharp angles, while the yellowish crystals change to black. This also was found to be a property of the suboxide of copper.

This deposition of oxide on the silver is accompanied by a corresponding solution of copper from the other plate. Thus, in an experiment made with nitrate-of-copper solution that had been exposed to air, and which was allowed to continue for four days, there was found:—

Gain of silver plate 0·016 grm.
Loss of copper plate 0·015 grm.

The copper necessary for the production of 0·016 grm. of suboxide would be a little above 0·014 grm.

The wire connecting the two plates in this experiment is capable of deflecting a galvanometer. The current takes place through the fluid from

copper to silver, that is, in the same direction as if the copper had been dissolved by an acid and hydrogen evolved on the silver plate.

If the two plates have their sides parallel, the suboxide is deposited not merely on that side of the silver plate which faces the copper, but after about a minute on the other side also, showing that in this, as in other cases, the lines of force curve round.

It became interesting to consider what started this electric current. The original observations convinced us that it was not due to the action of oxygen on the copper; but, to make the matter more certain, bright copper and silver plates in conjunction were immersed, the copper in a pure, *i. e.* deoxygenized, solution of nitrate of copper, the silver in an oxygenized solution: the two liquids communicated through the diaphragm of a divided cell. In half an hour the silver plate was covered with a reddish film, while not a trace of tarnish was perceptible on the copper. On continuing this experiment for three hours, it was found that the copper plate lost 0·003 grm., and the silver plate was increased by 0·004 grm. On cleaning the plates, and reversing their position, the copper was covered with a film of oxide, while the silver remained free from cupreous deposit. We believe therefore that, through the simultaneous action of the two metals, the dissolved salt is put into such a state of tension that oxygen brings about a chemical change which otherwise would be impossible, and that this change is initiated in close proximity to the more negative metal.

Though we have examined only this particular reaction, we have satisfied ourselves that it is not an isolated fact. Each of the elements concerned may be replaced by others: thus, the sulphate may be substituted for the nitrate of copper, or platinum may be used instead of silver; chlorine may take the place of oxygen, with the production of the subchloride instead of the suboxide; and zinc may be employed as the positive metal, with zinc chloride as the salt in solution, in which case copper may be taken as the negative metal, and on its surface will form a deposit of oxide of zinc.

April 18, 1872.

WILLIAM SPOTTISWOODE, M.A., Treasurer and Vice-President, in the Chair.

The following communications were read:—

I. "On the Connexion between Explosions in Collieries and Weather." By Robert H. Scott, M.A., F.R.S., and William Galloway, Esq., Mining Engineer. Received March 19, 1872.

The gas commonly called "Fire-damp," to the mixture of which with atmospherical air the formation of the explosive mixture in coal-mines is due, exudes from the coal at a certain pressure; so that the rate of its escape must, to some extent, depend on the pressure of the atmosphere,

especially in the shallower mines, where the tension of the gas is not great and the fissures are open. On the other hand, the effect of a given quantity of gas in rendering the air of a mine explosive must depend on the supply of pure air to the workings.

It has long been observed that when the barometer, after having stood at a high level for a time, begins to fall more or less rapidly, the accumulations of foul air and gas in the goaves and fissures of a mine emit part of their contents into the ventilating-currents which flow past them.

It is also well known that as the very existence of these currents depends, in almost every case, on the difference of temperature between two columns of air, those in the downcast and upcast shaft respectively, any increase in the temperature of the external air, from which the downcast shaft receives its supply, must necessarily render the circulation underground more sluggish.

It is therefore obvious that the tendency to explosion will be increased when the ordinary causes which lead to the fouling of the air in a mine, such as falls of roofs and leakages in the air-courses, are assisted from without by the meteorological phenomena we have just mentioned; and several investigators have compiled lists of explosions, in order to compare them with the meteorological observations which have been recorded prior to and at the time of the accidents.

At the Meeting of the British Association at Glasgow in 1855, Mr. T. Dobson, B.A., read a paper "On the Relation between Explosions in Coal-mines and Revolving Storms," which is printed entire among the Reports. The reasoning in this paper is to a great extent based upon a diagram given by Mr. J. Dickinson, Inspector for the North and East Lancashire District, in his Report for 1852, which shows the daily march of the barometer during that year at Manchester, and the explosions which occurred in his district during the year.

Mr. Dickinson, in his Report for 1866, gives a diagram showing a continuous barogram recorded at Manchester for one week (December 8–15th, 1866), characterized by a remarkable barometrical depression. Between the 10th and 13th of December 463 lives were lost in six different explosions, the two accidents at the "Oaks" Colliery causing 361 deaths.

In the 'Transactions of the North of England Institute of Mining Engineers' (vol. xix.) diagrams are given showing the meteorological records from the Observatories of Kew and Glasgow for 1868 and 1869, and the explosions reported during those years. No discussion is given, but the diagrams have been carefully compiled by the secretary, Mr. Bunning, and we have extracted the list of explosions for 1868 from this paper.

Lastly, in the 'Journal of the Austrian Meteorological Society' for February 1872, Herr F. M. Simmersbach* has published a paper on the

* "Ueber den Einfluss meteorologischer Vorgänge auf die Luftcirculation unter Tage, und auf die Gasgefährlichkeit in den Kohlgruben," von F. M. Simmersbach in Dortmund, Zeitschrift der österreichischen Gesellschaft für Meteorologie, 1872, p. 37.

subject of the present communication, but it does not call for special notice.

Mr. Dobson's is the most elaborate discussion of the subject which has yet appeared, and we must devote a few words to it.

One of the diagrams in his paper exhibits 491 explosions in five-day periods, and from it the author concludes that the annual march of the curve coincides with the march of temperature, with a relative maximum occurring in the autumn, which he attributes to the frequency of serious storms at that time of the year.

To this we can only say that the maximum and minimum of annual temperature do not occur at the precise epochs named by Mr. Dobson, and that the last quarter of the year cannot be considered as preeminently stormy, at least not more so than such a month as the January (1872) which has just passed.

In order to test this question of periodicity, we have collected from the Reports of the Inspectors of Mines all the recorded explosions for the last twenty years. These explosions are only the fatal ones, and include a few accidents due to suffocation by "choke-damp," or carbonic-acid gas. We consider that we may fairly count such accidents as due to causes closely related to those which produce explosions of "fire-damp."

We plotted the whole of these explosions in two decennial periods for the seventy-three intervals of five days each, and found that the curves hardly showed any agreement with each other, so that no confirmation was obtained for Mr. Dobson's alleged periodicity. The curve for the entire period of twenty years, including 1369 accidents, was also constructed, and all that is worth notice about it is that the number of accidents is somewhat greater in October than in other months. The absolute maximum falls at the end of January, and the absolute minimum in the middle of September.

From a comparison of the two decennial curves we learn that the number of fatal accidents has decreased from 768 in the former (1851–1860) to 601 in the latter period (1861–1870), notwithstanding the great augmentation of the output of coal during the time in question.

We do not consider it worth while to reproduce these curves.

At this juncture there is one matter which deserves attention, viz. that the number of *serious* accidents, involving the loss of ten or more lives, has very materially increased during the last five years. The number of such explosions during the years 1851–1855 (inclusive) was 13; 1856–1860, 15; 1861–1865, 12; and 1866–1870, 21. On this matter we do not offer any opinion, but it seems to us desirable to place the figures on record.

For the purposes of the present paper we have taken the continuous records furnished by the Observatory of Stonyhurst, one of the seven established by the Meteorological Committee. These records exist from the 1st of January 1868, and the curves for the years 1869 and 1870 have

een published in the Quarterly Weather Report. Stonyhurst has been selected inasmuch as from its situation, near Preston, it is more central as regards the coal-districts than any of the other observatories.

In the case of certain accidents, more especially in the South-Wales and Scotch districts, the records from Falmouth or Glasgow respectively might possibly throw more light on the matter, but we have thought it hardly worth while to complicate the diagrams by introducing them.

The curves for barometrical pressure and for temperature have been plotted from the daily maxima and minima registered for the years 1868, 1869, and 1870. The curves have not been actually reduced from the photographic records, as the horizontal scale has been so much compressed; but they afford a fairly correct representation of the march of the phenomena, and for the last two years they can be compared, if necessary, with the Quarterly Weather Report.

One serious disturbing cause interferes with the value of these curves, arising from the weekly suspension of work in the collieries, and in many instances of ventilation, too, on Sundays. The influence of this was eliminated in the former investigation, when we were seeking for periodicity, as we dealt with as many as twenty years.

The dates of the fatal explosions are all given in the Inspector's Annual Reports, those of the non-fatal explosions for 1868 have been taken from Mr. Bunning's paper. For the facts relating to the years 1869 and 1870 we are indebted to the kindness of the Inspectors, who have almost without exception responded to our application by furnishing all the information which it was in their power to give.

The occurrences have all been entered to their proper dates, and the districts in which they were reported have been noted. Fatal accidents are marked by a bar.

We have marked by special symbols all the explosions which appear to us to be connected with sudden changes in atmospherical pressure or with abnormally high temperature; a few of the occurrences may possibly be attributable to the conjoint action of these two causes.

Mr. Dobson, writing at a time when all our serious barometrical oscillations were imagined to be connected with true cyclones, considered that, as in the front of such an advancing storm the barometer is falling and the temperature rising, he was justified in putting down the liability to explosion as a phenomenon of the anterior segment of such a storm. He does not, however, give a continuous record of weather so that we can see how many storms were unaccompanied by explosions. There can be no doubt of the coincidence of certain serious explosions with severe storms; a notable instance of this will be found on the 8th of October, 1870; but the explosions do not happen only at the commencement of a barometrical depression, but occur also two or three days after the barometer has reached its lowest point and is again rising. We have taken a limit of three days.

The cause of this prolongation of the dangerous period is that when fire-damp issues in greater quantity than usual from cavities and fissures into the workings, and more especially into places where the air is stagnant and already more or less foul by admixture of gas, the volume of the explosive portion of this mixture will increase in consequence of the increased rapidity of diffusion, or, in other words, the explosive boundary will extend itself. This extension of the explosive boundary is gradual, and in some cases a considerable time may elapse before the boundary has reached its extreme limits and begins again to recede. During all this period the mine will be in an abnormally dangerous state.

Meanwhile, although the pressure of the atmosphere rises, and a current sets in backwards* into the cavities whence the pure gas has just issued, yet if the entrance to such a cavity be at a lower level than the highest portion of the space occupied by such escaped gas, which rises, owing to its low density, it is evident that what is driven back into the cavity will be a mixture of gas and air, and that no portion of the gas which lies above the level of the aperture to the cavity can be driven back into it. Accordingly a certain volume of this pure gas remains, diffusing itself freely and fouling the surrounding air.

It is evident from these considerations that, in the case of continued unsteadiness of pressure and repeated violent oscillations of the barometer, we need not expect that each of these reductions of pressure will cause the efflux of a quantity of gas proportionate to the extent of such reduction. If the successive falls of the mercury are of less magnitude than the first, or than any previous one in the series, the quantity of gas given off cannot possibly be as great on each occasion as if that fall had been preceded by a period of high pressure. If, however, any of the later oscillations be more serious than their predecessors, a certain fresh supply of pure gas will be given off. Hence we see that, as a general rule, we do not find a succession of explosions at a time when the barometer is in a state of continued violent oscillation.

Recurring to what has already been said about temperature, we would here remark that in cold weather the ventilation of the pits is exceedingly active, many collieries being ventilated easily by natural means without any extraneous agency whatever. In the height of summer, however, it is different; for then the temperature of the air in the downcast shaft is higher, and the ventilation can only be kept up by the help of the furnace. In some cases, then, a sudden rise of temperature may catch the miner unprepared, and where an active current would have remained safe, a sluggish one may become foul, and possibly an explosion may occur.

We shall now proceed to a discussion of the facts represented on the Plate for the years 1868, 1869, 1870.

* Parliamentary Evidence, 1855, p. 110. Transactions Nat. Hist. Soc. of Northumberland &c. 1831, p. 184.

1868.

The first quarter of this year was very exceptionally stormy, in fact Mr. Buchan, in a paper printed in the 'Atlas Météorologique' for 1868, states that during the sixty-three days comprised between the 13th of January and the 26th of March, as many as twenty-seven distinct storms passed over the north of Europe. We attribute thirty accidents which occurred during this period to barometrical causes.

The first serious depression occurred on the 18th of January, when three explosions were recorded, with a fourth on the 20th; the barometer remained unsteady for some days and reached a very low level on February 1, where we find four explosions close to each other. The disturbances of pressure of the 7th and 19th of February caused, the former two, the latter three explosions, as did also that of March 11th.

In April the temperature began to rise to a considerable height during the day, so that the amount of daily range was large; and so we attribute two explosions to this cause. The serious storm of April 21st was accompanied by five accidents.

A period of warm weather set in in May; and we attribute the batch of accidents between the 15th and 20th to temperature, and similarly those at the beginning of June and July. Occasionally accidents referable to temperature are also reported up to the middle of September.

After that date we find two months which were comparatively free from explosions, and also from serious storms. The first great disturbance of pressure which succeeded was on the 22nd of November, and it resulted in six explosions, extending over a period of four days.

The atmosphere was nearly as much disturbed in December as in January, and we count thirteen accidents as due to oscillations of the barometer. The most serious group was that of five, which accompanied the heavy storm of December 21st.

In all we have in 1868 154 explosions (44 of them fatal), and of these 72 seem to be due to changes of pressure, 41 to rise of temperature, while 41 appear to us to be more or less independent of either of these agencies.

1869.

The first two depressions of the year, those of January 8th and January 15th, were each accompanied by explosions, and on the 27th a period of prolonged violent disturbance set in. Two very marked barometrical oscillations passed over all our stations, bringing with them heavy equatorial storms on January 28th and February 1st. Seven explosions are recorded between the 27th and 29th, while the oscillation of the first only caused one. Pressure was very low during the first half of February, the lowest reading being recorded on the 8th, with one explosion, followed by three others on the next three days.

The storm of February 12th, which was very destructive to shipping in the Channel, was not marked by any colliery accidents, as the oscillation of

the barometer, unusually violent though it was, did not extend beyond the southern stations, and was very transient in its character.

At the end of February the weather was again much disturbed, the barograms reaching their lowest points on the 1st of March, when we find two explosions. Two more followed on the 11th, while the depression of the 17th caused three. Slight disturbances, accompanied by one or two explosions, occurred up to April 4th.

A sudden period of warm weather appears on the 10th of April, the five-day mean for the period from April 11th to April 15th, at Stonyhurst and Glasgow, being nearly 10° warmer than those which preceded and followed it; so that we consider ourselves justified in referring seven of the explosions during this period to temperature.

Another oscillation of the barometer occurred on the 16th, which was most serious in the north of England, and we find three explosions. The batch of four accidents on the 26th and 27th of April we refer to temperature. From this till the 7th of June we find only one accident referable to atmospherical causes; but on that day, as well as on the 24th and the 2nd of July, we find several explosions coincident with great warmth of the weather.

The storm of June 15th seems to have caused four explosions. The 17th of July was a very hot day, and so were the days immediately succeeding it; and we find six explosions close to each other, the last, that on July 21st, being the "Haydock" accident, which cost fifty-nine lives.

The first serious disturbance of pressure in the autumn was on September 11th, when a heavy gale swept up the Channel; and we find at this time a batch of accidents, which we attribute to barometrical causes.

On the 16th of October we had another serious disturbance, and seven explosions occurred within four days. The depression of the 29th caused three, and after that date there was nothing to call for special remark till the 21st of November. On this day a most extensive and sudden diminution of pressure was observed, the account of which, as given in the Quarterly Weather Report, is as follows:—

"The actual fall of the barometer, within twenty-four hours, had exceeded 0·9 in. over the entire district, stretching from Dover to Valencia, and from L'Orient to Shields. The superficial extent of this area is about 200,000 square miles."

This oscillation was marked by four accidents.

At the beginning of December the barometer ranged very high; but from the 13th to the 18th a period of violent oscillation is observed, the absolute minimum for the year being recorded in Scotland. Between the 13th and 15th seven explosions are registered, five of which were in the Scotch coal-fields.

The year closed with a fall of the barometer, and three accidents in the North-Staffordshire district.

On the whole, we have in 1869 192 explosions (47 of which were fatal);

92 of them we attribute to oscillations of the barometer, 34 to temperature, while 66 are left unaccounted for by either agency.

1870.

The storm of January 8th caused two explosions, and the depression of the 14th was followed by two others. The barometer then remained unusually high from the 16th to the 25th; and as soon as it began to give way, we have three explosions on the 27th, and two more to correspond with the barometrical minimum on the 31st.

The barometer at most stations read below 29·5 ins. during the first ten days of February, and we find seven accidents between the 4th and 9th; these were followed by as many more within the next few days, with a high barometer and cold weather, which are therefore not referred by us to either deficiency of pressure or a high temperature. One of these cost thirty lives.

Pressure was very unsteady and low at the end of February, and eight accidents occurred between the 21st and 26th; another somewhat similar series is observable in the second week in March.

On the 5th of April the daily range of the thermometer was considerable, 22° at Stonyhurst and 26° at Kew; and we refer two explosions to this cause.

The sudden outbreak of hot weather on the 16th, and the high temperature of 71°·7 at Stonyhurst on the 20th of May, account for two explosions in the adjoining coal-field on that day, with a third in the west of Scotland.

The weather was very hot from the 3rd to the 8th of June, and we find three explosions on the 4th, and one each on the 6th and 8th; then follow three, which we trace to the fall of the barometer. June 19th to the 22nd were also very hot days, especially in the centre and south of England; and of the six accidents reported at this time two were in the southern and central coal-fields.

In the months of July and August we have connected various explosions with the high temperature then prevailing. A period of serious disturbance existed from the 1st to the 11th of September, and we have eight explosions, and four more on the 13th. At the end of the month the weather became very warm, and we find six explosions close to each other.

The month of October was peculiarly stormy, and the first fall of the barometer occurred on the 7th and 8th; on the latter day we have five explosions; another sudden depression on the 12th was followed by three. The oscillations of the 16th and 19th had but little influence; but on the 23rd the barometer again fell very low, and we find six explosions in three days.

The next serious group of accidents was about the 20th of November, when we find several recorded simultaneously with a low state of the barometer. The mercury then rose and continued very high for some days;

but on the 11th of December a serious fall is observed, and two accidents occurred next day. The explosions which appear at the end of the year do not seem to be referable to pressure.

On the whole, we find in 1870 179 explosions (67 of them fatal), of which 92 are due to the state of the barometer, 43 to the temperature, while 44 remain unaccounted for by either of these agencies.

As a general summary, we consider that out of 525 explosions recorded in the three years, 49 per cent. may be reasonably connected with disturbance of the barometer, 22 per cent. with abnormally high temperature, while 29 per cent. are not traceable to atmospherical agency.

[Since handing in the paper, we have received the statistics of non-fatal explosions in the mines of Yorkshire during the years 1869 and 1870.

In 1869 there were eight occurrences, of which we attribute four to the state of the barometer, one to temperature, while three are unaccounted for.

In 1870 there were seventeen occurrences, of which we attribute six to the state of the barometer, four to temperature, while seven are unaccounted for.

These figures raise the total number of accidents to 550, but do not materially alter the percentages given in the text. The facts have been inserted in the diagrams.—April 13, 1872.]

It may not be out of place to discuss briefly the manner in which an increased supply of gas or a diminished supply of air brought about by any of the causes just alluded to, leads to a fouling of the ventilating-current in its passage through the workings; and also how the comparative purity or foulness of the ventilating-current affects the condition of the air in places adjoining its course.

The gas flows from the fissures of the coal and stone either into a ventilating-current, by which it is carried out of the mine, as when it escapes at the face of a long-wall working or bratticed bord, or from the sides of an air-course; or, secondly, it flows into a quiet atmosphere, such as that in goaves and cavities in the roof whence stone has fallen, in unbratticed bords, or in recesses between pack-walls in long-wall workings. In any of these latter cases it diffuses itself into the surrounding air, and an accumulation of a more or less explosive character is generated.

As long as the average quantity of air is circulating in the mine and the quantity of gas which escapes into the workings is not suddenly increased, we may take it for granted that little danger exists; for the districts of the mine in which explosive mixtures are to be met with are clearly defined and well known, so that precautions can be taken to prevent ignition.

As soon as these conditions are in any way changed (*i. e.*, if the supply of air be diminished in quantity or deteriorated in quality by an increased escape of gas), explosive mixtures may make their appearance in places where no danger had previously existed.

It may be assumed that a ventilating-current consists of pure air

when it starts from the end of the in-take air-course on its passage through the workings, and that for every equal space it travels between the in-take and return air-course it receives an equal quantity of gas. Thus when it arrives at the return air-course it consists of a mixture of air and fire-damp, whose constitution depends on the quantity of air passing through the workings, on the rate at which gas escapes into it, and on the distance between the in-take and return air-course.

It is therefore evident that if either the supply of air be diminished or the supply of gas increased, the resulting mixture will be rendered more explosive, not only in the return air-course, but also at every point of the passage between the in-take and return air-course.

If, then, from any causes the mixture shall have reached the firing-point when it enters the return air-course*, any aggravation of these causes would make the firing-point to travel backwards through the workings towards the in-take air-course. In this manner the ventilating-current may itself become explosive in some parts of its course. Again, although the ventilating-current itself may never become explosive, its gradual fouling may cause explosive mixtures to be generated in certain places, such as unbratticed bords, recesses between pack-walls, and cavities in the roof in the following manner (all other cases may be, more or less directly, referred to these):—

Fig. 1.

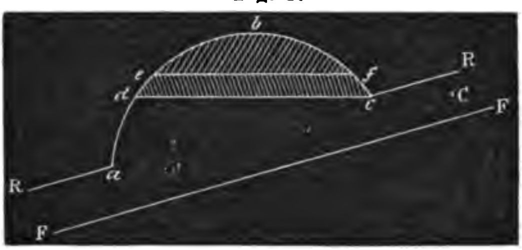
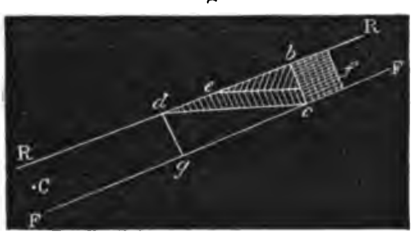


Fig. 2.



Figs. 1 and 2 represent sections of the workings. R is the roof, F the floor, C is a point where the ventilating-current passes in a direction normal to the plane of the paper. In fig. 1, *a b c* is a cavity in the roof

* Although the air entering any particular return air-course may be explosive, it will probably become diluted again before it reaches the upcast, by leakages from the in-take air-course and admixture of air from other districts.

which is filled down to the level *d c* with a mixture of air and gas. Gas may flow into such a cavity from a fissure in the sides, or from any lower part of the seam along the roof and under the edge at *a*.

In fig. 2, *d b c g* is an unbratticed bord, receiving gas from the face *b c*, filled with a similar mixture down to *d c*. The space occupied by the foul air is shaded in each case; and *e f* is a plane in that space which is chosen arbitrarily.

The plane *d c*, which bounds the lowest part of the accumulation, is level, and here the mixture contains the least proportion of fire-damp; the impurity of the air increases with the height, and the foulest atmosphere is found at *b*, the highest point of the cavity.

If any gas flows into the cavity a corresponding volume of the contents of the cavity is displaced and escapes into the ventilating-current.

Diffusion is also constantly going on; and as the result of this process is to produce below the plane *c d* a mixture lighter than the ventilating-current, this mixture rises along the roof and is carried away at C (fig. 1), *d* (fig. 2).

Gas is therefore constantly being removed from the cavity, partly by displacement, partly by diffusion, and its quantity is exactly equal to that entering the cavity.

If, now, the ventilating-current become fouled in any of the ways we have described, the space below the plane *c d* will be filled with the same mixture as that in the ventilating-current itself.

If we suppose that the mixture in the cavity up to a certain level *e f* be less foul, and therefore heavier than that which now forms the ventilating-current, the volume of air &c. in the space *d e f c* will be displaced, and no further escape of gas can take place from either accumulation until the whole of the contents above the line *c d* are more foul than the mixture below *c d*. As soon as this condition is fulfilled, diffusion and displacement will go on.

In order, however, that the rate of diffusion may be the same as before, the specific gravity of the whole mass must be reduced, for the square roots of the specific gravities of the mixtures above and below the plane *c d* must bear the same ratio to each other that they did before the current became fouled. This reduction of the specific gravity is, in other words, increased foulness of the air.

If, then, the mixture in the space *c b f* was previously near the firing-point, it is obvious that any impurity in the ventilating-current will cause it to approach nearer to that point, and so eventually an explosive mixture may be generated in a cavity while the ventilating-current itself is non-explosive.

It will be seen that by the above process a quantity of fire-damp may be stored up in such a cavity, which can only escape very gradually, after the ventilating-current has become purer.

It follows, then, that if an explosive mixture has been formed in places

and under conditions similar to those we have described, some time, possibly several days, must elapse after the causes which have led to its formation have disappeared before the contents of such a cavity shall have been rendered innocuous again.

We do not propose to treat specially of sudden "eruptions" of gas. Such occurrences, at least of any serious magnitude, are very rare. When they do take place in collieries where naked lights are used, the gas takes fire at the nearest such light, and at most one or two men are injured. If safety-lamps are employed, the gas may pass those which are in good order and travel on until it meets a defective lamp, when the whole explodes.

A few words on the subject of the dependence of ventilation on temperature above ground may not be undesirable.

When the temperature of the air at the surface is less than that in the mine, the phenomenon called Natural Ventilation ensues. The colder air which descends the downcast shaft is heated nearly to the temperature of the workings on its passage through the air-courses and along the working faces, and when it reaches the upcast shaft it has a temperature which is nearly constant if the workings are extensive. The temperature of the workings increases with their depth from the surface: thus, speaking generally, it is 55° at 50 fathoms, 60° at 100, 65° at 150, 70° at 200, and so on*. Now, natural ventilation ceases when the temperature at the surface is the same as that in the workings, and, moreover, as the temperature at the surface rises above that point there is an increasing resistance to artificial ventilation.

The amount of the force which produces natural ventilation is still further modified by the changes in the hygrometric state of the atmosphere.

For instance, if the tension of aqueous vapour in the air is less than that due to the temperature, water will be evaporated in a wet downcast shaft, and the air will not be able to rise in temperature as it descends, and may be actually much colder at the bottom than at the top of the shaft. These considerations will show why shallow mines are much less easily ventilated, and also much more affected by surface temperature than deep ones. Whatever be the artificial means adopted for producing ventilation at any mine, the quantity of air passing through the workings must vary with every variation of the natural force, unless the artificial power be changed at the same time. It follows, therefore, that if there be no means for ascertaining what is the actual quantity of air supplied to the workings at every instant†, a slight decrease, sufficient to bring the colliery into a dangerous state, may take place without being noticed.

* Report of the Commissioners appointed to inquire into several matters relating to Coal, 1871, vol. i. p. vii, and Report A, p. 86.

† The Select Committees on Accidents in Mines, 1835 and 1852, were aware of this, and were anxious to introduce instruments whereby a constant check might be kept over the ventilation.

The first intimation in such a case is the fouling of the currents; the artificial power may then be increased and the crisis passed without accident, but if an explosion takes place there is no trace of its cause discoverable.

One example of an explosion which happened under these circumstances may not be out of place. It occurred in winter. The temperature of the air rose for two days in succession to a higher point than it had reached for a considerable time before. On the second day, when natural ventilation would cease in many shallow mines and become sluggish in deeper ones, an explosion took place by which many men were killed.

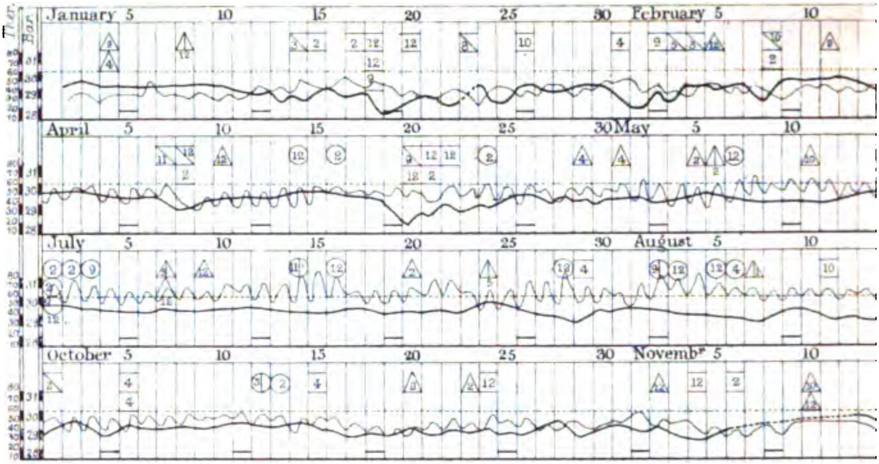
After the accident the Inspector for the district visited the mine; he described it as having strong *natural* ventilation, and looked upon the explosion as a mystery. It so happens, however, that for more than three months afterwards the temperature at the surface only reached the point which was observed on the day of the explosion on two occasions—once at midnight three days after the accident, and once on the thirty-second day after it. It is therefore very improbable that the Inspector saw the mine in at all the same state of ventilation as that which had prevailed at the time of the explosion, and hence his description of the phenomena. This explosion and some others like it are marked by triangles on our diagrams.

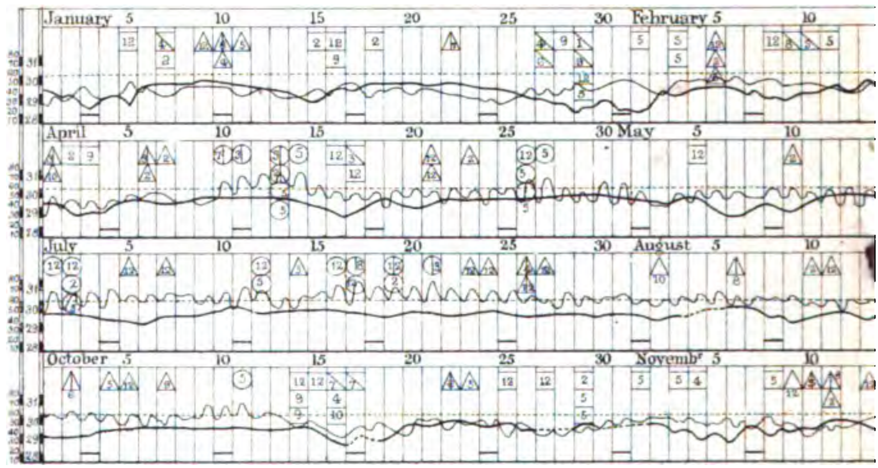
There are two groups of accidents during the year 1870, one between the 10th and 14th of February, the other at the end of December, both of which occurred when the temperature was abnormally low, and the atmospherical pressure high and steady. One of these accidents (that on February 14th in South Wales, whereby thirty men were killed) was traced to the escape of gas from a forty-acre goaf, in which it had been most injudiciously pent up until it attained so great a tension that it forced its way through the barriers into one of the air-courses, where it was ignited.

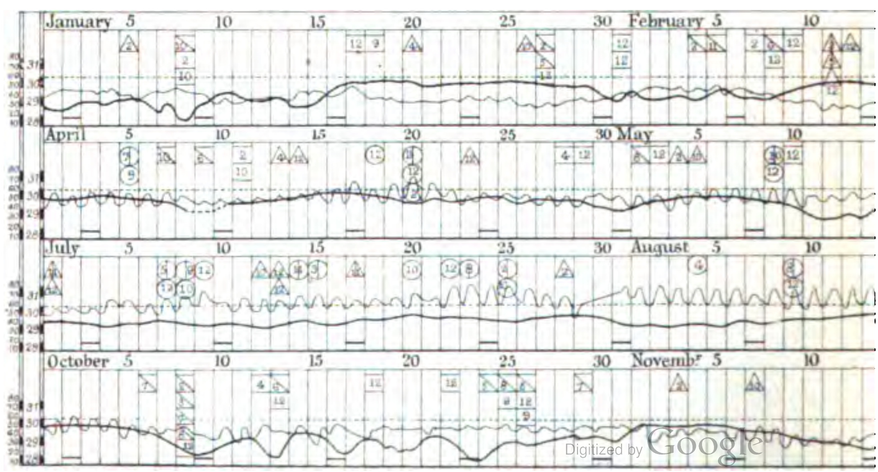
The others are probably ascribable to the so-called "sharpness" of the fire when ventilation is very active. Under ordinary circumstances, in the lower parts of small accumulations of explosive mixtures, there is a stratum of air containing less gas than is requisite to make it explosive; and when the miner slowly raises his candle into this stratum, shading all the flame excepting the very top with his hand, he is warned, by the increasing size of the "cap" (the blue flame of the gas seen on the top of the candle-flame), that there is an explosive mixture above. When the air in the mine becomes very pure this stratum disappears in many cases; there is no longer a space between the pure air, in which fire-damp cannot be distinguished, and the explosive mixture above, and the gas is then called "sharp," because it ignites without warning when a candle is raised into it.

It appears to us, in conclusion, that the evidence we submit fairly justifies the view that meteorological changes are the proximate causes of a large majority of the accidents. It will be remembered, as Mr. Dobson has observed, that the records "contain no account of cases like that of

Scott & Galloway.







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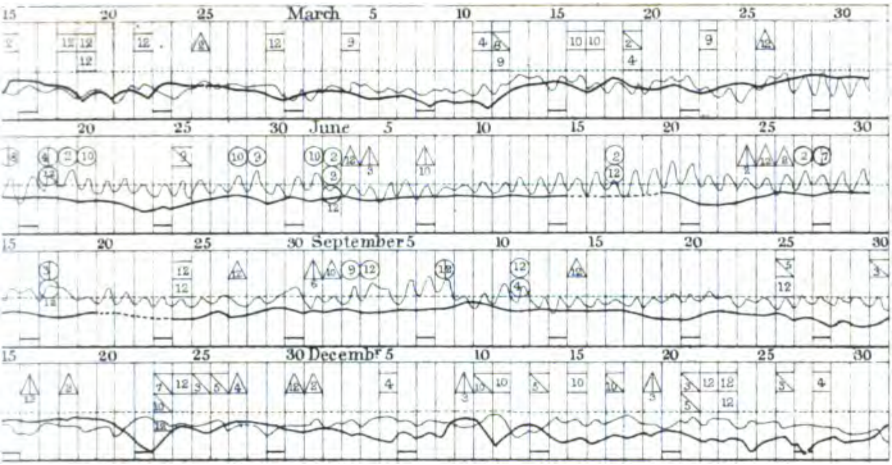

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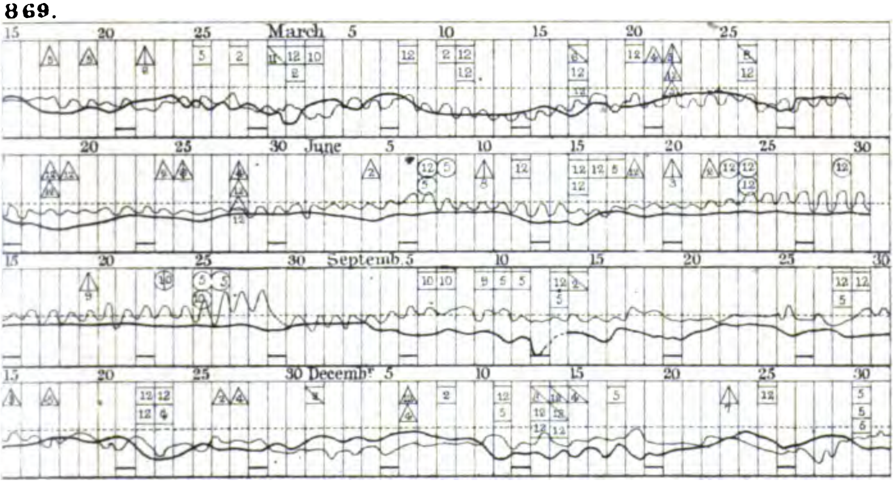

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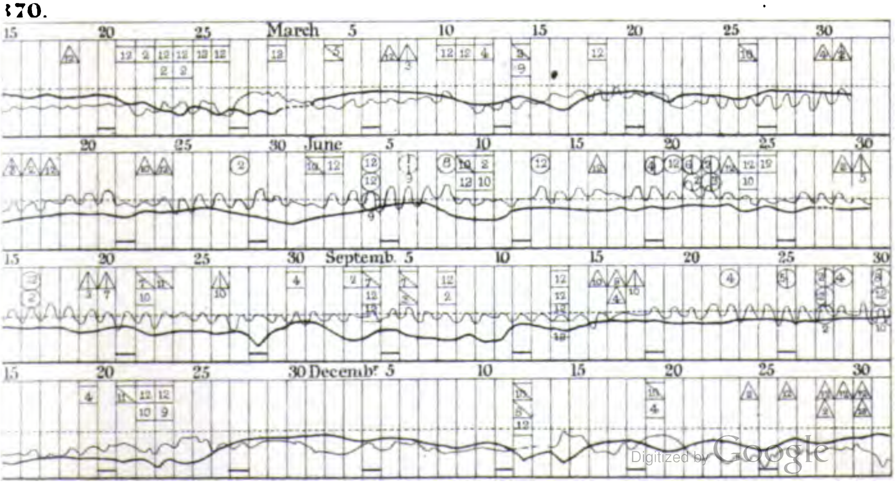

W. West & C.o

Jarrow in 1839, where mines have been filled with gas during stormy weather, and explosions have been prevented."

Whether, therefore, the barometer falls or the temperature rises it is absolutely necessary to keep a most careful watch over the amount of air passing through the workings, in order to prevent the formation of dangerous accumulations of explosive mixtures of air and fire-damp in all mines in which the margin between danger and safety is very small.

In a letter to the South-Shields Committee in 1843, George Stephenson said, referring to explosions, "Generally speaking, there has been some fault in the ventilation of the mines when accidents have occurred." A similar opinion was expressed by most of the gentlemen who were consulted when at the beginning of the year 1868 the Meteorological Committee proposed to send telegraphic intelligence of storms to colliery proprietors.

The one cry, whether we look to security against explosion, or to affording to miners an atmosphere which is respirable without injury to health, is *more air*.

EXPLANATION OF THE PLATE.

The *dark* line is the curve of the barometer, the *faint* line that of the thermometer at the Observatory of Stonyhurst, near Preston, taken from the continuous records for the years 1868, 1869, 1870.

The explosions which are apparently due to *a fall of the barometer* are shown by *squares*.

The explosions which are apparently due to *a rise of the thermometer* are shown by *circles*.

The explosions which are apparently *not caused by either agency* are shown by *triangles*.

The bar across the symbol for an explosion indicates that it was fatal.

Sundays are marked by a line near the base of the diagram.

The districts in which the respective explosions occurred are indicated by figures, the explanation of which is as follows:—

1. South Durham.
2. North and East Lancashire, or Manchester.
3. South-west Lancashire and North Wales.
4. Midland.
5. North Staffordshire, Cheshire, and Shropshire.
6. South-western (Monmouthshire &c.).
7. South Staffordshire and Worcestershire.
8. South Wales.
9. Northumberland, North Durham, Cumberland.
10. Yorkshire.
11. East of Scotland.
12. West of Scotland.

II. "On the Fossil Mammals of Australia.—Part VII. Genus *Phascolomys*; species exceeding the existing ones in size." By Prof. R. OWEN, F.R.S. Received March 25, 1872.

(Abstract.)

The author, referring to a former communication, in which the osteological and dental characters of the existing kinds of Wombat were applied in the determination of the fossil remains of extinct species resembling the living ones in size, proceeds in the present paper to give the evidences which have reached him of species of *Phascolomys*, formerly existing in Australia, exceeding in size any that are now known to live there.

The extinct species so reconstructed are described in the order in which they progressively predominate in bulk over the existing Wombats. They are referred to *Phascolomys medius*, *Phascolomys magnus*, and *Phascolomys gigas*.

The characters of these species are shown, not only by the size of ascertained adult and aged individuals, but by modifications of shape of both upper and lower jaws, and by differences of shape and of relative proportion of certain teeth. Fossils exemplifying a moderate and seemingly determinate range of variety in different individuals and probably different sexes are noticed, and the evidences of some of these varieties of the largest extinct species are figured.

In his concluding remarks the author notices that, in the case of the genus *Phascolomys*, as in that of other genera of which an adequate number of fossil evidences have been acquired, the number of species which have passed away exceeds that of the known living ones.

Of the latter the growth of knowledge has been rapid and comparatively recent, and all the species adhere to the size compatible with the burrowing habits of the first observed Wombat (*Phascolomys fossor*, Wagner). But a like habit and power of concealment cannot safely be ascribed to the larger extinct Wombats. "If," remarks the author, "we knew the Hare only by fossil remains, we should err in attributing to it the underground retreat and way of life of the Rabbit."

The evidence adduced in the present paper shows that all the larger kinds of Wombat have perished. The author, in reference to the cause of extirpation, refers to his remarks on the conditions affecting the contest for existence, as applied to the larger extinct wingless birds of New Zealand, and observes that the comparatively diminutive representative of *Dinornis*, viz. the *Apteryx*, escapes pursuit and observation by being able to avail itself, like the small existing Wombats, of an underground retreat. The paper concludes with reference to the bearings of its subject on other generalities.

April 25, 1872.

GEORGE BIDDELL AIRY, C.B., President, in the Chair.

The following communications were read :—

I. "Contributions to Formal Logic." By ALEXANDER J. ELLIS, F.R.S., F.C.P.S., F.S.A., &c. Received March 21, 1872.

(Abstract.)

This paper contains the following contributions to Formal Logic :—

Statement of the Problem of Deductive Logic, with a classification of its cases.

Method of solution for three principal cases, of which the second has not been hitherto formally considered.

New notation and calculus of logical relations.

New system of diagrams, adapted for typography, and coextensive with the system of notation for cases of composition.

Purely logical solution of all the problems within the scope of Boole's system, with complete results, and without his mathematical analogies or hazardous theories, with a proof that his primary and secondary propositions only partially, and not, as he assumes, wholly obey the same laws.

Fusion of Hamilton's judgments and De Morgan's propositions, as part of one system of assertions.

Exhaustive analysis of the syllogism.

Re-cast of the theory and notation of De Morgan's numerically definite assertions, as the general case of the logic of composition, and a legitimate application of algebra to logic.

Direct passage from the purely logical formulæ of consistency to the mathematical formulæ of Boole's system of probabilities.

The above contributions are believed to be entirely original, and are given with the least possible restatement of former theories; but, for convenience, frequent reference has been made to Boole, De Morgan, Thomson as representing Hamilton, Ueberweg, and Jevons. Boole and De Morgan have been constantly before my mind, and whatever is common to this paper and their works must be credited to them. Jevons first led my thoughts in this direction, but all resemblance between us is entirely superficial.

The problem of deductive logic as here conceived is :—*Given any assertions, to determine precisely what they affirm, precisely what they deny, and precisely what they leave in doubt, separately and jointly.*

Assertions have respect to *Coexistence* or *Succession*, or both.

COEXISTENCE generates :

Composition, arising from the coexistence of different attributes in the same thing, and the coexistence of the same attribute in different things;

Combination, arising from the coexistence of different things, objectively or subjectively, in the same aggregate;

Consistency, arising from the coexistence of different events affirmed by different assertions.

SUCCESSION generates: *Logical inference, Sequence in Space, in Time, and in Action.*

Mixed cases arise from combining assertions of different kinds.

Composition, combination, and consistency only are considered in the present paper. Composition constitutes the primary, and consistency the secondary propositions of Boole. Combination has not been distinctly recognized in logical works, but is constantly implied in Boole's and De Morgan's treatments of Composition, whereby its real character has been overlooked. Logical Inference is partially considered in the treatment of Composition, Combination, and Consistency. Sequence in Space, Time, and Action is not treated at all.

Thomson's, De Morgan's, and Boole's notations are carefully interpreted. Particular attention is paid to Boole's results for Consistency, and the nature of the error which he committed in accommodating those results to Composition, together with the value of his accommodated results, is exactly determined. The ascertainment of these errors by a fundamental reconsideration of the bases of the relations of Composition and Consistency, and a purely logical method of obtaining precise results, forms the distinctive character, as it was the special object, of the present investigation.

II. "On a supposed Periodicity in the elements of Terrestrial Magnetism, with a period of $26\frac{1}{3}$ days." By GEORGE BIDDELL AIRY, Astronomer Royal. Received March 26, 1872.

In a paper published in the 'Proceedings of the Imperial Academy of Sciences of Vienna,' vol. lxiv., Dr. Karl Hornstein has exhibited the results of a series of observations which appeared to show that the earth's magnetism undergoes a periodical change in successive periods of $26\frac{1}{3}$ days, which might with great plausibility be referred to the rotation of the sun.

It appeared to me that the deductions from the magnetic observations made at the Royal Observatory of Greenwich, and which are printed annually in the 'Greenwich Observations,' or in the detached copies of 'Results of Magnetical and Meteorological Observations made at the Royal Observatory of Greenwich,' would afford good materials for testing the accuracy of this law, as applicable to a series of years. The mean results of the measured hourly ordinates of the terrestrial magnetic elements are given for every day, and it is certain that there has been no change of adjustments of the declination and horizontal-force instruments in the course of each year. For the horizontal-force instrument the tem-

perature of the room has been maintained in a generally equable state, and in later years it has been remarkably uniform.

It is easy to see that an error of a single day, or of a large fraction of a day, in the beginning of each period, is of no importance, provided that the errors are not permitted to accumulate. It was allowable, therefore, to take successive periods of 26, 26, 27, 26, 26, 27, &c. days; and in instances when a single day was omitted, or even two days, no sensible error would be introduced by interpolating between the numbers for the days immediately preceding and following the omitted days.

The years selected for this examination were 1850, 1851, 1852, 1868, 1869, 1870; and the beginning of the first period in each year after the first was thus found:—Fourteen periods of $26\frac{1}{3}$ days each amount to $368\frac{2}{3}$ days. For convenience after completion of the annual winter adjustments, the first period in 1850 was made to commence on January 17; therefore the first period in 1851 was commenced on January 21, and that in 1852 on January 25. Similarly, the first periods in 1868, 1869, 1870 commenced on January 1, 4, and 8 respectively; and the beginnings in the three later years are not unconnected with those in the three earlier years: for, from 1852, January 25, to 1868, January 1, are 5820 days, and 221 periods of $26\frac{1}{3}$ days each are $5819\frac{2}{3}$ days; but as the years are widely separated, and a small error of period would produce a large discordance, it has appeared best to exhibit the results of the two three-years' groups separately.

Some periods, in which there were unusually large interruptions, or which were partly occupied with experiments, were omitted entirely. The following is a complete list of periods omitted:—In 1850, that beginning with December 24 for horizontal force; in 1851, that beginning with March 14 or western declination, and those beginning with March 14, June 28, July 24, for horizontal force; in 1852, those beginning with February 20, May 9, December 6, for both elements; in 1868, those beginning with February 23 for declination, and January 1, January 27, February 23, and December 8 for horizontal force; in 1869, those beginning with October 21 and December 12 for both elements; and in 1870, those beginning with June 15 and December 16 for declination, and that beginning with December 16 for horizontal force. Interpolations of three days occur only in the following instances:—1850, (dec.) Feb. 4–6, (h. f.) Feb. 9–11, July 23–25; 1851, (dec.) Feb. 18–20, Oct. 20–22, (h. f.) June 9–11; 1852, (dec.) Feb. 7–9; 1868, (dec.) Feb. 15–17, (h. f.) none; 1869, (dec.) June 6–8, (h. f.) June 6–8; 1870, (dec.) Sept. 24–26, (h. f.) Sept. 24–26.

The mean values of each element for each progressive day in every period of the several years, uncorrected for the proportional part of secular change through the 26 days, and omitting the imperfect 27th day, are as follows:—

Progressive day of each period.																										
Year.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26
Declination (omitting 22° in 1850, 1851, 1852; 20° in 1868, 1869; 19° in 1870).																										
1850.	23′·62	22′·91	22′·96	23′·23	23′·05	23′·13	23′·07	23′·00	22′·73	23′·64	23′·39	23′·56	23′·19	22′·83	23′·29	23′·06	22′·75	22′·54	23′·03	23′·15	23′·21	23′·32	23′·24	23′·09	23′·47	23′·17
1851.	17·95	18·25	18·26	18·18	17·92	18·03	17·91	17·90	18·32	18·32	18·48	19·25	19·22	18·79	18·37	18·35	18·38	18·17	18·11	18·45	18·64	18·59	18·36	18·49	18·53	18·63
1852.	17·93	17·88	18·27	17·93	17·47	17·70	17·66	17·36	17·49	17·30	16·86	16·82	16·92	17·16	17·28	16·99	17·59	17·38	17·18	17·11	17·39	17·01	17·23	16·90	16·97	17·20
1868.	13·35	13·16	12·83	13·2	12·90	13·09	13·42	13·27	13·17	12·57	12·47	12·59	12·50	12·84	12·99	12·75	12·75	12·77	12·66	12·67	12·78	13·05	13·11	13·05	12·95	13·34
1869.	5·53	5·34	5·16	5·10	4·88	4·96	4·93	4·40	4·78	4·60	5·02	4·50	4·74	4·78	4·62	4·87	4·53	4·47	4·39	4·84	4·57	4·54	4·59	4·50	4·62	4·69
1870.	53·41	53·28	53·03	53·53	53·13	53·27	54·05	53·62	53·54	53·42	53·21	53·27	53·36	53·22	53·37	53·29	53·18	53·25	52·73	52·98	53·09	52·91	52·78	52·33	52·65	52·51
Horizontal Force (omitting ·11000 in 1850, 1851, 1852, and ·14000 in 1868, 1869, 1870).																										
1850.	310	288	242	269	262	278	259	293	292	280	252	250	208	250	256	245	272	236	234	224	221	251	235	243	236	256
1851.	582	557	545	538	561	544	530	555	530	528	538	565	598	586	568	578	565	553	547	522	530	556	545	539	580	559
1852.	381	357	332	325	348	319	345	350	321	386	404	410	388	353	397	388	412	364	356	357	364	353	352	342	357	373
1868.	283	284	299	307	290	275	265	258	222	265	278	248	249	247	280	305	283	298	303	298	300	298	293	314	321	317
1869.	299	302	284	273	267	270	291	281	295	267	297	287	304	307	284	277	281	313	286	277	273	296	275	243	258	247
1870.	856	853	848	861	888	885	906	930	920	918	941	942	967	955	966	943	938	922	925	905	908	902	882	836	885	892

Taking the means of these numbers separately for the groups of years 1850–1852 and 1868–1870, and applying the proportional parts of secular correction, at the rate of $+0'{\cdot}62$ for 26 days in western declination, and at the rate of $-{\cdot}00013$ for 26 days in horizontal force, we have the following results:—

Range of years.	Progressive day of each period. 1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26
Western Declination (omitting 22° 10′ in the first range of years, and 20° in the second).																										
1850–1852.	9′·83	9′·70	9′·88	9′·85	9′·58	9′·74	9′·70	9′·59	9′·71	9′·97	9′·83	10′·15	10′·07	9′·91	9′·99	9′·84	9′·96	9′·78	9′·88	10′·04	10′·24	10′·16	10′·15	10′·06	10′·25	10′·29
1868–1870.	4·10	3·95	3·72	3·95	3·74	3·89	4·28	3·93	4·03	3·75	3·82	3·72	3·82	3·93	4·00	4·04	3·88	3·92	3·70	3·97	3·97	4·02	4·03	3·86	4·00	4·13
Horizontal Force (omitting ·11300 in the first range of years, and ·14400 in the second).																										
1850–1852.	124	101	72	75	88	77	75	95	77	93	93	102	92	89	99	96	107	75	69	58	61	76	65	63	78	83
1868–1870.	79	83	76	78	80	74	84	86	75	78	100	86	101	96	102	100	92	102	95	83	83	88	71	69	75	72

The mean for declination in 1870 and, still more remarkably, the mean for horizontal force in 1870 appear to exhibit an increase about the 14th day. But I do not remark in the other means, either as given in numerals or as projected in curves, any thing to support the idea of an inequality periodical in the $26\frac{1}{3}$ days. It might almost be suspected that the secular changes used in the period 1850–1852 are too large; but no alteration of these renders the inequality of $26\frac{1}{3}$ days more probable.

Dr. Hornstein's investigation was limited to observations made in 1870.

Postscript. Received May 9, 1872.

As the diurnal changes of the magnetic elements appear to be due to the action of the sun, it seemed probable that any irregularity in that action depending on the face of the sun presented to the earth might be principally conspicuous in the diurnal change. I therefore thought it prudent to avail myself of the computations of diurnal change of western declination already prepared at the Royal Observatory. The following numbers have been computed in exactly the same manner as those for absolute western declination.

Total diurnal change of western declination.

Year or range of years.	Progressive day of each period.																								
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26
1850.	12′·16	13′·01	14′·14	12′·69	11′·29	11′·94	13′·60	10′·32	12′·21	13′·33	11′·89	12′·63	12′·81	12′·84	12′·55	13′·11	13′·06	13′·23	14′·04	14′·13	14′·29	11′·64	12′·46	12′·35	12′·78	14′·29
1851.	11·83	10·58	10·96	11·21	11·45	13·75	10·99	13·14	12·82	12·31	11·23	11·92	12·92	13·15	11·04	13·27	11·62	10·41	11·31	11·77	10·41	11·02	11·54	10·25	11·65	12·19
1852.	12·25	14·95	14·65	14·52	12·03	11·66	13·15	14·35	15·73	12·82	14·16	13·47	13·82	14·43	13·45	15·15	12·43	14·59	14·68	13·27	13·55	13·79	13·18	13·35	13·35	12·73
1850–1852.	12·08	12·85	13·25	12·81	11·61	12·45	12·58	12·60	13·59	12·82	12·43	12·67	13·18	13·47	12·35	13·84	12·37	12·74	13·34	13·06	12·75	12·15	12·49	11·88	12·59	13·07
1868.	11·39	10·68	11·89	12·14	11·86	13·45	12·58	12·78	13·95	11·45	13·47	15·36	13·75	11·09	10·05	11·01	10·30	10·62	10·25	10·35	9·70	10·28	11·00	11·02	12·22	11·58
1869.	11·46	11·61	11·55	13·58	12·93	14·49	12·87	13·09	13·30	14·32	13·78	14·03	14·81	14·18	14·03	14·28	13·09	13·93	14·28	14·13	12·68	11·72	12·68	12·42	13·33	12·53
1870.	16·01	14·93	15·24	15·23	13·88	15·19	15·48	16·09	15·31	14·91	14·03	13·70	15·02	14·49	13·07	14·70	14·27	13·48	16·25	15·40	15·99	15·87	16·06	15·12	16·43	15·39
1868–1870.	12·95	12·41	12·89	13·65	12·89	14·38	13·64	13·99	14·19	13·56	13·76	14·36	14·53	13·25	12·38	13·33	12·55	12·68	13·59	13·29	12·79	12·62	13·25	12·85	13·99	13·17

In 1870, the diurnal change appears to be less from the 10th to the 18th day than in other parts of the period, but the difference is small. In 1869 it appears to be larger at the same part of the period. In 1868, the small values occur principally towards the end of the period.

Presents received April 11, 1872.

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April 25, 1872.

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May 2, 1872.

The EARL OF ROSSE, D.C.L., Vice-President, in the Chair.

In conformity with the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair as follows:—

Prof. William Grylls Adams, M.A.
Andrew Leith Adams, M.B.
Frederick Le Gros Clark, F.R.C.S.
Prof. John Cleland, M.D.
Prof. Michael Foster, M.D.
Prof. Wilson Fox, M.D.
Arthur Gamgee, M.D.
Rev. Thomas Hincks, B.A.
Prof. William Stanley Jevons, M.A.
Prof. George Johnson, M.D.
Prof. Thomas Rupert Jones.
Major Thomas George Montgomerie, R.E.
Edward Latham Ormerod, M.D.
Edward John Routh, M.A.
William James Russell, Ph.D.

The following communications were read:—

I. "On a new Great Theodolite to be used on the Great Trigonometrical Survey of India, with a short Note on the performance of a Zenith-Sector employed on the same work." By Lieut.-Colonel A. STRANGE, F.R.S., Inspector of Scientific Instruments, India Department. Received April 25, 1872.

On the 28th of February, 1867, I had the honour of submitting to the inspection of the Royal Society a Transit-Instrument and a Zenith-Sector made for the Great Trigonometrical Survey of India, to be used respectively for the determination of longitude and latitude on that work. These were one instalment of the following list of geodesical and astronomical instruments which the Secretary of State for India deputed me to design and superintend under construction:—

One GREAT THEODOLITE, with a 3-feet Horizontal Circle. By Messrs. Troughton and Simms.

Two ZENITH-SECTORS. By Messrs. Troughton and Simms.

Two 5-FEET TRANSIT-INSTRUMENTS. By Messrs. T. Cooke and Sons, York.

Two SMALLER TRANSIT-INSTRUMENTS (*German form*). By Messrs. T. Cooke and Sons, York.

Two 12-INCH VERTICAL CIRCLES (*German form*). By Messrs. Repsold, Hamburg.

Two GALVANIC CHRONOGRAPHS for registering Transit-Observations. By MM. Secretan and Hardy, Paris.

Three ASTRONOMICAL CLOCKS. By Mr. Charles Frodsham.

The Zenith-Sector was, shortly after its exhibition to the Society, forwarded to its destination, as have been more recently the other Zenith-Sector, one of the 5-feet Transit-Instruments, the two 12-inch Vertical Circles, the two Galvanic Chronographs, and the three Astronomical Clocks. Of these, the first forwarded Zenith-Sector alone has been used in actual field-operations. It has been employed during two seasons in determining latitudes in Southern India, by Capt. J. Herschel, R.E., F.R.S., of the Great Trigonometrical Survey. One of my main objects in designing the instrument, which is entirely unlike any of its class, was to qualify it for more rapid observing than had hitherto been possible. In this respect it has been successful. Capt. Herschel reports that he can observe with it a series of stars differing only five minutes of time in Right Ascension. Each star is observed twice in reversed positions of the telescope at the same culmination, in the manner prescribed by the Astronomer Royal for the Zenith-Sector designed by him for the British Ordnance Survey. Each of these two reversed observations involves two settings of the telescope in altitude, four microscoped, two level, and one micrometer reading. To admit of all these operations being performed within five minutes of time with the deliberation requisite for observations aiming at fractions of a second, demands not only convenience of instrumental construction, but the greatest system and efficiency in the observatory arrangements, and a very high order of skill on the part of the observer.

In the latest Report on the Survey, that for 1870–71 by Major Montgomerie, R.E., Officiating Superintendent, that officer states that "Captain Herschel's further experience with the Zenith-Sector has on the whole confirmed his first impressions as to its excellence." Captain Herschel reports on an anomaly in the level readings, amounting, as I understand through private sources, to about $0''\cdot3$, the cause of which he has not yet been able to detect. It may possibly, as he suggests, be partly due to the rigidity of the instrument as a whole not being absolute. I am myself inclined to suspect that imperfections in the levels themselves may in some measure contribute to the observed discrepancies. The levels have been reported, and are known to me, as deficient both in uniformity of run and in sensibility; they are certainly not on a par with the graduation, which is superb, or with the optical power of the telescope and microscopes. The accuracy of the instrument is thus limited by that of the levels, which are its weakest member. I may here state incidentally that I have not been able to procure levels in England which come up to the standard now required, and I must therefore have recourse to foreign makers, who have paid more attention to these adjuncts than, as it seems, it is worth the while of our English artists to do.

Comparing the facility of working the Zenith-Sector and the former ·onomical Circles, Captain Herschel says:—"The Sectors are competent to turn out at least double the amount of work *of the same order*," adding, "at this rate two or three years' work would equal in amount the

whole results up to the date of the arrival of the Sectors; and ten years (a comparatively short period for which to arrange a system of observation on a matter of this magnitude) will see us in a position to look back on the arrival of the Sectors as on the commencement of a new era."

I now pass to the Great Theodolite, which I do myself the honour this evening to expose to the scrutiny of the Royal Society. A brief history of the design of this instrument may here with propriety be recorded.

There are at present at the disposal of the India Survey Department two instruments of the same order as the one under consideration, namely, Theodolites having Horizontal Circles 3 feet in diameter, the largest dimension hitherto employed,—hence the term Great Theodolite. There are, I believe, not more than four in existence of that size—one by Ramsden, with which the greater part of the principal triangulation of Great Britain was executed, the two Indian instruments, and the one now before the Society.

Of the two Indian instruments, one was made by Messrs. Troughton and Simms about forty-five years ago, its Horizontal Circle bearing divisions cut by the hand of the celebrated Edward Troughton himself. The other was constructed and divided in India by the late Henry Barrow, then attached as instrument-maker to the Survey, its design being the work of the late Sir George Everest, then Surveyor-General of India. There are also in use with the Department several theodolites of 2-feet diameter, and of course many of smaller sizes.

The principles of construction, the advantages, and the defects of all these instruments were a constant theme of discussion to the officers of the Department, who being accustomed, according to the excellent system of the Great Trigonometrical Survey of India, to take every observation personally, have thereby at all times been led to display the most critical fastidiousness in every thing relating to the apparatus with which use has made them perfectly familiar.

Sir Andrew Scott Waugh, R.E., F.R.S., then Surveyor-General of India, having collected all the experience of his officers, including of course his own, gathered during many years of actual work in the field, combined the whole into a "*Project and Specification of a new Great Theodolite,*" which in 1855 he placed in my hands for the purpose of guiding me in the preparation of a design, with working drawings and a fully detailed specification. The project in question fixes all the general principles intended to be kept in view, and is ample for that purpose; but it enters into very few details, merely pointing out what is required, without definitely stating how it is to be attained, and it contains no drawings whatever, and only three or four slight diagrams. The part I had taken in the previous discussions had supplied me with a knowledge of the desiderata not actually described, knowledge which of course was also in great measure derived from the fact that I had worked for five years with Troughton's Great Theodolite, and subsequently with other excellent instruments.

At the time the task of executing this design was entrusted to me, I was commanding a party of the Great Trigonometrical Survey engaged in carrying a chain of principal triangles along the eastern coast of the Peninsula, a work known as the "Coast Series." I at once, during such leisure as I could snatch from these primary duties, began the design; but I soon found that it was quite impossible to do justice to so difficult a task whilst the mind and time were engrossed by matters of more pressing urgency, and of at least equal difficulty and importance. Short and broken periods did not admit of the continuity of thought and attention that were indispensable, and the work made very slow progress, besides needing constant revision in consequence of oversights and omissions caused by such desultory application. This will be understood, by any one who has gone through the same process, when I mention that the conception I had formed of my task was that I should execute, in the most minute detail, working drawings to scale of every part of this complex structure, in plan, section, and elevation, accompanied not only by an ample written descripton of every part, but also an explanation of the grounds on which the numerous novelties introduced had been adopted; so that when the whole was in the maker's hands, he need do little more than reproduce the drawings in metal by simple measurement, turning to the specification in all cases of doubt. The intention was that the whole should be sent home to be executed without any personal superintendence by the designer.

In April 1857, being at work in the Goomsoor Hills, a notoriously unhealthy tract, I was struck down by jungle-fever, being nearly the last of my camp of about 200 followers, only one of whom escaped, to succumb to that dreadful malady. A few months afterwards I was sent, under medical advice, to the Neilgherry Mountains for the recovery of my health; this gave me the leisure requisite for my design, which otherwise I might never have enjoyed. For several months I did little else but work at my drawings, which I now offer to the notice of the Society. One of these alone, the complete vertical section, occupied me nearly incessantly about three months, involving as it did the reconsideration of every other drawing in order to render the whole consistent.

At the expiration of my leave on medical certificate, the design, though very far advanced, was not complete; I had to lay it aside to resume my ordinary field duties on the Coast Series triangulation. In 1869, on my promotion to a regimental majority, I quitted the Survey in accordance with the regulations of the service, and, after twenty-six years' continuous residence in India, returned to England. Feeling a natural reluctance to leave the completion of my design to other hands, and, indeed, doubting whether any one unacquainted with my views could have completed it satisfactorily, I took it with me, hoping to finish it, as I ultimately did, at home, after my retirement from the army.

In 1862 I was directed oy the Secretary of State for India to superin-

tend the carrying out of this design and that of the other instruments already enumerated.

It is now ten years since the work was placed in the hands of Messrs. Troughton and Simms—a long period certainly. The delay has arisen from several causes—from the removal of the factory of that firm to Charlton, from the necessity for various modifications and some experiments, particularly on aluminium bronze as the material for some portions of the instrument, from the time occupied in the desiguing, supervising, and testing the other numerous and important instruments which I have enumerated, and, finally, from the time which my duty of superintending the whole scientific instrument supply of India necessarily occupies.

I cannot here attempt a full description of the instrument. I trust that such a description may at some future time be executed by myself, or by some officer of the Survey Department; but, for my own part, the leisure necessary for such an undertaking, although all the necessary materials are at hand, is denied me at present.

I will, however, indicate some of those salient features which may be rendered intelligible without illustrative figures.

The principal dimensions are as follows :—

Horizontal Circle.—3 feet in diameter, divided on silver to 5′, and read to tenths of a second by five equidistant micrometer microscopes.

Vertical Circle.—2 feet in diameter, similarly subdivided by two micrometer microscopes for ordinary terrestrial work; but by four, whose positions can be shifted, for astronomical work.

Telescope.—Aperture 3·25 inches; focal length 36 inches; supplied with two distinct eye-ends, carrying respectively a vertical and a horizontal parallel wire micrometer, and furnished with both bright and dark field illumination.

Vertical Axis.—A truncated cone of steel, base downwards, 10·6 inches high, and 3·31 inches and 2·06 inches in diameter at the base and summit respectively, the flange being 4·4 inches in diameter, and of the "isolated" form described by me in the Royal Astronomical Society's Memoirs, vol. xxxi.

Horizontal Axis.—13·18 inches between the shoulders of the pivots, cast in one piece of gun-metal with steel pivots.

The Vertical-Axis socket and the five Horizontal-Microscope arms are cast in one piece of aluminium bronze.

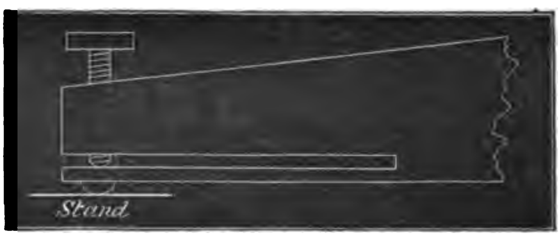
The Stand has three massive mahogany legs, braced together with horizontal and oblique bars of wrought iron, wooden braces having been found, in great hygrometrical changes, to impart azimuthal disturbance. I believe this to be the first stand for such instruments in which the means for levelling approximately are completely worked out. Within each leg, divided vertically for that purpose, is a long very substantial square-threaded upright screw actuated by a ratchet-wheel and endless screw, and capable of being immovably clamped, after adjustment, at points 15 inches asunder. On the upper conical ends of these three screws rest

three inverted radial grooves formed in the lower surface of a massive cast-iron circle, which is thus supported by the three elevating screws without being attached to them, and which is therefore able to accommodate itself to expansional changes without restraint. The upper surface of the cast-iron circle is turned flat and true, and receives the three feet of the instrument. It has three lateral screws for centering the instrument over the station-mark, as in existing large theodolites; it has also three rollers, capable of being pressed upwards by strong spiral springs thrown in and out of action by a cammed lever, this arrangement, already in use elsewhere, being intended to facilitate the setting of the horizontal zero.

The Feet-screws.—These are, as usual, three in number; no part of such instruments has been so entirely neglected as this. Whilst the Circles, Micrometers, and Levels indicated fractions of a second of arc, the foot-screw remained so coarse and primitive in form that the due levelling of the instrument was a matter of tedious difficulty, accomplished after all by a sort of adroitness helped by accident.

The first desideratum was that all clamping of the foot-screw after adjustment should be rendered unnecessary, since no clamping arrangement hitherto tried could be used without disturbing the level after adjustment. What was wanted, therefore, was that no azimuthal deviation of the mass of the instrument should ensue from an unclamped foot-screw, however loose.

The idea, as it presented itself to my mind, was that the effect which would be produced by vertical expansion of the end of the tribrach should be imitated if possible. After much thought I realized this idea by a very simple contrivance, the principle of which the annexed diagram will make plain. The tribrach arm is slit horizontally near its lower surface. This leaves a kind of long flat



tongue of metal, which, being of considerable width, has great horizontal rigidity, but, being comparatively thin, is easily bent vertically. The outer end of the tongue rests on the stand; the foot-screw passes through the solid upper part of the tribrach, but not through the lower tongue.

It is evident that when the screw is turned inwards with the screwing-motion, the solid end of the tribrach will be raised, the slit being widened by the vertical yielding of the tongue, and *vice versâ*. But since the end of the screw does not rest on the stand, but on an intermediate plate or tongue, which is actually a portion of the tribrach itself, it is clear that if a lateral force be applied to the tribrach, no motion will be caused thereby, however loose the screw may be, so long as that force is less than the lateral rigidity of

the plate or tongue. The lateral force generated by turning the instrument in azimuth, when taking observations, is greatly within this limit. This arrangement may be found useful in other cases. It is perfectly successful; but it is only available where a moderate range of vertical movement is needed. In the present instance, as the stand on which the instrument is supported is always first made sensibly level, the vertical range of the foot-screw need not be more than $\frac{1}{100}$ of an inch; but in practice this is capable of being extended, without fear of injury to the parts, to about $\frac{1}{10}$ of an inch.

In the actual construction, the tongue above described as formed by slitting the tribrach, is, in fact, a separate plate, similar in plan to the tribrach, and firmly bolted to the centre of the instrument.

The second desideratum in the feet-screws was delicacy and certainty of action; this I attained by applying to them a clamp and tangent-screw arrangement very similar in principle to that sometimes applied to circles. Although the feet-screws themselves are very coarse, having about eight threads only per inch, the arrangement is such that one entire revolution of the slow-motion tangent-screw alters the level only one second of arc. Hence the foot-screw, though coarse and strong enough to bear the great weight imposed on it, is for the first time in keeping, in point of refinement, with the most delicate parts of the instrument.

The Horizontal Circle and its Micrometers.—The disposition of these most important parts has been the subject of much controversy. Two arrangements are possible:—(1) that called "the flying-circle" plan, in which the microscopes are immovably attached to the basis of the instrument, the circle revolving with the telescope; and (2) "the flying-micrometer" plan, in which the circle is fixed and the micrometers revolve. The question was carefully considered, and the balance of advantages appearing to be in favour of the second plan, that was prescribed in the project, and has been adopted. I have no doubt as to its superiority for instruments of this kind.

The horizontal circle is attached at the centre to the tribrach, and is elsewhere perfectly free.

The Guard Circle.—This addition was made to another instrument by Sir A. S. Waugh, late Surveyor-General, some time before the present design was commenced. I saw it subsequently in an Altazimuth by Brunner, of Paris, in the Great Exhibition of 1862. It was, I believe, arrived at in both cases quite independently. It consists of a second horizontal circle, exterior to and concentric with the circle carrying the working graduation already mentioned. There is a space of about $\frac{1}{10}$ of an inch all round between the two circles, and the upper plane of the outer guard circle stands about the same quantity above that of the inner principal or working circle. The guard circle has supporting radii of its own, quite independent of those of the inner circle. The guard circle has four objects:—(1) it protects the working circle from accidental injury; (2) it tends to distri-

bute changes of temperature uniformly over the circumference of the working circle; (3) it receives the clamp and tangent-screw, leaving the working circle absolutely free from contact at all times; and (4) it bears a strongly cut set of divisions, more visible to the naked eye than those of the working circle, which are exceedingly fine, and therefore inconvenient for setting the instrument approximately in azimuth.

Relieving Apparatus.—The moving parts of the instrument, namely, the pillars and their foundation-plate, the vertical-axis socket and horizontal-microscope arms, the telescope and the vertical circle, with its adjuncts, constitute a very ponderous mass, weighing 284 lbs.* If the whole of this great weight were allowed to bear on the flange of the vertical axis, the friction would be so great as almost to cause cohesion. To obviate this in similar cases, as in that of Troughton's Great Theodolite, internal rollers were introduced, pressed upwards by means of spiral springs against the lower surface of the moving mass. But this arrangement was imperfectly carried out in Troughton's instrument; and it is clear that at that time the action of such an antifrictional apparatus was not fully understood. There were three soft brass rollers with central holes revolving on axes fixed immovably in a circular ring, which ring was pressed upwards by three spiral springs.

The late Sir George Everest, in his work on the 'Measurement of the Meridional Arc of India,' p. cvii, states that when the instrument was first received "angles taken with it could not be depended on to within 50 seconds of the truth,"—a defect which was found to be mainly attributable to the inequality of tension of the three spiral springs, which exerted forces respectively in the proportions 8, 11, 15. Shortly after I joined the Survey Department, I, with the consent of its head, made a new set of rollers myself, the axes of which were fixed to them, instead of to the frame, the axes revolving in Y bearings. This improved the levelling of the instrument; but it was not possible, with every care, to maintain perfect equality of tension of the three springs; and this part of the instrument always caused anxiety.

The arrangement adopted for the present instrument is as follows:—The three rollers are of larger diameter than before, and they are of steel, finished with extreme care. They do not support the weight on their axes at all, but on their circumferences, precisely as is now customary with heavy revolving observatory-domes. The friction is therefore no longer of the rubbing, but of the rolling kind. The three rollers are connected, as in domes, by a light ring, which performs no function but that of keeping them equidistant. Means are also provided for restricting the path of the rollers to a circle perfectly concentric with the axis of motion of the instrument. Finally, the rollers are pressed upwards by no less than forty, instead of only three, spiral springs, by which multiplication a generally uniform tension is much more easily maintained, the mean tension of each spring

* This has been recently increased to 299 lbs. by additions.

being about 6·2 lbs., and of course $\frac{1}{40}$ instead of $\frac{1}{5}$ of the whole. An external deep groove, filled with oil, into which a thin circular rib dips, effectually excludes dust from the relieving apparatus and from the vertical axis.

The proper amount of relief has been a question much discussed. Sir George Everest, in his work above quoted *, fixes it, somewhat arbitrarily, at $\frac{3}{4}$ of the whole moving mass. This cannot be accepted as a general rule, because, if the mass were very great, $\frac{1}{4}$ of its amount might be sufficient to cause a most destructively abrasive action.

My method of adjusting the relief is as follows :—I assume that, for the purposes of horizontal angles only, the azimuthal motion cannot possibly be too free. I therefore first make the relieving force nearly equal the weight of the moving mass. Then I try the levelling of the instrument. With such excessive relief this is sure to be unsteady. I then cautiously diminish the relieving force until I obtain steady levelling. When I have done this, I know that I have given all the relief which is possible in the existing construction, consistently with the general purposes for which the instrument is required. In the present instrument the result stands thus :—

Weight of moving mass................	284 lbs. †
Relief (40 springs × 6·2 lbs.)	248 „

which leaves nearly $\frac{1}{8}$ of the moving mass active, instead of $\frac{1}{4}$, as prescribed by Sir G. Everest.

Horizontal Tangent-screw.—This, although apparently a mere subordinate agent, is really one of great importance and presenting some difficulties. There is always "loss of motion" in every screw unprovided with special means to prevent it. Loss of motion increases the difficulty of observation and also causes insecurity ; for the tangent-screw is the link which connects the moving with the fixed parts, and the length of this link, once adjusted in observation, should be unalterable. Several methods of obviating loss of motion have been tried ; and though many of them are suitable to small instruments, none have been quite satisfactory with large ones. The matter attracted great attention when I was in the Indian-Survey Department, and I was much engaged in studying it. The general conclusion I came to was that, however a spring might be applied, its tension ought to be constant and invariable. Hitherto this had not been the case, an ordinary *external* antagonizing spiral spring having been used, the compression of which was continually being varied by the action of the tangent-screw. It was found that after the observation had been made the spring continued to act, which was fatal to the observation. And the evil was brought home to this agent by the fact that in a particular instrument the springs attached

* Page cviii.

† Since this adjustment was effected, the weight of the moving mass has been increased by additions to 299 lbs., and the springs will have to be readjusted ; but the *absolute* weight left active will probably remain nearly as at present.

to two different tangent-screws having been accidentally made to act in directions opposite to each other, the observation was disturbed in opposite ways.

In the present instrument I have adopted an arrangement which, I believe, is not new in principle, namely the "divided-nut" principle, sometimes used in machinery to prevent loss of motion. The block into which the tangent-screw is tapped is divided transversely, and the two halves are forced asunder, and therefore against the contrary sides of the screw-threads, by four *internal* spiral springs. The tension of these springs is necessarily constant, and therefore not subject to the disturbance and slow recovery of elastic force unavoidable in an *external* spring. Means are supplied for regulating the tension of the four springs, which must be a little in excess of the force necessary to move the revolving mass, without taking the parts to pieces. This arrangement I believe to be on the whole satisfactory.

Bubble-trier.—Experiments have constantly to be made to determine the angular value of the scales of levels of such instruments. Hitherto there have been no conveniences for this purpose. I have supplied a frame attachable at will to the telescope, on which the level under experiment can be laid, in order to compare its scale with the angular indications of the vertical circle.

Bearing of the Transit-Axis pivots.—Hitherto these have always been capable of adjustment in order to level the axis: but to this two objections exist:—first, there is always ground for fearing lest the adjustable parts should be left loose, in which case, owing to their distance from the centre of motion, they would be disturbed by the momentum generated in rotating the instrument in azimuth (since this would alter the relation between the telescope and the horizontal circle, observed angles would be vitiated to the extent of such disturbance); secondly, adjustable bearings must necessarily support the pivots at points and not surfaces, and consequently tend to wear them into grooves.

For these reasons the bearings are immovably fixed and finally adjusted by the maker by grinding. They are also of what is known as the segmental form, first introduced, I believe, by the Astronomer Royal when designing the great Greenwich Transit-Circle, a form which supports the pivot throughout its length and over a considerable surface, and which has been found at Greenwich to wear the pivots so equally that no sensible change in their form in the course of many years has been detected. Still the application of the non-adjusting principle to a large portable instrument must be considered as an experiment. Should the horizontality of the transit-axis undergo, as it may, a gradual change, the officers of the Survey will have to restore the adjustment by means of the grinder supplied with the instrument, an operation, no doubt, of some delicacy, even in the hands of an experienced mechanic, but which, I trust, nothing but accidental violence to the parts will ever render necessary.

Level Mountings.—The mode in which the levels are mounted was contrived by me, at the request of Sir Andrew Scott Waugh, then Surveyor-General of India; and I made the first example with my own hands, in about 1853, for Troughton's Great Theodolite. The same arrangement has since been applied to many other large instruments in India. Being rather a matter of detail, I shall here only say that its object is to remove, as much as possible, all restraint from the glass spirit-level, which should be allowed to adapt itself to changes of temperature with perfect freedom. The level is also encased in an external glass cylindrical cover, to protect it from sudden currents of air tending to alter rapidly the temperature of the parts. I believe that these principles were first applied by the Astronomer Royal to the Greenwich Altazimuth; but the details in my plan are somewhat different, and, as I venture to think, more complete. The appliances for adjusting the level in my plan are, I believe, new in their arrangement; my main object in devising them was to obviate strains, without introducing risk of shake, and to improve delicacy of action.

Material.—Whilst the instrument was under construction I became acquainted, at the Great Exhibition of 1862, with aluminium bronze, an alloy of 90 parts of copper with 10 parts of aluminium. Its properties seemed to be exactly those required for the material of such an instrument. With some difficulty, arising from the fact that no national establishment exists in England for such purposes, I got some experiments made on the alloy (partly by the makers, partly by the kindness of Mr. John Anderson, C.E., of the Woolwich Gun Factories *), which showed that the rigidity of the alloy might be taken at three times that of ordinary gun-metal. This being the most important property for my purpose, I determined on using it, and on reducing the thicknesses, not the depths, of all the lower parts of my design. This would still leave such parts twice as rigid as if constructed of the previous dimensions in gun-metal.

Accordingly the elevating screws of the stand and their bearings, the tribrach, horizontal circle, vertical-axis socket, horizontal-microscope arms, and foundation-plate carrying the pillars are of aluminium bronze. The remaining parts, having much less weight to bear, are of the usual materials, gun-metal and yellow brass, which are more easily worked than the bronze.

Probable performance of the Instrument.—The trials I have as yet made of the instrument lead to the conclusion that it is subject to no essential defect, and that the objects sought in its construction have been to a great extent attained. Actual work in the field, submitted to all the elaborate verifications indispensable in modern geodesy, can, however, alone ascertain its character; and this must be a work of years.

I now allude, as it may seem on first consideration rather prematurely,

* See my paper, Monthly Notices of the R. Astron. Soc., 14th Nov. 1862, vol. xxiii. No. 1, "On Aluminium Bronze as a Material for the Construction of Astronomical and other Philosophical Instruments."

to such a point for the purpose of guarding against undue expectations and of disclaiming excessive pretensions. The work that has been executed in India is of such a character, as attested by an enormous accumulation of results treated with absolute rigour, that, as respects some branches of it, I do not shrink from saying that improvement is scarcely possible. In respect to horizontal angles, the most important of all its branches, an elaborate investigation by Colonel Walker, R.E., F.R.S., the present head of the Survey, shows that the probable error of such angles, deduced impartially from a vast mass of materials, lies between $\pm 0''\cdot 28$ and $\pm 0''\cdot 20$, according as the circumstances are more or less favourable *. I believe that residual errors of that small amount contain hardly any instrumental or observational element, but that they are chiefly due to atmospheric disturbances, which no instrumental perfection can control.

As to vertical angles, the test of direct levelling compared with trigonometrical levelling, applied to a distance of 2700 miles, shows a difference between the results of these two independent methods averaging 7·3 inches per 100 miles, or $\frac{7}{100}$ of an inch per mile. There seems little room for instrumental improvement here.

In respect to astronomically determined azimuths, I hope the present instrument will give improved results, on account of the superior steadiness of level which I think it possesses.

For the determination of astronomical latitudes it has more powerful and more complete vertical appliances than any former theodolite, and it should therefore give better results.

Finally, I believe it will be found to be more permanent in its adjustments, and more convenient both to adjust and to use, than any of its predecessors. To these points, as those which makers, not being observers, are peculiarly prone to overlook or mismanage, I have given the most earnest attention, knowing how indispensable it is that a man engaged in the most difficult and refined of all observations should be spared every anxiety and inconvenience which it is in the power of mechanical contrivance to prevent. If I have in some measure only attained this apparently humble object, I shall feel that the labour of years and the anxious ponderings of many a sleepless night have not been in vain.

II. "On some Elementary Principles in Animal Mechanics.—No. V. On the most perfect form of a Plane Quadrilateral Muscle connecting two Bones." By the Rev. Samuel Haughton, F.R.S., M.D. Dubl., D.C.L. Oxon., Fellow of Trinity College, Dublin. Received April 3, 1872.

Let us suppose two bones, A B and A′ B′, lying in the same plane, joined by muscular fibres, and any two planes drawn through these bones inter-

* Account of the operations of the Great Trigonometrical Survey of India, by Col. J. T. Walker, R.E., F.R.S., &c., Superintendent of the Survey, vol. i. pp. 83, 84.

secting in a line P Q. If we suppose one bone A B to be fixed and the other bone A′ B′ to be movable, and that the contraction of the muscle compels the bone A′ B′ to revolve round P Q as an axis of rotation, it is required to find the conditions necessary in order that the work done by the contraction of the muscle shall be a maximum. I shall in this note discuss only the case in which the bones A B, A′ B′ lie in the same plane. In this case I have succeeded in demonstrating the following propositions in the case of maximum work:—

1. The axis of rotation P Q must be formed by the intersection of rectangular planes passing through A B and A′ B′.

2. The quadrilateral muscle A B A′ B′ must be inscribable in a circle.

If it be true (as I believe it is) that the muscles, bones, and joints of all vertebrate animals are so related to each other as to produce in all cases the maximum amount of work possible with a given weight of muscle, then the second proposition affords us a severe test of the truth or falsehood of my postulate. If the muscle be circumscribable by a circle, it follows, from Ptolemy's theorem, that its sides and diagonals must fulfil the following condition:—

$$ab + cd = ef,$$

where a, b are one pair of opposite sides and c, d the other pair, and e, f are the diagonals of the quadrilateral.

The *pectineus* muscle of many animals forms a plane quadrilateral when in its position of maximum extension of fibres, and therefore furnishes us with a favourable test. In the freshly dissected animal it is easy to measure with accuracy the lengths of the six lines in question, and so test the truth of the postulate.

I have thus obtained the following results:—

1. *Adult Male Lion.* Pectineus Muscle.

		in.
a.	Length of pubic origin	1·70
b.	„ femoral insertion	2·95
c.	„ anterior fibre	4·52
d.	„ posterior fibre	7·61
e.	First diagonal	5·77
f.	Second diagonal	6·76

Hence we have

$$\begin{aligned} ab &= 1{\cdot}70 \times 2{\cdot}95 = 5{\cdot}01 \\ cd &= 4{\cdot}52 \times 7{\cdot}61 = \underline{34{\cdot}39} \\ ab + cd &\ \ldots\ldots = 39{\cdot}40 \\ ef &= 5{\cdot}77 \times 6{\cdot}76 = \underline{39{\cdot}00} \\ &\text{Difference} = 0{\cdot}40 \end{aligned}$$

This difference amounts to $\frac{1}{98\cdot5}$ part of the whole.

2. *Llama.* Pectineus Muscle.

		in.
a.	Length of pubic origin	1·91
b.	„ femoral insertion	3·50
c.	„ anterior fibre	3·05
d.	„ posterior fibre	6·91
e.	First diagonal	4·09
f.	Second diagonal	6·77

Hence we find

$$ab = 1{\cdot}91 \times 3{\cdot}50 = 6{\cdot}68$$
$$cd = 3{\cdot}05 \times 6{\cdot}91 = 21{\cdot}07$$
$$ab + cd \ \ldots\ldots = 27{\cdot}75$$
$$ef = 4{\cdot}09 \times 6{\cdot}77 = 27{\cdot}69$$
$$\text{Difference} = 0{\cdot}06$$

This difference amounts to $\frac{1}{462{\cdot}5}$ part of the whole.

III. "On some Elementary Principles in Animal Mechanics.—No. VI. Theory of Skew Muscles, and investigation of the conditions necessary for Maximum Work." By the Rev. Samuel Haughton, F.R.S., M.D. Dubl., D.C.L. Oxon., Fellow of Trinity College, Dublin. Received April 3, 1872.

Let us suppose two bones, A B and A′ B′, not lying in the same plane, connected by muscular fibres; and through these bones let us draw any two planes intersecting in a line P Q; if the bone A B be fixed and the bone A′ B′ be movable and compelled to turn round the line P Q regarded as an axis of rotation, it is required to find the conditions necessary in order that the work done by the contraction of the muscle shall be a maximum.

A muscle such as is here imagined will form a skew surface, and no two of its fibres will intersect in space. I have succeeded in demonstrating the following propositions in the case of maximum work:—

1. The axis of rotation P Q must be formed by the intersection of rectangular planes passing through A B and A′ B′.

2. A certain hyperboloid of one sheet may be drawn passing through the bones A B and A′ B′; and the axis of maximum work is a generator of this hyperboloid belonging to the group different from that to which A B and A′ B′ belong.

3. The generator which is the axis of rotation of maximum work is found by the solution of a biquadratic equation.

4. In the muscles which are found in nature, the root of the biquadratic which fixes the axis of maximum work is always nearly equal to zero.

5. If there be *n* fibres in the skew muscle, we can draw a certain line O O′, joining two points on A B and A′ B′ (or A B and A′ B′ produced), such that a single fibre acting in the line O O′, with *n* times the force of a

fibre of the original muscle, will produce the same mechanical effect as the whole skew muscle.

6. When the equivalent fibre O O′ is drawn, it is always found to be so placed in relation to the axis of rotation as to produce the maximum work possible.

7. Two generators can be assigned on the locus hyperboloid, dividing the surface into two regions; in one of these regions each generator is an axis of stable equilibrium, and in the other region each generator is an axis of unstable equilibrium; the generators separating the two regions are axes of neutral equilibrium. The axis of maximum work is a *unique* axis lying in the middle of the region of unstable equilibrium.

8. Every skew muscle is a *supplemental contrivance* for the purpose of procuring a line of force joining points O and O′ already occupied by other structures, so that it would be impossible to apply the force directly.

Let I I′ be the shortest line joining A B and A′ B′, and take this line for axis of z, and the lines bisecting the angles between A B and A′ B′ for axes of x and y, the origin being placed at the point of bisection of I I′.

The equation of the locus hyperboloid is

$$(y^2-m^2x^2)+(1-m^2)(z^2-c^2)=0, \quad \ldots\ldots \quad (1)$$

where m is the tangent of half the angle between the bones, c is half the line I I′.

The equations of the bones are

$$\mathrm{AB}\left\langle\begin{matrix} y-mx=0, \\ z-c\quad=0,\end{matrix}\right. \qquad \mathrm{A'B'}\left\langle\begin{matrix} y+mx=0, \\ z+c\quad=0.\end{matrix}\right.$$

The equations of the rectangular planes passing through the bones are

$$\left.\begin{matrix} y-mx+\lambda\,(z-c)=0, \\ y+mx+\lambda'(z+c)=0,\end{matrix}\right\} \quad \ldots\ldots\ldots \quad (2)$$

where λ and λ' are parameters connected by the relation

$$\lambda\lambda'=m^2-1.$$

If x, x' be the coordinates of the extremities of any fibre whose length is p, and if we write

$$\left.\begin{matrix} \mathrm{X}=\Sigma\left(\dfrac{x}{p}\right), \quad \mathrm{X}'=\Sigma\left(\dfrac{x'}{p}\right), \\ \mathrm{K}=m^2\Sigma\left(\dfrac{xx'}{p}\right)+c^2(m^2-1)\Sigma\left(\dfrac{1}{p}\right),\end{matrix}\right\} \quad \ldots\ldots \quad (3)$$

then the biquadratic which determines the generator of maximum work is

$$\left.\begin{matrix} -\mathrm{K}(m^2+1)\,.\,\lambda^4 \\ +2c\{m\mathrm{X}(m^4+1)-m\mathrm{X}'(m^4-1)\}\,.\,\lambda^3 \\ +2c(m^2-1)\{m\mathrm{X}(m^4-1)-m\mathrm{X}'(m^4+1)\}\,.\,\lambda \\ +(m^2-1)(m^4-1)\mathrm{K}=0.\end{matrix}\right\} \quad \ldots \quad (4)$$

The sums (3) were calculated from measurements made on freshly dis-

sected animals, and the biquadratic coefficients then calculated, with the following results.

1. *Llama.* Adductor secundus (β):—

$$-0{\cdot}116\lambda^4+3{\cdot}375\lambda^3+4{\cdot}903\lambda+0{\cdot}042=0;$$

root, $\lambda=-0{\cdot}0085$.

2. "*Master Macgrath*"*. Adductor secundus (β):—

$$-0{\cdot}479\lambda^4+2{\cdot}395\lambda^3+2{\cdot}128\lambda+0{\cdot}135=0;$$

root, $\lambda=-0{\cdot}063$.

3. *Woman.* Adductor secundus (β):—

$$-0{\cdot}153\lambda^4+5{\cdot}288\lambda^3+3{\cdot}710\lambda+0{\cdot}040=0;$$

root, $\lambda=-0{\cdot}011$.

4. *African Leopard.* Adductor secundus (β):—

$$+1{\cdot}372\lambda^4+1{\cdot}343\lambda^3+1{\cdot}091\lambda-0{\cdot}077=0;$$

root, $\lambda=+0{\cdot}07$.

5. *Lion.* Adductor secundus (β):—

$$-0{\cdot}647\lambda^4+5{\cdot}456\lambda^3+4{\cdot}495\lambda+0{\cdot}405=0;$$

root, $\lambda=-0{\cdot}09$.

The position of the points O and O′ being determined, I found, by calculations too long to give in an abstract, the relation between the work actually done by the muscle and the maximum work possible, with the following satisfactory results:—

Muscle.	Work done expressed in percentage of maximum possible.
Adductor secundus (β):—	
1. Llama	99·63 per cent.
2. " Master Macgrath " ..	98·78 ,,
3. Woman..............	99·86 ,,
4. African Leopard	97·13 ,,
5. African Lion	97·92 ,,
Adductor secundus (α):—	
6. African Leopard	99·98 ,,
7. African Lion..........	98·88 ,,
Adductor primus:—	
8. African Lion..........	97·70 ,,

Skew muscles are only employed in nature as a *supplemental contrivance*; and they differ from all other muscles in this respect, that whereas the *equivalent fibre* of any other muscle always coincides with some real fibre of the actual muscle, the equivalent fibre of the skew muscle does not coincide with any actual fibre, but, on the contrary, may lie completely outside the muscle. Hence the skew muscle is used to produce an additional force along O O′, where the points O and O′ are already occupied for other purposes; and in producing this force along O O′ portions of bone A B, A′ B′ at a distance, and not occupied for other purposes, are made

* Lord Lurgan's famous greyhound.

use of, to serve as the origin and insertion of the supplemental muscle. The Table just given shows that in the employment of these supplemental muscles they are always so arranged as to work to the maximum advantage.

Thus the action of skew muscles, which, on a hasty examination, would be pronounced to be *imperfections*, furnishes another proof of my postulate.

POSTULATE.—*The muscles, bones, and joints of all animals are so related geometrically to each other as to produce in every case the maximum amount of work.*

IV. "On the Rings produced by Crystals when submitted to Circularly Polarized Light." By WILLIAM SPOTTISWOODE, M.A., Treas. R.S. Received April 24, 1872.

The general optical arrangements here used are known. Particular cases of the phenomena resulting from it have been described by Fresnel and by Airy; and more have doubtless been observed by others. The main part of the apparatus consists, so far as polarization is concerned, of the ordinary polarizer P, analyzer A, and crystal plate to be examined, C. To this are added two quarter-undulation plates of mica, Q, Q_1, one of which, Q, is placed below and the other, Q_1, above the crystal C. Let i, a, b, j be the angles between the principal sections of P, Q, C, Q_1, A, taken two and two together in the order written, all the angles being considered to be of the same sign when measured in the same direction—say, positive with that of a clock-hand. Then, if θ be the retardation produced in any ray, whose wave-length is λ, by the crystal C, the intensity of the ordinary image at any point is given (Verdet, 'Leçons d'Optique Physique,' tome ii. p. 201 *) by the formula

$$\begin{aligned} I^2 = {} & \cos^2(j-i)\cos^2(a+b)+\sin^2(j+i)\sin^2(a+b) \\ & +(\cos 2i \sin 2a \sin 2b \cos 2j - \sin 2i \sin 2j)\sin^2\frac{\theta}{2} \\ & +(\cos 2i \sin 2a \sin 2j + \sin 2i \sin 2b \cos 2j)\sin\frac{\theta}{2}\cos\frac{\theta}{2}. \end{aligned}$$

Of this general case four particular instances have been studied, viz. :—

$$a = b = 45°, \text{ whence } I^2 = \sin^2\left(j+i+\frac{\theta}{2}\right); \quad . \quad . \quad . \qquad \text{(I.)}$$

$$a = -b = 45°, \text{ whence } I^2 = \cos^2\left(j-i-\frac{\theta}{2}\right); \quad . \quad . \quad . \qquad \text{(II.)}$$

$$i = j = 45°, \text{ whence } I^2 = \cos^2\frac{\theta}{2}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(III.)}$$

$$i = -j = 45°, \text{ whence } I^2 = \sin^2\frac{\theta}{2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(IV.)}$$

* From which work the greater part of the discussion of the cases I., II., III., IV. has been taken.

In all these cases the light falling on the analyzer is plane polarized; and the following Table will give the angle through which the plane of polarization has been turned in its passage through the system Q, C, Q_1.

Case.	Angle between plane of polarization and principal plane of A.	Angle between principal planes of P and A.	Sum = angle through which plane of polarization is turned.
I.	$\frac{\pi}{2}-i-j-\frac{\theta}{2}$	$\frac{\pi}{2}+j+i$	$\pi-\frac{\theta}{2}$
II.	$j-i-\frac{\theta}{2}$	$j+i$	$2j-\frac{\theta}{2}$
III.	$\frac{\theta}{2}$	$\frac{\pi}{2}+a+b$	$\frac{\pi}{2}+a+b+\frac{\theta}{2}$
IV.	$\frac{\pi}{2}-\frac{\theta}{2}$	$a+b$	$\frac{\pi}{2}+a+b-\frac{\theta}{2}$

Cases I. and II. represent Fresnel's experiments, cases III. and IV. Airy's. In all four the rotation depends upon θ, that is, upon the wave-length; and consequently if the analyzer be turned round, we shall have a succession of colours which recur after every 180°. Thus far the effects will resemble those produced by quartz, excepting that the tints will not be exactly the same; because in these cases the rotation of the plane of polarization is approximately proportional to λ^{-1}, while in that of quartz it is proportional to λ^{-2}. It may be noticed that if I., II., IV. represent a right-handed, III. will represent a left-handed quartz.

The result in case I., being independent of i and j, shows that if the system of plates Q, C, Q_1 be turned round bodily in its own plane there will be no change in the phenomena produced.

The result in case II. shows that if the system of plates be turned bodily in its own plane, we have a sequence of colours the reverse of those produced when the analyzer is turned round, and returning at every 90° instead of every 180°.

The results in cases III. and IV., being independent of the separate values of a and b, but depending only upon the sum $a+b$, show that if the system P, Q, Q_1, A remain fixed, and the crystal C be turned in its own plane, the phenomena will undergo no change.

If the experiments be made with convergent light, two general results are manifest: first, that rings are formed as with plane-polarized light, but better defined, because in the absence of any constant term in the value of I^2 the minima values are absolutely zero; and secondly, that no dark brushes will exist.

If we now consider the rings produced with convergent light and the system Q, C, Q_1, in which the axes of Q, Q_1 are crossed, and the axis of

Q is inclined at 45° to the principal section of P, we have the conditions

$$a+b=90°,\ i=45°;$$

whence

$$\sin(a+b)=1,\quad \cos(a+b)=0,$$
$$\sin 2a=\sin 2b,\quad \cos 2a=-\cos 2b,$$
$$\sin 2(i+j)=\cos 2j,\quad \sin 2i=1,\quad \cos 2i=0.$$

Introducing these into the general expression for the intensity, we obtain

$$I^2=\tfrac{1}{2}\{1+\sin 2j\cos\theta+\sin 2b\cos 2j\sin\theta\}.$$

This shows that if the analyzer be placed parallel to the polarizer ($j=45°$) and then turned round in a positive direction until $j=90°$, $\sin 2j$ will retain its sign +, while $\cos 2j$ will take a negative sign. Also if we exchange the crystal supposed positive for a negative one, $\cos\theta$ will retain its sign while $\sin\theta$ will change it. It follows, therefore, that if in examining uniaxal crystals with this arrangement the analyzer be turned in one direction, the distortion of the rings from a circular to an oval form will take place in a vertical line for positive crystals and in an horizontal for negative; while if the analyzer be turned in the opposite sense, the distortion of the rings will likewise be reversed. This process, therefore, will give a method of distinguishing positive from negative crystals.

That the same law holds good for biaxal crystals is clear from the formulæ which apply equally to both classes; but it may be experimentally verified by applying the method to the series of mica plates crossed in different groups so as to show the transition from the biaxal to the uniaxal condition. In the case of biaxal crystals, however, the readiest method of making the experiment is to observe the rings near the centre of the field where the lemniscate loops meet, and to notice whether, on turning the analyzer in a given direction, the rings appear to stream in towards one another from the axes, or to be drawn out towards the axes. If for a given arrangement of the plates the rings of a negative uniaxal crystal are drawn out horizontally when the analyzer is turned to the right, for the same arrangement and motion the rings of a negative biaxal crystal will appear to flow together.

The Society adjourned over Ascension Day to Thursday, May 16.

May 16, 1872.

FRANCIS GALTON, M.A., Vice-President, in the Chair.

The following communications were read:—

I. "On the Specific Heat and other physical characters of Mixtures of Methylic Alcohol and Water, and on certain relations existing between the Specific Heat of a Mixture or Solution and the Heat evolved or absorbed in its formation." By A. DUPRÉ, Ph.D., Lecturer on Chemistry at the Westminster Hospital. Communicated by W. ODLING, M.B. Received April 4, 1872.

(Abstract.)

The pure methylic alcohol used in these experiments was prepared from rectified wood-spirit according to a process of E. Th. Chapman, being a modified chloride-of-calcium process as originally recommended by Sir Robert Kane. The anhydrous alcohol obtained had a specific gravity of ·81371 at 10° C.; it boiled at a temperature of 58°·6 C. under a pressure of 757·4 millims., had a specific heat of 58·325 between the temperatures 60° and 18°, and was perfectly miscible with water in every proportion. When oxidized with excess of bichromate and sulphuric acid, it yielded nothing but carbonic acid and water.

Section 1. *Specific Heat.*

This is estimated in the usual manner, by heating a known weight of the liquid enclosed in a suitable vessel to a certain temperature, and plunging it, vessel and all, into the water of a calorimeter; the rise in temperature produced will, with the necessary corrections, furnish the data required for the calculation of the specific heat sought. This calculation was performed by help of the following formula:—

$$C = \frac{W(t''-t)}{m(T-t')} - \frac{\mu}{m},$$

wherein C is the specific heat required;

W the water-value of the calorimeter and contents;

t the temperature of the calorimeter at the beginning;

t' the temperature of the calorimeter at the end;

t'' the temperature of the calorimeter at the end plus the necessary corrections;

T the temperature of the steam-oven;

m weight of fluid or mixture employed;

μ water-value of vessel holding the mixture.

The vessel to contain the mixture is an annular brass vessel, with a fan-

wheel in the inner cylindrical space to serve as stirrer. The steam-oven is heated by the vapour of methylic alcohol.

The mean results are given in the following Table:—

TABLE I.

Percentage of methylic alcohol, by weight.	Specific heat found.	Specific heat calculated.	Difference.
10	98·582	95·832	+2·750
20	95·914	91·665	4·249
30	92·658	87·497	5·161
40	89·219	83·330	5·889
50	84·645	79·162	5·483
60	80·177	74·995	5·182
70	75·500	70·827	4·673
80	69·999	66·660	3·339
90	64·282	62·492	1·790
100	58·325		

Section 2. *Heat produced by mixing Methylic Alcohol and Water.*

This is estimated by mixing the alcohol and water, in the required proportion, in an annular vessel similar to the one used for the estimation of the specific heat, and observing the elevation of temperature produced in the water of a calorimeter in which the annular vessel is immersed. The units of heat evolved in the formation of 5 grms. of each mixture were thus calculated and found to be:—

TABLE II.

10 per cent. spirit	20·930	60 per cent. spirit	41·493
20 „ „	37·276	70 „ „	34·456
30 „ „	44·744	80 „ „	22·448
40 „ „	45·384	90 „ „	13·164
50 „ „	44·429		

The temperature at which the mixing took place ranged between 16° and 20° C.

Section 3. *Boiling-points.*

The mixtures were boiled in a flask connected with a condenser, by which the vapours are condensed and made to run back into the flask; the composition of the mixture is thus kept as uniform as possible.

The cold liquid running back is prevented from disturbing the boiling by having to pass down a tube placed inside the flask, in which it becomes heated nearly to boiling before it flows into the boiling mixture. The following Table gives the results, the barometer standing at 757·4 millims. The numbers in column 3 are calculated on the assumption that the

boiling-point of a mixture is influenced by its components in proportion to their respective weights.

TABLE III.

Percentage of absolute methylic alcohol, by weight.	Boiling-point observed.	Boiling-point calculated.	Difference.
0	99°·93	°	
10	82·57	95·80	−13·23
20	75·20	91·76	16·56
30	70·68	87·53	16·85
40	68·31	83·40	15·09
50	67·08	79·26	11·18
60	65·75	75·13	9·38
70	64·65	71·00	6·35
80	63·13	66·87	3·74
90	60·96	62·73	1·77
100	58·60		

Section 4. *Capillary Attraction.*

This is estimated by means of an apparatus similar to the one employed by Gay-Lussac, the height being measured by a cathetometer. Two capillary tubes are employed, and the mean taken. The third column in Table IV. gives the length of a column of water equal in weight to the thread of alcoholic mixture in the second column, and affords, therefore, a measure of the relative strength of the molecular attraction in the various mixtures. The heights in column 4 are calculated on the assumption that they will be proportional to the weight of the constituents.

The experiments are made at a temperature of 13°·5 C.

TABLE IV.

Percentage of methylic alcohol, by weight.	Height, assuming water =100 millims.	Relative molecular attraction.	Height calculated.	Difference.
0	100·000	100·000		
10	68·820	67·818	93·334	−25·416
20	58·830	57·264	86·667	29·403
30	52·463	50·381	80·001	29·620
40	48·947	46·252	73·335	27·083
50	46·538	43·136	66·668	23·532
60	46·467	42·170	60·002	17·832
70	45·176	40·034	53·336	13·302
80	43·997	37·955	46·669	8·714
90	42·612	35·671	40·003	4·332
100	40·970	33·337		

Section 5. *Specific Gravity and Rate of Expansion.*

Table V. gives the specific gravities of the various mixtures at the temperatures of 10° and 20° C., water at 4° C. taken as unit, together with the calculated specific gravities at 10° C., and the difference between the specific gravities as observed and calculated.

Table VI. gives the expansion of 100 volumes of the mixtures when heated from 10° to 20° C., calculated from the data of the previous Table. The figures in column 4 are calculated on the assumption that the expansion is proportional to the volumes of the constituents, the contraction taking place on mixing being allowed for.

TABLE V.

Percentage of methylic alcohol, by weight.	Specific gravity at 10° C.	Specific gravity at 20° C.	Specific gravity at 10° C., calculated.	Difference.
0	99973	99819		
10	98632	98384	97762	+ 870
20	97478	97080	95622	1856
30	96222	95675	93573	2649
40	94729	94054	91611	3118
50	92991	92205	89727	3264
60	91048	90207	87923	3125
70	88933	88035	86188	2745
80	86598	85655	84520	2078
90	84054	83079	82916	1138
100	81371	80334		

TABLE VI.

Percentage of methylic alcohol, by weight.	Volume at 10° C.	Volume at 20° C., found.	Volume at 20° C., calculated.	Difference.
0	100	100·154		
10	100	100·252	100·293	−0·041
20	100	100·410	100·429	−0·019
30	100	100·571	100·562	+0·009
40	100	100·718	100·689	+0·029
50	100	100·853	100·809	+0·044
60	100	100·932	100·922	+0·010
70	100	101·019	101·028	−0·009
80	100	101·101	101·124	−0·023
90	100	101·173	101·212	−0·039
100	100	101·290		

Section 6. *Compressibility.*

This is estimated by means of an apparatus similar to the one employed by Regnault and Grassi. Instead of a reservoir of compressed air a

vessel holding liquid carbonic acid was employed, by means of which any required pressure could be obtained with great facility.

TABLE VII.

Percentage of methylic alcohol, by weight.	Temperature in degrees Centigrade.	Compressibility for 1 atmosphere.		Difference.
		Found.	Calculated.	
0	16·8	0·00004741		
10	16·5	·00004368	0·00005497	−0·00001129
20	16·0	·00004365	·00006303	·00001938
30	15·4	·00004289	·00007052	·00002763
40	17·4	·00004781	·00007758	·00002977
50	16·5	·00004916	·00008420	·00003504
60	16·4	·00005541	·00009029	·00003488
70	15·7	·00006167	·00009586	·00003419
80	16·3	·00007416	·00010083	·00002667
90	15·2	·00009103	·00010511	·00001408
100	15·0	·00010879		

All relations pointed out in the former paper as existing between the various properties of mixtures of ethylic alcohol and water find their parallel in the mixtures now under consideration. Certain sets of properties come to a maximum deviation from the calculated mean at the same strength; in some cases the values found are always below, in others always above the calculated mean; and in both mixtures the rate of expansion shows the same singular peculiarity, viz. of being for certain mixtures below, for others above the mean. In this, as in the previous mixture, the specific heat and the heat evolved during mixture not only come to a maximum deviation from the mean in mixtures of the same strength, but all mixtures evolving the same amount of heat during their formation possess a specific heat elevated to the same amount above the mean; and, moreover, the numerical relation between these two values is the same for mixtures of every degree of strength. Hence, if the heat evolved in the formation of 5 grms. of any of the mixtures be divided by 7·9, the elevation of the specific heat of this particular mixture above its calculated mean value is obtained. Between the boiling-point and the capillary attraction a somewhat similar relation is found. If in this case the observed depression of the capillarity of any mixture below its calculated mean value be divided by 1·9 (the capillarity of pure water taken as 100), the depression of the boiling-point of this mixture below the mean is obtained.

It has been pointed out above that an intimate relation exists between the heat evolved during the formation of a mixture and its specific heat. A closer study of this relation has led the author to the following conclusions, which seem not only to hold good for the two mixtures now under consideration, but to be of very general application; they may be formulated as follows.

Relation existing between the specific heat of mixtures and the heat evolved during their formation :—

1. The difference between the number of heat-units evolved during the mixing of given weights of two substances, at the temperatures t and t' respectively, is equal to the difference between the number of heat-units required to raise the mixture, and that required to raise the two constituents taken separately, from the lower to the higher temperature. Or let U and U′ be the units of heat evolved by mixing x and y at the temperatures t and t' respectively, S, S′, and S″ the specific heat of the mixture z and its constituents x and y respectively, then

$$U \sim U' = z \,.\, S\,(t-t') \sim \{x \,.\, S'\,(t-t') + y\,S''\,(t-t')\}.$$

2. If more units of heat are evolved at the higher than at the lower temperature, the specific heat of the resulting mixture will be below the calculated mean ; on the other hand, the specific heat of the mixture will be above the calculated mean, if the greater number of heat-units be evolved at the lower temperature*.

3. The absorption of a lesser number of heat-units will be of course equivalent to the evolution of a greater number, while the absorption of a greater number will be equivalent to the evolution of a smaller number of heat-units.

A series of Tables are next given showing that the following mixtures, namely, ethylic alcohol and water, methylic alcohol and water, prussic acid and water, ethylic alcohol and bisulphide of carbon, aqueous solutions of potassium chloride, sodium chloride, potassium nitrate, and potassium hydrate, conform to proposition 2, and all, except the mixture of ethylic alcohol and bisulphide of carbon, also conform as closely as can be expected with proposition 1. Perfect agreement is only possible if the specific heat of each constituent, as well as that of the mixture, has been accurately determined between the two temperatures for which the heat evolved on mixing has been estimated ; and this has not, in the majority of cases, been done as yet.

The author next points out that, at first sight, the above observations may be explained by assuming that a difference in the chemical constitution of the mixture is produced by a change of temperature†. In some cases a rise in temperature may produce a certain amount of decomposition or dissociation, while a fall would be accompanied by recombination ; the apparent specific heat of the mixture would thereby become raised. In some other cases the reverse may happen, and the specific heat of the mixture would fall below the mean. This supposition seems, however, incompatible with the observed rate of expansion of mixtures, both of ethylic and methylic alcohol and water. In these the rate of expansion is in some of the mixtures below the mean, indicating apparently that a more intimate union takes place on heating, in others the rate of expansion

* This relation had already been suggested by Berthelot.

† This has already been pointed out by Pfaundler, as well as by Marignac.

is above the mean, indicating dissociation. The specific heat of these mixtures does not, however, show any corresponding alterations.

The author finally shows that if these propositions should prove to be an exposition of a general law, they will enable us to calculate the specific heat of one constituent of a mixture or solution, if we know the specific heat of the other constituent, the specific heat of the mixture, and the heat evolved or absorbed at two different temperatures between which these specific heats have been estimated. It may thus be possible to compare the specific heat of liquid and solid substances when both are placed under similar conditions, and relations may be discovered which at present are masked in consequence of the difference in the physical conditions of the substances to be compared.

II. "On Supersaturated Saline Solutions. Part III.—On a relation between the Surface-tension of Liquids and the Supersaturation of Saline Solutions." By CHARLES TOMLINSON, F.R.S., and G. VAN DER MENSBRUGGHE. Received April 6, 1872.

It was stated by one of us in Part II.* that when a drop of a liquid is deposited on the surface of a supersaturated saline solution, it will do one of three things:—(1) mingle with the solution without any nuclear action; (2) spread out into a film with powerful nuclear action; or (3) assume the form of a lens, without any separation of salt. It was further stated that when a liquid forms a film or a lens, it does so according to the general proposition, that if a drop of a liquid B, with the surface-tension b, be placed on the surface of another liquid A, with the surface-tension a, the drop will spread into a film, if $a>b+c$ (c being the tension of the common surface of the liquids A and B); but if, on the contrary, $a=<b+c$, the drop will remain in the form of a lens. Hence if B spread on A, A will not spread on the surface of B. When the liquids A and B mingle in all proportions, c has no value. The spreading of the drop may also be interfered with by the superficial viscosity of the solution, or the greater or less difficulty in displacing the superficial molecules.

It was also stated that if a greasy smear be made upon the clean interior surface of a flask above the solution, and the flask be inclined so as to bring a portion of the solution against such smear, the liquid does one of two things:—(1) it breaks up into well-defined globules, which roll over the smear without loss of tension, in which case the smear has no nuclear action; or (2) as soon as the solution reaches the smear its edge flattens and becomes ragged, in which case the smear is nuclear and the salt separates.

A glass rod drawn through the hand becomes covered with a smear or film; or the same rod, by exposure to the air, contracts a film by the

* Philosophical Transactions for 1871, p. 52.

condensation of floating vapour, or a deposit of film-forming dust, and so is brought into the nuclear condition.

It was further stated that when a lens of oil is resting on the surface of a solution, the flask may be rapidly rotated or briskly shaken, so as to break up the oil-lens into a multitude of minute globules, giving the solution the appearance of an emulsion; but that by repose the solution regains much of its transparency, without any separation of salt; but that if, while the flask is being turned round, a sudden jerk be given to it, so as to flatten some of the globules against the side, the solution instantly becomes solid.

The powerful action of films in putting an end to the state of supersaturation being thus established, it occurred to one of us, who had already succeeded in explaining a number of obscure phenomena on the principle of surface-tension *, that that force, properly handled, would suffice to account for most, if not all, the varied phenomena of supersaturation. According to this view, whatever tends greatly to lower the surface-tension of a supersaturated saline solution, causes a separation of salt, and at once puts an end to the condition of supersaturation.

In order to test this view, a large number of experiments have been performed by one of us during the last six months, consisting of repetitions of former experiments or of new ones suggested by one or both of us. All these experiments have been performed in the open air at Highgate, near London, the object being to avoid all possible miscarriage from the effects of floating dust in the air of a room. It had been suggested that some of the former results as to the action of films might have been vitiated from this source; and although this does not appear to have been the case, yet it is with much satisfaction that the experimenter refers to the greater facility and certainty with which experiments of this kind are conducted in the open air, as compared with those made in a room. In the open air a gentle wind would sometimes blow over the mouths of the flasks, sufficient to produce a low musical note, without any nuclear action, unless a speck of soot or a small insect were carried into the solution; but in general, in order to prevent evaporation, the flasks were kept covered with watch-glasses or small beakers, except when performing an experiment.

The salt used in the following experiments was sulphate of soda, in large crystals, not effloresced, one of three strengths being adopted as circumstances required, and which will be indicated when necessary, namely, 1 part of salt to 1 of water, 2 parts of salt to 1 of water, and 3 parts of salt to 1 of water. Every solution was first made in a large flask, and filtered boiling into eight or ten small flasks, which were reboiled, covered with watch-glasses or beakers, and carried on a tray into the open air. The same experiment was repeated on a number of these solutions of the same strength.

* "Sur la Tension superficielle des Liquides," par G. Van der Mensbrugghe, Répétiteur à l'Université de Gand. Mémoires couronnés par l'Acad. Royale de Belgique, tome xxxiv. 1869. See also Phil. Mag. for Dec. 1869 and Jan. 1870.

The points to which this experimental inquiry tended are included in the four following propositions:—

I. That a supersaturated saline solution contained in a catharized flask will remain liquid so long as its free surface, or the surface in contact with the sides of the flask, does not undergo in one or many points a notable diminution of surface-tension.

II. That if we deposit on the surface of a supersaturated saline solution a drop of a liquid of feeble tension, it spreads, and crystallization takes place immediately or after a short time.

III. That while a liquid of feeble tension produces crystallization after a time more or less short, a liquid of considerable contractile force (such as pure water) not acting chemically on the solution, may be brought into contact with it without producing change of state.

IV. That as a liquid of feeble tension produces crystallization, so a solid covered more or less with a film of such liquid produces change of state, either at once or after a short time.

But before any conclusions could be drawn from the results of experiments as to the relation between the surface-tension of liquids and the state of supersaturation in saline solutions, it was necessary to measure the surface-tension of the solutions of Glauber's salt operated on. Accordingly the following data were determined, *first*, for a solution containing 1 part of salt to 1 of water, and, *secondly*, for a solution containing 2 parts of salt to 1 of water. The diameter of the capillary tube was 1·598 millim.*

Specific gravity of the solution 1 salt to 1 water at 17° C.=1·198.
The capillary height 11 millims.
The specific gravity of the other solution=1·289.
The capillary height 8·7 millims.

These data give, according to the formula $t=\frac{r\,.\,h\,.\,d}{2}$ (in which t is the tension, h the height, d the density, and r the radius of the tube), for the superficial tensions of the solutions in question, not a greater value than from 4 to 5·2.

If the states of supersaturation of saline solutions depend on the maintenance of surface-tension, according to the first proposition, any force or substance that produces a notable diminution of such tension will cause the state of supersaturation to cease.

Such a force is heat, while such substances as camphor, benzoic acid, &c. have a marked effect in lowering the superficial tension of water, and in doing so undergo those remarkable gyrations which are so well known.

And first with respect to heat, applied, not so as to affect the whole solution, but locally, so as to raise the temperature at one part or point of the surface, while the other parts remained at the temperature of the atmosphere.

* The tube was calibrated by Dr. E. J. Mills, F.C.S. &c.

Experiment 1. Four flasks, each about half full of a supersaturated solution of Glauber's salt (2 salt to 1 water), were exposed to a temperature of 32° F. for an hour. A red-hot poker was then passed down the neck of each flask, and in two of them the hot metal was brought into contact with the surface of the solution so as to raise a volume of vapour. There was no separation of salt in any one case.

Experiment 2. A solution containing a considerable mass of the seven-atom salt at the bottom of the flask was moved over the flame of a spirit-lamp in a line from the bottom of the flask to the neck, so as to heat one part only of the flask. The only effect was to convert a portion of the surface of the seven-atom salt into the anhydrous; but there was no crystallization. After some hours the anhydrous portion had again taken up its water of crystallization.

Experiment 3. A solution of 2 salt to 1 water that had been in the open air during twenty-four hours was uncovered, and water nearly boiling was dropped upon it. A slight cloudiness came over the solution, but there was no crystallization.

Next day a very weak solution of Glauber's salt nearly boiling was dropped upon the surface with no nuclear action.

Experiment 4. An eight-ounce globular flask had the globe filled with a solution of 2 salt to 1 water. Solutions of two different strengths, namely 1 salt to 1 water, and 3 salt to 1 water, at a nearly boiling temperature, were dropped upon it, but with no nuclear action.

Experiment 5. A solution of 1 salt to 1 water had filtered into it a nearly boiling solution of 3 salt to 1 water. The drops descended to the bottom of the flask in beautiful rolling rings, but there was no nuclear action.

Experiment 6. The neck of a flask was inclined over the flame of a spirit-lamp, so as to boil the upper part of the solution, while the lower part remained cold. Water was driven off in vapour, so as to leave a crust of salt in the neck. This, when the flask was left to itself, gradually absorbed moisture and trickled down, and was also washed down into the solution; but there was no nuclear action either from this or from the heat.

These experiments on the action of heat lead to the conclusion that, however much it may diminish the superficial tension of the solutions, it does not apparently disturb the state of supersaturation. This result may be explained with reference to the feeble tension of the solution (=4), and to the fact that heat locally applied does not greatly diminish it. Moreover, heat tends to oppose crystallization by increasing the solubility.

Numerous experiments were tried as to the action of newly sublimed camphor and benzoic acid on the solutions. The flasks containing these bodies floating on the solutions were plugged with cotton-wool and kept for some months, during which time they were repeatedly shaken; but there was no separation of salt. The camphor and benzoic acid formed weak solutions with the supersaturated solutions; but the tension of camphorated

water being =4·5, and that of an aqueous solution of benzoic acid falling within the limits 4 and 5·2, the difference in tension is too small to produce a rupture of equilibrium. The same remark applies to a solution of soap and of bicarbonate of soda, which had no nuclear action.

Action of Vapours.—It has been shown by recent researches that the presence of vapours in the air of a room, even in minute quantity, has a marked influence in lowering the tension of water and other liquids, so as to account for the discordant values of various careful measurements of the capillary heights of such liquids. As to the nuclear action of the vapours of certain volatile liquids upon supersaturated saline solutions, many observations had been made by one of us, leading to the conclusion that such vapours are strongly nuclear when they become condensed into the form of films on the surface of the solutions, as when the latter is of a lower temperature than the former. In order to ascertain whether vapours, as such, that is, without forming films, have any nuclear action, the following experiments were contrived. The vapour was presented to the surface of the solution by means of a bit of sponge tied to the end of a glass rod, wetted with the volatile liquid and carefully passed down the neck of each flask, so as to avoid touching the side, and bringing the sponge close upon the surface to avoid touching that also*. The sponge was held over the solution several minutes, then carefully withdrawn and the flasks covered, leaving the interior charged with vapour. The liquids used were ether, absolute alcohol, chloroform, bisulphide of carbon, wood-spirit, and benzole. The solutions were of all three strengths, and the temperature from 40° to 47° F. After many hours and even days the flasks had a strong odour of the vapours in question, but there was no separation of salt.

Vapour of camphor was also tried in the following manner:—

Experiment 7. A quantity of camphor was placed in a small retort, the beak of which, made chemically clean by being heated in the flame of a spirit-lamp, was passed into a flask containing a solution of 2 parts salt to 1 of water. The camphor in the belly of the retort was then boiled so as to produce a powerful jet of vapour upon the surface of the solution. The camphor condensed upon such surface in the form of a fine white powder without any nuclear action.

In this case a portion of the vapour of camphor or of the powder would dissolve in the solution without producing in it a notable diminution of surface-tension. The same remark applies to the other vapours, to the action of solid camphor and benzoic acid, of heat, &c.

So also, as stated in Part II., glycerine mingles with the solution without any nuclear action. Now the surface-tension of glycerine =4·2; so that it can have no effect in lowering the surface-tension of a solution =4, and does

* In a few cases the wet sponge did touch the solution for an instant, so as to take up a small portion, which immediately crystallized upon the sponge; but the crystallization thus produced, not being in contact with the solution, the latter retained its liquid state.

not sufficiently lower the tension of a solution $=5{\cdot}2$ to produce a rupture of equilibrium.

It was also stated that bisulphide of carbon $t=3{\cdot}3$ to $3{\cdot}5$, and chloroform $=2{\cdot}98$ to $3{\cdot}12$, formed lenses on the surface of the solution, and that on gently agitating the flask they fell to the bottom, where they remained permanently without any nuclear action. Creosote $=3$ behaves in the same manner. Now, in any one of these cases, the tension $t+c$ must be greater than $4{\cdot}5$, and hence there can be no separation of the salt.

We now pass on to consider the second proposition, namely, that if on the surface of a supersaturated saline solution there be deposited a drop of a liquid of feeble tension, the drop spreads and crystallization is determined. Now it is shown in Part II. that drops of ether, of alcohol, and of similar volatile liquids, as well as of certain oils, both volatile and fixed, spread over the surface of the solutions and act as powerful nuclei. On the surface-tension theory, a liquid such as ether, of which the tension $=1{\cdot}88$, or alcohol $=2{\cdot}5$, or wood-naphtha $=2{\cdot}11$, or oil of lavender $=2{\cdot}9$, must spread on the surface of a supersaturated solution of Glauber's salt of which the surface-tension is as high as from 4 to $5{\cdot}2$. This is true in a large number of cases that have been observed, and so far the phenomena are consistent with the theory; but there are cases in which liquids of low tension, such as oil of turpentine $=2{\cdot}2$ to $2{\cdot}4$, and some varieties of castor-oil $=2{\cdot}5$, do not form films, but well-shaped lenses, and remain as such during many hours and even days. Quincke seems to have met with cases of this sort in his elaborate inquiry on the capillary phenomena of the common surface of two liquids*; and he endeavours to account for these exceptions to the general law by the statement that if a lens-shaped drop of a liquid 2 (of low tension) remain on the free surface of a liquid 1 (of much higher tension) without spreading itself out, then it is certain that in most, and probable that in all cases the free surface of liquid 1 is rendered impure by a thin layer of a foreign liquid 3. Now in experiments on supersaturated saline solutions, the flasks, the filtering-apparatus, and the solutions must be, as already explained by one of us, chemically clean; so that in boiling and filtering a solution into clean flasks in which it is boiled up again, covered over, and left to cool in the open air of the country, it is difficult to imagine the existence of such a film as M. Quincke refers to. Moreover, did such a film exist, the solution in cooling would probably become solid under its action. Indeed this sometimes happens in the case of flasks that have been already used in experiments on the nuclear action of oils; for, however carefully they are cleansed, it may happen that one or two out of a dozen may not be quite clean, so that, in the cooling of a boiling solution, a film detached from the walls of the flask may spread over the surface with nuclear action. In order, if possible, to prevent the formation of such a film, the following experiment was made:—

Experiment 8. A solution of 1 part of Glauber's salt to 1 of water,

* Poggendorff's Annalen, vol. cxxxix. See also Phil. Mag. for April 1871.

with the addition of a bit of caustic potash, was boiled and filtered into four clean flasks. When cold, a drop of castor-oil was deposited upon the surface of each of the solutions. It flattened at first, but soon recovered the lenticular form. There was no nuclear action during an hour. On gently shaking the flasks, the oil was diffused through the solution without nuclear action.

In an experiment described in Part II. fragments of stearine were scraped into a solution with immediate nuclear action. In such a case, the stearine furnished the film-forming material that produced the solidification of the solution. The solution was boiled with the stearine in it; and in cooling the stearine formed into solid disks without nuclear action, although the flask was frequently shaken. In this case the boiling solution had saponified or otherwise removed the film-forming matter, or, in other words, had made the stearine chemically clean.

There is also a difficulty in the case of oil of turpentine, as in the following experiment:—

Experiment 9. A drop of an old but clear and bright oil of turpentine was deposited on the surface of a solution containing 2 parts of salt to 1 of water. The drop flashed out into a film, and the solution immediately became solid. The turpentine was now distilled, and a drop of the distillate was deposited on a similar solution, when it formed a well-shaped lens with no nuclear action, although the flasks were left out during several days.

Now the tension of the old oil first used is $=2 \cdot 2$, and had the effect of distillation been greatly to exalt the tension, the experiment would have been intelligible according to the theory; but on measuring it the tension was found to be only $2 \cdot 4$.

A somewhat similar case is given in Part II., in which an old oil of bitter almonds was strongly nuclear, while the same oil freshly distilled had no such action, but became converted into benzoic acid, still without any separation of salt. After some days, to prove that the solution was still supersaturated, it was touched with an unclean wire and it immediately became solid.

Still, however, there are such a large number of cases in which oils and other liquids spread upon the surface of the solutions with nuclear action as to justify the labour bestowed upon the theory by one of us during the last six months. Many of these cases are stated in Part II.; but a few of them may be repeated here for the sake of comparing the action of such liquids upon solutions of different strengths, which was not done before.

If we take a number of oils, the tension of which varies from about $2 \cdot 5$ to $3 \cdot 5$, a drop of any one of them, according to the theory, ought to spread on the surface of a solution where $t=5 \cdot 2$, and not in all cases spread on the solution of which $t=4$.

Experiment 10. Twelve flasks, containing a solution of 1 part salt to 1 of water were prepared, and a drop of each of the following oils formed

films with immediate crystallization of the solutions, viz. pale seal-oil, sperm-oil, cotton-seed oil, and niger-oil. A drop of linseed-oil formed a lens; but this soon becoming ragged, crystals diverged from it. A drop of castor-oil formed a lens with no nuclear action.

Experiment 11. Three of the above solidified solutions were heated over a lamp, boiled and covered over. The oil collected on the surface in innumerable small disks. Next morning one of the solutions was found crystallized, and the other two became solid on gently agitating the flasks.

In this case as the solutions cooled down or were gently agitated the disks spread out into films with nuclear action.

Experiment 12. A solution of 3 parts salt to 1 of water was filtered into twelve flasks, when a drop of each of the following oils deposited on the surfaces of the solutions became lenticular without any separation of salt, viz. pale seal-oil, olive-oil, rape, castor-oil, croton-oil, niger, sperm, and cotton-seed oil.

So far this result is in accordance with the theory.

Experiment 13. A solution of the same strength as in the last experiment was employed, when a drop of seal-oil, sperm, cotton-seed, and niger spread out into films with powerful nuclear action. Linseed- and castor-oil formed lenses with no such action.

Now it must be remarked that on the day when Exp. 12 was made the weather was dull, damp, and cloudy, and during the time of Exp. 13 the weather was bright and clear. Some years ago it was a matter of frequent observation to one of us, that the formation of cohesion-figures on the surface of water was much more rapid and decisive, with altogether finer and sharper results, in bright weather as compared with dull, damp, wet, or foggy weather. The same remark applies to the motions of camphor on water, and to those curious phenomena known as "camphor-currents" and "camphor-pulsations"*. In the production of all these phenomena, as has been shown by one of us†, surface-tension plays a most important part; and such tension is lowered in dull foggy weather probably by the condensation of the vapour of volatile matters contained in the atmosphere. A drop of a liquid under such conditions may not spread on the surface of water or of mercury, the latter being especially liable to such influences; whereas on a bright day such surfaces are particularly active, and experiments succeed which some hours or days before failed to produce the results expected.

Then, again, as pointed out by one of us in Part II., the viscosity of the surface, or of the drop of liquid placed upon it, may greatly interfere with the operation of the law by which a liquid B spreads upon the surface A. A supersaturated saline solution has a considerable viscosity of surface, which it retains for many hours after it has cooled down. In the course of about twenty-four hours the more watery particles come up to the sur-

* Phil. Mag. for Dec. 1869.

† Sur la Tension superficielle des Liquides, par G. Van der Mensbrugghe.

face and the tension improves; so that the same surface which may have sufficient tensile force to cause a drop of oil to spread upon it, might some hours earlier have retained it in the lenticular form*.

There are also certain modifications to which oils &c. are subject in consequence of the presence of ozone and other matters in the air, which may somewhat disturb the results expected to be obtained from the action of surface-tension.

It was stated in Part II. that when an oil &c. assumes the lenticular form, the solution may be agitated so as to break up the lens into a multitude of globules, and give the solution the appearance of an emulsion. In such a case the tensions of the two liquids are of nearly the same value; if not, the agitation often produces crystallization; but even in the former case it was stated that a sudden jerk will sometimes produce immediate solidification of the solution. Now taking the tension of the solution at 5·2, and that of oil of olives at 3·7, and the tension at the surface of separation of the solution and the oil-lens at about 2, then the sum $3{\cdot}7+2$ is equal to the tension of the solution, and the spreading on the surface ought to be impossible, unless fine clear weather, absolutely clean vessels and solutions, and the absence of surface-viscosity concur to increase the surface-tension of the solution. At the surface of separation of the solution and of the glass, spreading may be possible in the case of certain oils without these concurring circumstances. Suppose a drop or a minute globule of oil to be brought into direct contact with the wet solid side of the solution, as by the jerk above referred to, the film of solution is displaced and the oil can wet the solid side. It may happen that the tension t of the solution at the wall of the flask is greater than the sum of the tension t of the surface of separation of the solution and of the oil, plus the tension of the oil in contact with the solid side; that being the case, the instant solidification consequent on the jerk is accounted for.

It will be seen, then, that when the drop of oil &c. remains as a lens on the surface, there is a diminution of tension at the surface of the solution in contact with the oil; but in such a case the tension is not sufficiently lowered at one point as to render molecular equilibrium impossible at this point, and so break up the whole system of supersaturation. But if the solution be agitated, so as to bring into contact with the surface of the glass a portion of the drop, there will still be diminution of tension at the surface of the solution in contact with the solid, and now the diminution is sufficient to produce crystallization. Thus it appears that oils may act

* Some of the distinguished physicists who are now engaged in studying the phenomena of surface-tension refer to the embarrassing effects of surface-viscosity. Thus Herr Lüdtge remarks that a solution of soap ($t=2{\cdot}8$ to 3) does not spread upon a solution of Panama-wood ($t=5{\cdot}7$); and it has been shown by one of us that the viscosity of the surface explains why a solution of soap does not spread on a solution of saponine or of albumen; and, on the other hand, the liquid drop being viscous, there is no extension, or only a feeble one, since the slight difference in tension is equilibrated by the resistance of the viscous liquid.

differently according as they alter the tension of the liquid freely exposed to the air, or the tension of the liquid in contact with the glass, which is not of the same value.

With respect to Proposition III. there is no difficulty. A liquid of considerable contractile force, such as pure water, produces no separation of salt in a solution of less contractile force. This explains a number of cases described in a note by one of us submitted to the Society in July last*, in which solutions exposed for hours together to heavy rain did not crystallize, unless the rain brought down a speck of soot or some unclean body that lowered the surface-tension of the solution. Indeed we know of no liquid of superior tensile force to that of the solution, and not acting chemically upon it, that has any influence in producing crystallization.

Proposition IV. also agrees with the phenomena. A glass rod or other solid, more or less smeared with a film of a liquid of low tension, when brought into contact with the solution determines crystallization by lowering the surface-tension. Such, then, is the function of a nucleus with respect to supersaturated saline solutions. If the solid be made chemically clean, it may be plunged into the solution without altering its tension, and hence there is no separation of salt. And here it may be remarked that such a case is possible that a crystal of the salt itself may be brought into contact with the solution without disturbing its tension, and hence be inactive. It has never been pretended that a crystal of the salt is not a good nucleus for a supersaturated solution of its own kind; all that has been stated by one of us is that, under special conditions, such a crystal may be lowered into the solution without acting as a nucleus.

III. "Remarks on the Sense of Sight in Birds, accompanied by a description of the Eye, and particularly of the Ciliary Muscle, in three species of the Order *Rapaces*." By Robert James Lee, M.A., M.D. Communicated by Robert Lee, M.D., F.R.S. Received April 11, 1872.

It is proposed in this communication to describe certain peculiarities in the eye of the bird as compared with the eyes of other vertebrata; and further to examine to what extent those peculiarities enable us to explain the remarkable powers of sight with which all species of birds are more or less highly endowed.

Those who study the habits and modes of existence of the lower animals, find great interest in applying to various phenomena connected with them the results of anatomical investigation, and in endeavouring to discover such causes, or means adequate to produce such effects, as to render the supposition of the existence of an indefinite property like instinct very frequently unnecessary.

This method it is my desire to apply in the explanation of those high

* *Suprà*, p. 41.

and distant flights which are performed by certain species of birds in search of food or in their migrations to different localities.

For us it is difficult to form a clear conception of the power of sight possessed by birds if we only use our own faculties in this respect as the standard of comparison; by which I mean to imply that the mind must be prepared for the consideration of the phenomena referred to by observing in detail numerous important differences in the structure of the eye, which combine to facilitate a conception of ideas otherwise beyond the reasonable limits to which even imagination might extend.

This field of inquiry will long engage the attention of the naturalist and anatomist; indeed it may be said to be inexhaustible; and I feel considerable hesitation in offering a contribution insignificantly small to the elucidation of a subject of such magnitude.

We may acquire some idea of the sight of the bird by comparing the dimensions of the eye with those of the brain or the optic lobes; and by arranging the measurements thus obtained, and referring them to some fixed standard, we may estimate the relative and individual powers of vision enjoyed by different species. In illustration of this we have an instance, in the case of one of the birds which I propose to describe minutely in this communication, in which the eye is actually considerably larger than in the human species; and we have a still more striking example, considering the size of the bird, in the *Goura coronata*.

Again, if we regard the eye as an optical instrument, we may estimate its efficiency by examining the internal structures on which the formation and perception of the image depend,—such as the size and coefficient of refraction of the lens, the extent and character of the retina, and particularly those differences of minute structures which have relation to susceptibility to light, by which the night-flying birds are distinguished from the day-flyers. Nor does the inquiry into the effects of domestication upon the sight appear less interesting.

It is only to point out the various ways in which we may deal with this subject that I have mentioned these different lines of research, and in order that it may be understood that I have not overlooked their importance. It is to one particular property of the eye that my own observations have been chiefly directed, namely, the power of accommodation for distance; and I shall endeavour to show that in birds great range of vision depends upon the development and character of the ciliary muscle, to which all are agreed that the power of adjustment is to be attributed.

It is chiefly, then, a comparison of the ciliary muscle in different birds to which I invite attention, assuming the perfection of the sight to depend on this power of accommodation, and that again on the character of the muscle. Let me first mention the general opinion entertained by those who are best acquainted with the habits of that class of birds which astonish us by the rapidity and duration of their flights, namely the pigeons, in regard to the means by which they accomplish them. In his

interesting work on this subject Mr. Tegetmeier gives his reasons for concluding that "Homing," as it is termed in the Antwerp pigeon, is not the result of "instinct," but of "observation." These pigeons require to be trained stage by stage, or they are certain to be lost. The best of them refuse to fly in a fog or in the dark. They crave in new localities some known landmark; and hence their gradually increasing gyrations, until having descried some familiar object, they recollect their route and fly straight ahead. The objection that no pigeon can possibly see for two hundred miles ahead is met by the details of aëronautic experience. Mr. Glaisher, half a mile aloft in air, could embrace in his "bird's-eye view" the course of the Thames from the Nore to Richmond; and Mr. Wheelwright, though puzzled to account for the flying pigeons "homing" across seas (as from London to Antwerp), which can offer no landmark, is disposed to attribute their power of doing so to their habit of soaring round, circling, and beating about until, sooner or later, they can descry their familiar guide-posts.

My own observations entirely support Mr. Tegetmeier's conclusions. This part of my subject is one of general interest, and I trust that I shall be pardoned for attempting to alleviate the tediousness of anatomical details by this digression.

It must clearly be understood that perfection of sight for very near objects is as important as very extensive range, and that the chief function of the ciliary muscle is to adjust the sight for the former rather than for the latter. When the eye is at rest (that is to say, when the muscle is relaxed) vision of very distant objects is permitted; and it is when the distance is diminished to a very few inches, and in small species of birds to considerably less than an inch, that the action of the muscle is exerted.

The exact functions performed by the ciliary muscle in all those vertebrata in which it exists are still undecided; but it is not difficult to reconcile the accounts which have been given by different anatomists of its structure, if we are aware of the fact that the muscle does not possess the same characters in all classes of animals; indeed that it is not precisely the same in those that are very nearly allied, so that it is important, particularly in the case of birds, as will be seen, to mention the species under consideration.

It may be stated generally that in birds it is developed in a remarkable degree; in fish it is entirely wanting; in the mammalia it varies directly in proportion to the powers of sight possessed by the species, except in the feline class and in those animals which enjoy the power of nocturnal vision, and in which the ciliary muscle is peculiarly large and differently developed from the same structure in other mammals.

The three specimens which are to be described belong to the Eagle Owl, the Egyptian Vulture, and the Buzzard. They were brought from Egypt by a gentleman who shot the birds himself, and removed the eyes while in the fresh state, preserving them in spirit of wine till he sent them to me.

The eye of the Eagle Owl presents in the most striking degree the peculiar characters of the class to which it belongs. The first of these are its shape and size, too well known to require description, adapted as they are to the very shallow cavities of the orbits.

In the Egyptian Vulture the pyramidal shape of the eye is less remarkable, and a slight approach is observable in it to the spherical globe. In the Buzzard this is still more marked, and the eye resembles as much the eye of the Pigeon as it does that of the Eagle Owl.

In examination of specimens which have been preserved in spirit, it is necessary to restore the pliancy of the tissues of the ciliary muscle by allowing them to remain in water for some days; and I may observe that as this condition must be obtained in order to make satisfactory preparations, the method of using solutions of chromic acid or the bichromate of potash to enable the anatomist to make sections is not to be recommended, if the object be to ascertain the dimensions of the muscle and the elasticity of the ligament, which will be presently described. It need hardly be stated that the best mode of treating the eye is to freeze it and then make sections.

The strong plates of bone which exist in the sclerotic of birds preserve the shape of the eye sufficiently well to allow of the dimensions being ascertained after it has been preserved in spirit.

In the Eagle Owl the dimensions are as follow:—

	in.
Diameter of cornea	$\frac{7}{8}$
Diameter of base of eye	$1\frac{9}{32}$
Antero-posterior length	$1\frac{6}{16}$
Lateral diameter of lens	$\frac{9}{16}$
Antero-posterior diameter of lens................	$\frac{1}{2}$

The shape of the lens does not appear to be altered by the action of alcohol; but the size is diminished, and the measurements just stated are less than they would be found to be if the lens had been perfectly fresh.

The eye is first to be divided into equal halves by cutting through the sclerotic, choroid, cornea, and iris. We may regard the sclerotic as a hollow case enclosing a sphere, of which the choroid is the proper covering, and which sphere is attached to its case by tissues of highly elastic and muscular properties, by which a certain amount of movement is capable of being effected in the parts on which the formation of the image depends. It is to be observed, however, that the posterior surface of the choroid is kept in close apposition with the inner and posterior surface of the sclerotic, so that movement of the anterior parts is not communicated to that part on which the optic nerve is expanded. In the eye of the Eagle Owl these conditions are obtained in the following manner.

The whole of the posterior surface of the choroid which corresponds to the optic disk is kept in close apposition to the sclerotic by the direct attachment of the circumference of the part immediately beneath the

margin of the retina; it is also fixed where the nerve passes through the sclerotic, while delicate fibres from the choroid keep it in its position at other points.

The anterior part of the choroid, on the contrary, is not in contact with the sclerotic, as the ciliary muscle and the structure I have termed the posterior elastic ligament intervene.

This division of the choroid is not artificial, but is clearly defined by a difference of structure. The posterior part is but slightly vascular, is not elastic, is of considerable tenuity, and has greater resemblance in its general characters to the choroid of fish than to that of the mammalia.

The anterior portion is covered on its internal surface by the ciliary processes, which extend to the angle of curvature of the posterior part of the eye. The tissue of this part of the choroid is of peculiar character; it is dense, strong, and inelastic, and appears to be composed of delicate fibrous tissue. The combination of these characters enables it to preserve its symmetrical shape, and ensure to some degree the preservation of the structures within it. It possesses a rigidity which may be compared to that of ordinary writing-paper, and is of about the same thickness. The anterior part of the choroid is attached to the sclerotic by another structure—a system of fine elastic fibres which pass from the corneal margin of the sclerotic to the line of union between the iris and the choroid, and for which I proposed the name of anterior elastic filaments. Between the anterior elastic filaments and the posterior elastic ligament (a distance in the eye of the Eagle Owl of nearly five eighths of an inch) is interposed the ciliary muscle. The body of the muscle is attached to the line of union of the sclerotic and cornea, so that it may be said to arise from the anterior angle of curvature. The greater part of the posterior portion of the muscle is of delicate tendinous structure; its line of insertion into the choroid is the same as, but on the opposite side of, the line of insertion of the posterior elastic ligament. The breadth of the latter structure is about one eighth of an inch, while the length of the anterior elastic filaments is nearly the same. Thus, passing from before backwards, we have the anterior elastic filaments, the body of the ciliary muscle, its long delicate tendinous portion, and lastly the posterior elastic ligament. To exhibit the structures satisfactorily, the best plan is to make a section of the choroid and sclerotic of one sixteenth of an inch in thickness, and after fixing the two ends of the section on a layer of cork with needles, to dissect the muscle under water or alcohol—a very simple process if a magnifying-glass of an inch focus is employed. It is only necessary to draw the iris gently away from the sclerotic so as to extend the anterior elastic filaments, fixing it with a needle, and then to do the same with the choroid, taking care to hold that membrane at a point posterior to the line of insertion of the posterior elastic ligament.

The length of the ciliary muscle is about three eighths of an inch. I have attempted to preserve sections made in this way in Canada balsam, but

have found that rupture of the ligament usually takes place, I presume from its tenacity being destroyed by the action of the fluid. It is on that part of the choroid which lies between its two lines of attachment, on its internal surface, that the ciliary processes are developed, and to the anterior part of those processes that the crystalline lens is attached. Contraction of the ciliary muscle, it is reasonable to suppose, would produce a change in the position of the lens, and would take place when the object to which the sight was directed was close to the eye—that is to say, the muscle is employed in accommodation for short range of vision. The position of rest is restored by the posterior elastic ligament, which acts in direct opposition to the muscle.

The eye of the Vulture is smaller than that of the Owl, is not so decidedly pyramidal in shape, and may be placed between the latter and the eye of the Buzzard. The chief difference, however, between them is in the greater degree of concavity which the posterior portion of the sclerotic assumes; so that in the Owl the retina lies on a flatter surface than in the Buzzard, while in that respect the Vulture is between the two.

The dimensions of the eye of the Vulture are as follow:—

	in.
Diameter of cornea	$\frac{9}{16}$
Lateral diameter of the sclerotic in its broadest part	$1\frac{3}{16}$
Antero-posterior diameter of eye	$\frac{15}{16}$
Length of ciliary muscle	$\frac{9}{32}$
Breadth of the posterior elastic ligament	$\frac{1}{10}$

Length of the anterior elastic filaments approximately the same.

With regard to the anterior elastic filaments and the posterior elastic ligament, it is unnecessary to make further remark, beyond that they resemble those structures in the eye of the Owl.

In the Buzzard the dimensions of the eye and its structures are as follow:—

	in.
Diameter of cornea	$\frac{4}{10}$
Lateral diameter of eye	1
Antero-posterior diameter of eye	$\frac{3}{4}$
Length of ciliary muscle	$\frac{3}{16}$
Length of posterior elastic ligament } Length of anterior elastic filament }	$\frac{1}{16}$

In order to ascertain the mechanical effect produced by the ciliary muscle, the simple experiment may be performed of applying traction, by means of a pair of forceps, on the choroid, the dissection being arranged and fixed as I have described. It will readily be seen that the elastic ligament acts in direct opposition to the muscle, and in the living eye has the power of restoring the parts to the condition of rest.

The ciliary muscle is composed of striated fibre of very distinct character.

It varies, as is seen in the three examples described, in length and amount of muscular tissue. The tendon in the Owl is long and the body of the muscle short; but in the other species, as in most birds, the muscular fibres extend to a great length, if not entirely from the origin to the insertion of the muscle. These minute differences should be pointed out in detail in the case of each species of bird.

The elastic ligament is composed of very delicate elastic tissue, the microscopical character of which is well defined.

On the peculiar nature of the anterior elastic filaments I beg to postpone any decided opinion.

With regard to the nerves which supply the ciliary muscle and the iris, I have no particular remarks to offer, as the description which I gave some years ago of the ganglia and plexuses on the ciliary nerves in the eye of the Pheasant will apply generally to all birds. Whether the contraction of the iris and the accommodation of the sight be voluntary or involuntary actions on the part of birds we cannot say positively; I am inclined to believe that the latter is the case.

For the sake of convenience, and to render any further researches on the dimensions of the different parts of the eye in other species of birds symmetrical with those contained in this communication, I have arranged the principal facts in the following tabulated form:—

	Eagle Owl.	Egyptian Vulture.	Buzzard.
Diameter of cornea	·875	·506	·4
Greatest diameter of sclerotic (transversely)	1·312	1·182	1
Antero-posterior diameter	1·375	·932	·75
Diameter of lens (transversely)	·506	Not recorded.	·343
Antero-posteriorly	·5	....	·22
Length of ciliary muscle	·375	·3	·187
Breadth of posterior elastic ligament	·125	·1	·063
Length of anterior elastic filaments .	·125	·1	·063
Character of ciliary muscle	Body short, tendon long.	Muscular fibres form more than three fourths of it.	Muscular fibres extend from origin to insertion.

From the above Table we may draw the following conclusions:—that in the Eagle Owl the range of vision is small, the power of accommodation very rapid; in the Vulture range of vision is great, the power of accom-

modation considerable, but slower than in the Owl; in the Buzzard the range of vision is greater still, and the power of accommodation capable of being readily and extensively exercised.

These conclusions, I think, will be found to accord with the observations of those who have had opportunities of making themselves acquainted with the habits of the birds during life.

It has been usual for those who have devoted much attention to the physiology of vision to propose some original and independent explanation of the means by which accommodation for distance is effected, if their researches have been attended with the observation of any previously unknown facts connected with the subject, either experimental or anatomical. It appears to me that as yet we have not sufficient data to afford a perfectly satisfactory explanation of that remarkable property possessed by the eye, partly on account of the difficulty of ascertaining the exact functions of different structures, and particularly by reason of the very various conditions which the same structures assume in various species of vertebrate animals. The line of investigation which is pointed out in this communication it is by no means certain will assist in the solution of the problem of the means by which adjustment for distance is effected; but I am inclined to think that we have not yet exhausted all the resources which careful anatomical inquiry places at our command, and that when a sufficient number of details have been collected, the subject will be in a more suitable state for the application of optical laws than it is at present.

Supplement, containing a Description of the Eye in *Rhea americana*, *Phœnicopterus antiquorum*, and *Aptenodytes Humboldtii*. Received April 27, 1872.

In the American Ostrich the eye is large, and the structures concerned in the adjustment for distance are well developed. In the Ostrich (*Struthio camelus*) the observation was first made by Sir P. Crampton of the existence of the ciliary muscle; and as the views of physiologists regarding the mechanical functions of the muscle in the accommodation of sight were various, while numerous inquiries were made very soon after the publication of this new anatomical fact, I am gratified in having the opportunity of pointing out the cause of the discrepancies in opinion which have continued to the present time.

The description which Crampton has given is correct so far as it goes, but it was limited to that part of the ciliary muscle which forms the thickest portion of it, that is to say, the dense part which lies closest to the margin of the cornea. The tendon of the muscle and its insertion into the choroid were not observed by Crampton, and the structure termed the posterior elastic ligament was overlooked. It can thus be explained how it was that the deflection of the margin of the cornea and consequent

change in its curvature were advanced as the means by which accommodation was effected.

The eye of *Rhea americana* appears to be very similar to that of *Struthio camelus*, though not quite so large. The globe is of irregular shape, and bulges out both laterally and vertically; its diameter in the former direction is an inch and two thirds, in the latter an inch and a half, and antero-posteriorly an inch and one third.

The sclerotic is not particularly thick, and contains but slightly developed osseous structure. The crystalline lens is about half an inch in its lateral diameter, and one third of an inch in its antero-posterior diameter. The ciliary muscle is large and strong, the body thick, and the fibres diminishing in size as they become tendinous near their insertion; its length is $\frac{3}{16}$ inch.

The anterior and posterior elastic ligaments are each about $\frac{3}{16}$ inch in length, though it is to be understood that their elasticity is so great that they might be stretched to a considerably greater length.

In the first part of this communication I expressed some doubt regarding the microscopical character of the anterior elastic ligaments; indeed the term ligament was not applied to them, as they did not possess the same distinct character as the posterior elastic ligament.

In all the species of birds which have come under my observation, the microscopical character of the last-mentioned structure was the same. In the *Rhea* the anterior elastic filaments are distinctly composed of the same kind of elastic fibres; their colour is a light-grey; they coil up very readily when torn from one another with needles; they are to some extent covered with fine granular or spongy tissue, which at first conceals their elastic character; they are continuous and of equal diameter from their origin to their insertion, and are united more closely than in most birds, so that the filamentous character so clearly seen in the Owls is not observed.

A more complete investigation into the anatomy of this part of the subject allows of the conclusion that the anterior elastic filaments are composed of cellular and elastic tissue combined in different proportions, and that the differences in their strength, elasticity, and appearance depend on the collection of the filaments into fibres of varying sizes, or their approximation so as to form a continuous suspensory band between the iris and the cornea.

The iris in this bird is not composed entirely of muscular fibres as in many other genera, but is soft and spongy in its general character, and more like the iris in mammalia than in birds.

As it is desirable to limit myself to those particular structures which are concerned in the accommodation of the eye for distance, deferring for the present certain general conclusions which fresh observations are required to confirm, I shall leave to the consideration of the naturalist the subjoined facts arranged in a tabulated form, and which appear to me to be applicable to the explanation of the habits of the birds by anatomical peculiarities.

	Cornea.	Sclerotic.	Lens.	Ciliary Muscle.	Elastic ligament. Posterior.	Elastic ligament. Anterior.
	in.	in.	in.	in.	in.	in.
Rhea americana.........	vertical $\frac{29}{32}$	vertical $1\frac{1}{2}$	lateral $\frac{11}{32}$	$\frac{2}{16}$	$\frac{1}{16}$	$\frac{1}{16}$
	lateral $\frac{27}{32}$	lateral $1\frac{14}{20}$				
		ant.-post. $1\frac{7}{20}$	ant.-post. $\frac{11}{32}$	fibres long.		
Phœnicopterus antiquorum	vertical $\frac{13}{32}$	lateral $\frac{31}{32}$	lateral $\frac{8}{32}$	$\frac{1}{16}$	$\frac{2}{64}$	$\frac{2}{64}$
	lateral $\frac{1}{24}$ more	ant.-post. $\frac{18}{32}$	ant.-post. $\frac{8}{32}$	gradually diminishing.		
Aptenodytes Humboldtii	$\frac{11}{32}$	lateral $\frac{14}{16}$	lateral $\frac{8}{32}$	$\frac{2}{16}$	$\frac{1}{16}$	$\frac{1}{16}$
		ant.-post. $\frac{12}{16}$	ant.-post. $\frac{8}{32}$	gradually diminishing.		

The Society then adjourned over the Whitsuntide Recess to Thursday, May 30.

May 30, 1872.

GEORGE BIDDELL AIRY, C.B., President, in the Chair.

THE BAKERIAN LECTURE was delivered by WILLIAM KITCHEN PARKER, F.R.S., "On the Structure and Development of the Skull of the Salmon (*Salmo salar*, L.)." The following is an Abstract.

A few years ago Mr. Waterhouse Hawkins put into my hands some newly hatched salmon and also three of the first summer. Seeing their fitness for embryological research and the interest attaching to the formation of an osseous fish, I applied to my friends Messrs. Frank Buckland and Henry Lee, and these gentlemen most liberally supplied me with a large number of unhatched embryos and of the "fry" of this large fish.

My last subject, the frog, being fairly out of hand, I set myself last summer to this newer and more easy task,—more easy by far, for the translucency of the young salmon contrasts most favourably with the obscurity of the embryo frog.

I found that the two types at the time of hatching did not start fairly, but that the salmon had hastened to finish its *fourth stage* before emerging from the egg; this, however, is partly in consequence of the difference of the envelope in which the embryos are contained; for in the salmon this is a leathery "chorion," and in the frog a mere gelatinous bleb.

Moreover, it soon became apparent that these two "Ichthyopsidans" are

in nowise near akin to each other. In the very first stage, where there is an essential agreement, in one important particular they greatly disagree; for the embryo of the salmon has two arches in front of its mouth, while the tadpole has but one; there is also an additional gill-arch in the osseous fish.

In the earliest stage of the salmon worked out by me I found a much more distinct condition of the parts than in frogs at the same stage; the differentiation of the latter is obscure as compared with the fish, and this not merely because of the quantity of *pigmentum nigrum* in the tissues of the former.

Then, in addition to other causes of obscuration, the mouth of the tadpole is strangely modified in harmony with its "suctorial" character and affinities (showing a remarkable affinity to the mouth of a lamprey), so that a whole system of cartilages has to be eliminated from the lips before the mouth (proper) can be understood. The labial system is slightly and slowly developed in the salmon, and its mouth is thus much more in harmony with that of the embryo reptile or bird than with that of the tadpole.

After the simple stage is passed, the development of the facial arches is very different in the two types,—as different, indeed, as in any two possible examples that could be given in the whole vertebrate group.

The facial arches behind the mouth now undergo segmentation; first the hyoid, and then the mandibular. The hyoid is cloven from top to bottom, and also has a single distal piece separated off.

At this stage we get an explanation of what is seen in certain rays, where the hyoid suspensorium is permanently double; and also ascertain that this second postoral arch, which retains the anterior piece in relation to the skull as the great "hyomandibular" pier, does not need the *saw* of the transcendentalist to put it into proper relation to its surroundings. Nature's invisible *wedge* has done what was needed, and the supposed *double rib* turns out to be *half* a visceral arch. On the whole, this second stage is extremely "Plagiostomous," for the details of which I must refer to the main paper.

While in the egg the head of the embryo is flattened, and so twisted that one of the eyes (it may be the left or the right) looks upwards towards the "chorion," the other having a *visceral* direction.

The facial bars, at first having all a simple sigmoid form, rapidly change towards the time of hatching, and, when the head gets free, the cerebral vesicles speedily swell, taking on the form so familiar to the embryologist; and the head now gains the "mesocephalic flexure."

After this an approach is made to the Teleostean type of structure; but this is not done at a stride. The intermediate condition is thoroughly "Ganoid," and, happily, comes in to explain the related structures of the *older* and *newer* "Orders." I am not aware that any stage of the heart or of the intestines shows either the many valves of the "aortic bulb" or the intestinal spiral valve: this must be seen to; yet if these never show

themselves in the "fry" of the osseous fish, their absence does not affect the general skeletal morphology.

The salmon amongst fishes, like the fowl amongst birds, never attains to the greatest degree of special class-modification; it remains *subtypical*, with a dentigerous maxillary, a ductus pneumaticus, a very *chondrosteus* state of the skull, and a very heterocercal tail.

Yet, from an ichthyological point of view, this fish is an immense height above the Sharks and Rays, and is far in advance, as a fish, of the whole group of "Ganoids."

The results of the *gradational* study of the fish-forms by the zoologist, and of their *secular* study by the palæontologist, are both in harmony with morphological facts. Although the light obtained is but as the first streak of dawn, yet it is a pleasant light, and quite sufficient to show each kind of worker where and how to renew his own special toil.

I cannot close this brief abstract without remarking that my researches in these, the highest types of animals, seem to me to be in perfect accordance with the results obtained by long study of the very lowest, the Rhizopods—namely, that they both yield increasing evidence in favour of the doctrine of Evolution.

Researches of this kind show what the life-processes can accomplish in the history of one individual animal, and also that the morphological steps and stages are not arbitrary, but take place in a manner in accordance with all that has of late been revealed to us of the gradation of types in the ages that are past.

The following communications were read:—

I. "On Ammonia in the Urine in Health and Disease." By C. MEYMOTT TIDY, M.B., Joint Lecturer on Chemistry and Medical Jurisprudence at the London Hospital; and W. BATHURST WOODMAN, M.D., Joint Lecturer on Physiology, and Assistant Physician to the London Hospital. Communicated by T. BLIZARD CURLING, President of the Royal Medical and Chirurgical Society. Received April 16, 1872.

(Abstract.)

The authors refer to the researches of Andrews, Clark, Neubauer and Vogel, and others on the presence of ammonia as a constant constituent of healthy urine, Neubauer regarding 10·8 grains per diem as the average normal excretion. After numerous experiments, the authors regard this quantity as excessive, and suggest some reasons for this discrepancy.

The method adopted by the authors is as follows:—

The freshly passed urine is to be first diluted with a given bulk of distilled water, sufficient to destroy all apparent colour, a known quantity of the urine being taken in each case. To these is to be added an excess

of Nessler's solution, and then compared with the tint-depth produced by known quantities of ammonia in a similar bulk of liquid treated with the same solution. The trace of ammonia in the water itself must be always allowed for. The external temperature in the observations was always about 62°.

They regard 60 oz. as the normal daily excretion of urine in adults.

I. *Ammonia in Health.*

In 50 cases the average quantity of ammonia

=0·0825 gr. in 1000 grs. =2·1656 grs. per diem.

The 10 *highest* cases gave an average of

0·1620 gr. per 1000 grs.=4·2525 grs. per diem.

The 10 *lowest* cases gave an average of

0·0252 gr. per 1000 grs.=0·6615 gr. per diem.

The *mean* of these 20 cases gives

0·0936 gr. per 1000 grs.=2·457 grs. per diem.

The authors conclude that the average daily excretion of ammonia in health amounts to rather less than 2·5 grs. in 24 hours.

These results are modified by:—

1st, *age*. The amount of ammonia was largest under the age of 35 years, to the extent of a little over one third.

2nd, *sex*, but very slightly (?).

3rd, *food*. The amount of ammonia *after meals* was nearly double that excreted before meals. Their experiments seem to prove that a large proportion of the urinary ammonia is derived from the food ingested; confirmed by other experiments.

4th, *atmospheric conditions*. The authors consider that the excretion of ammonia by the kidneys is governed by a similar law to that which regulates the formation of dews.

5th, *exercise* increases the excretion of ammonia.

II. *Ammonia in Disease.*

Before dealing with special diseases, the authors examined the influence of some general conditions both of the urine and the patients.

A, *colour* of the urine. (Vogel's colour-scale was adopted.) From No. 1 to No. 7 the ammonia rises in proportion to colour (jaundice and hæmaturia being excluded). True also, to a great extent, of normal urine.

B, *specific gravity* of the urine. The general rule is that the excretion of ammonia keeps pace with the specific gravity.

C, *pulse*. The lowest quantities were met with when the pulse was rapid, the highest average being met with when the pulse was nearly normal.

D, *respirations*. It appears that the amount of ammonia decreases with accelerated respirations.

E, *temperature* (axillary). The largest excretion per 1000 grains is

found with nearly normal temperatures; but when the total quantity of urine is estimated, it is seen that such normal temperatures (as in diabetes) go with increased ammonia.

F, *condition of skin.* The largest excretion occurred when the skin was moist.

G, *condition of tongue.* Excretion largest with moist tongue.

H, *condition of bowels.* Ammonia was slightly in excess when the bowels were open.

I, *diet.* Even in various diseases the amount of ammonia was very much larger when the diet was full and included stimulants.

K, *medicines.* Only acids and alkalies contrasted. The amount was nearly double in the case of *acids.*

III. *Special Diseases.*

A, *acute rheumatism.* In nineteen observations ammonia equalled 0·0684 gr. per 1000 grs.

=1·7955 gr. per diem.

Considerably less than normal.

B, *erysipelas.* In ten observations

NH_3=0·0402 gr. per 1000 grs.=1·0552 gr. per diem.

Very much under health.

C, *diabetes.* In seven observations, with an average of 240 oz. of urine per diem,

NH_3=0·0348 gr. per 1000 grs.=3·654 grs. per diem.

Considerably in excess of healthy urine.

D, *smallpox.* In eleven cases

NH_3=0·0627 gr. per 1000 grs.

E, *enteric fever.* In eleven cases

NH_3=0·0543 gr. per 1000 grs.

F, *typhus fever.* In two cases

NH_3=0·0435 gr. per 1000 grs.

N.B.—The results of B, D, E, and F, considering that the amount of urine will be nearer 25 oz. than 60 oz. per diem, show that the amount of ammonia excreted in these diseases is actually less than one fourth that of health.

G, *cancer* (verified by *post mortem* examinations). In five observations

NH_3=0·0918 gr. per 1000 grs.=2·4097 grs. per diem.

H, *heart disease* (chiefly valvular). In ten cases

NH_3=0·0927 gr. per 1000 grs.=2·4334 grs. per diem.

I, *chronic alcoholism.* In four observations

NH_3=0·1065 gr. per 1000 grs.=2·7956 grs. per diem.

N.B.—G, H, I show ammonia very nearly normal, as might be expected.

K, *chorea.* In six observations

NH_3=0·09 gr. per 1000 grs.=2·3625 grs. per diem.

L, *albumenuria.* In eight observations

$NH_3=0{\cdot}0521$ gr. per 1000 grs.

M, *phthisis.* In five observations

$NH_3=0{\cdot}072$ gr. per 1000 grs. $=1{\cdot}89$ gr. per diem.

N, *nervous diseases.* In five observations

$NH_3=0{\cdot}0546$ gr. per 1000 gr. $=1{\cdot}4332$ gr. per diem.

O, *chronic nodular arthritis* (rheumatic gout). In 4 observations

$NH_3=0{\cdot}15$ gr. per 1000 grs. $=3{\cdot}9375$ grs. per diem,

or nearly double that of health.

P, *gout.* The ammonium seems to be increased in this disease.

Q, In nine cases of *complicated disease*, with extreme physical prostration,

$NH_3=0{\cdot}0069$ gr. per 1000 grs. $=0{\cdot}1835$ gr. per diem.

R, the cases taken *just before death* are very remarkable, showing a vast decrease in the amount of ammonia. Eight cases gave an average of

$NH_3=0{\cdot}0304$ gr. per 1000.

In two cases it was entirely absent, the only cases of entire absence known to the authors.

The authors refrain from any generalization. The total number of cases upon which observations were made exceeded 200.

II. "Examination of the Gases occluded in Meteoric Iron from Augusta Co., Virginia." By J. W. Mallet, Ph.D., M.D., Professor of Analytical and Applied Chemistry, University of Virginia. Communicated by R. Mallet, C.E. Received April 23, 1872.

The investigation by Graham of the gases given off by meteoric iron from Lenarto, in Hungary, when heated in a vacuum produced by a Sprengel pump, excited much interest at the time of publication*, but does not seem to have been followed up by any similar examination of other meteorites. I have made use of pieces of the iron found about three years ago in Augusta Co., Virginia, the description and analysis of which were published by me in the 'American Journal of Science' for July 1871, in order to repeat the experiment of Graham, and ascertain whether similar results to his would be obtained. A large part of the work of the extraction and analysis of the gaseous contents of this iron has been done by two of the students in my laboratory, Mr. F. P. Dunnington, of Baltimore, and Mr. J. B. Adger, of South Carolina, to whom I am much indebted for their assistance.

Two preliminary experiments were made,—the first with some shavings from the cutting of the iron upon a planing-machine; the second with a solid piece of the metal planed to smooth, clean surfaces, and quite free

* Proc. Roy. Soc. xv. p. 502.

from any crust or scale. The shavings were subjected to the purification practised by Graham, namely, washing with a hot solution of potassic hydrate, followed by washing with distilled water and thorough drying. The solid strip of iron was not so treated, care having been taken to use no oil upon the tool employed in cutting it. Both specimens gave off gas readily when heated in the Sprengel vacuum, the amount in each case being larger in proportion to the bulk of the iron than in the experiment of Graham; and analysis showed that the same gases were present as those found by him, with the addition of carbonic anhydride in not inconsiderable amount.

The final experiment was made as follows, with great care, and with all precautions which could be thought of to avoid error.

A parallelopiped of iron was cut upon a planing-machine from the largest of the three masses found (that spoken of as No. 1* in the paper above referred to), the work being done with special care, to avoid the least trace of grease being derived from the machine.

Not only was the cutting-tool itself made red-hot in the blacksmith's fire, hardened in clean water, and tempered and ground without contact with any thing greasy, but every part of the machine-bed, set-screws, and frame, from which any risk was to be feared, was carefully cleansed, and paper used to cover the whole of the iron, except where actually borne upon by the tool. The piece of iron measured about 75 millims. long, 16 millims. wide, and 12 millims. thick. It was cut from as solid a portion of the mass as could be found, and was quite bright upon the surface and free from crust, though traces of a very minute crack or fissure were barely perceptible at one end. The piece weighed 124·589 grammes; and as the specific gravity of the iron had been found to be 7·853, the volume was 15·87 cub. centims. A new and perfectly clean porcelain tube, with sound glaze, was used, heated by a small upright fire-clay furnace with good draught, through holes in the opposite sides of which the tube was passed. The fuel was charcoal, in pieces a little larger than a walnut. The Sprengl pump had a fall-tube of about 1·34 metre long; its connexions were made with great care, and were protected by outer casings of india-

* The results of ordinary analysis were:—

Iron	88·706
Nickel	10·163
Cobalt	·396
Copper	·003
Tin	·002
Manganese	trace.
Phosphorus	·341
Sulphur	·019
Chlorine	·003
Carbon	·172
Silica	·067
	99·872

rubber tube, with the annular space between the tubes filled with glycerine. A plate of glass floating on the mercury in the funnel at top served to prevent the risk of air being carried down, as the metal was gently poured on through another and smaller funnel with narrow aperture.

A good vacuum having been obtained in the cold, lighted charcoal was placed in the furnace, and gas very soon began to come off.

It was determined to analyze separately that collected at the beginning, middle, and end of the process, in order to see whether the different constituent gases were given off at the same or at different rates. The total amount obtained was 36·33 cub. centims., reduced to 0° C., and 1 metre pressure. This was divided into three portions for analysis as follows:—

			h	m
Portion A....	52·02	per cent. of the whole was collected in	2	30
Portion B....	24·11	„ „ „	2	20
Portion C....	23·87	„ „ „	9	40
	100·00		14	30

It will be seen that the greater part came off within the first two hours and a half; but the process lasted *fourteen hours and a half*, and was not entirely over at the end even of this time. The heat had been gradually raised from dull redness to something nearly approaching whiteness at the end of the time; and when the experiment was stopped very small but still perceptible traces of gas were still coming off, though their appearance was immediately arrested whenever the temperature was allowed to fall but a little below the high point which had been reached.

The piece of iron taken out from the tube when it had become quite cold was found glazed by a thin film of fused phosphide of iron and nickel (Schreibersite), thickest on the edge which had been lowest, this phosphide having oozed out from the mass at the very high temperature used.

The tubes used to collect the gas during the first portion of the time occupied in the experiment were found slightly moistened on the inside, and the moisture, which had a distinctly acid reaction, was proved to contain hydrochloric acid, this having no doubt been derived from the chlorine existing in the iron in combination with that metal and with nickel.

Careful analysis of the gas yielded the following results by volume for the three portions separately collected: the fourth column of figures, obtained by summing up the three which precede it, gives the percentage composition of the whole of the gaseous matter extracted from the iron:—

	Portion A.	Portion B.	Portion C.	Total gas.
Hydrogen	22·12	10·52	3·19	35·83
Carbonic oxide	15·99	11·12	11·22	38·33
Carbonic anhydride .	7·85	1·02	·88	9·75
Nitrogen	6·06	1·45	8·58	16·09
	52·02	24·11	23·87	100·00

Other gases were tested for, but none could be found; no free oxygen could be detected, nor any compound of carbon and hydrogen.

From these figures it appears that hydrogen maintains about the same proportion to the other gases in A and B, but diminishes largely in C, that carbonic oxide increases in amount in B as compared with A, but remains about the same in relative amount in C, that carbonic anhydride diminishes throughout the whole continuance of the experiment, and that nitrogen falls off in B as compared with A, but largely increases again in C.

Contrasting the results with those of Graham, and noticing first the total volume of gas obtained from the iron, it becomes necessary to reduce this volume to the same standards of pressure and temperature employed by him. In the paper read before the Royal Society, as reported in its 'Proceedings,' I find no statement in regard to such standards; but, supposing it probable that the barometer at 30 inches and thermometer at 60° F. were referred to, I have calculated the volume of gas obtained in all from 15·87 cub. centims. of iron as equivalent under these conditions of pressure and temperature to 50·40 cub. centims., or 3·17 times the volume of the metal. This is a somewhat larger quantity than that of Graham, namely 2·85 times the volume of the Lenarto iron used; but the time of heating was longer in the experiment now described, and the temperature attained probably much higher.

As to the nature and relative amount of the constituent gases, the results differ very noticeably from those of Graham, as is evident when the figures of the two analyses are placed side by side:—

	Lenarto iron.	Augusta Co., Virginia iron.
Hydrogen	85·68	35·83
Carbonic oxide	4·46	38·33
Carbonic anhydride	——	9·75
Nitrogen	9·86	16·09
	100·00	100·00

The gases obtained in the experiment now in question agree more nearly with those of common wrought iron (clean horseshoe-nails) as found by Graham*, viz. in the first portion collected,—

Hydrogen	35·0
Carbonic oxide	50·3
Carbonic anhydride	7·7
Nitrogen	7·0
	100·0

and the conclusion arrived at by him, that "the predominance of carbonic oxide in its occluded gases appears to attest the telluric origin of iron,"

* *Loc. cit.*

would deny to the Virginia specimen the right to be classed amongst meteoric masses, with which, however, all its other physical and chemical characteristics agree most fully.

It is to be noted that the analysis of the gases from the Lenarto iron was not made with the whole of the gaseous matter collected: the first portion, amounting to about 32·5 per cent. of all collected, was used for merely qualitative examination; the second portion, 57·6 per cent., was that fully analyzed; while no mention is made of the disposition of the remaining third portion of 9·9 per cent.; and it is stated that the iron was not fully exhausted at the end of two hours and thirty-five minutes, for which time only the experiment was continued. In my own experiment it appears probable that the amount of hydrogen (and with it the total volume of gas) has been slightly diminished by its union with chlorine of metallic chlorides to form the minute quantity of hydrochloric acid observed in the faint film of moisture on the sides of the first tubes; and probably also this moisture itself may have been caused by the partial reduction, by means of hydrogen, of carbonic anhydride to carbonic oxide. Although it might be assumed, especially in view of the strong tendency of iron to take up and "occlude" carbonic oxide, that this gas had been the original form in which the gaseous carbon compounds obtained existed in the iron, and that it had in part broken up at the temperature of the experiment into carbon (remaining united with the iron) and carbonic anhydride (which escaped as gas), yet, in view of the steady decrease in the quantity of this latter gas collected as the experiment proceeded and the temperature became higher, and bearing in mind the ready decomposition it undergoes in contact with ignited iron, it seems more likely that a larger amount of carbon originally existed in the iron in this higher state of oxidation than appears from the figures of the analysis. Although the proportion of hydrogen found is so much less in the Virginia than in the Lenarto iron, it yet represents for the former about 1·14 times the volume of the iron itself, whereas common terrestrial iron occludes but about ·42–·46 of its own volume under ordinary pressure.

I am quite satisfied, from the condition of the masses of iron as they came into my hands, and especially from the character of the crust, that the metal has not been subjected to any heating in a blacksmith's fire or otherwise by human hands since it was found, as has sometimes happened to similar specimens in the endeavour to discover their nature, or to make use of them.

Whether or not this analysis be considered as furnishing presumptive evidence of the Virginia iron having come to our earth from a different atmosphere to that of which the Lenarto meteorite brought us a sample*, the result differs so far from that of our sole previously recorded determi-

* Some of the observations of Secchi and Huggins seem to render it probable that carbon may play an important part in some regions of the universe, though the results on this head are not as full or satisfactory as those in reference to hydrogen.

nation of the kind as to make it a matter of much interest that a larger number of meteoric irons from various localities should be subjected to careful examination in the same direction, thus supplementing our knowledge of the fixed constituents of these curious bodies by a study of their gaseous contents.

III. "On the Structure and Function of the Rods of the Cochlea in Man and other Mammals." By Urban Pritchard, M.D. Communicated by Prof. Lionel Beale, M.D. Received April 18, 1872.

(Abstract.)

The ear is, it is well known, one of the most complicated organs of the body, consisting of the external, middle, and internal sections, the two former being concerned in collecting and conducting sounds or vibrations, while the duty of the internal portion consists in receiving, localizing, and clearly distinguishing them. It is simply with this last function of the organ that I purpose to deal, my aim being to describe the true construction and use of the cochlea, so far as its task of distinguishing the various sounds is concerned. This cochlea, it must be borne in mind, consists of a spiral canal, in form and shape very similar to the inside of a snail-shell. From the axis of this spiral, there proceeds horizontally a plate of bone, the lamina spiralis, almost dividing this canal into two; from this plate, again, there extend two membranes, the membrane of Reissner and the lamina spiralis membranacea, as far as the walls of the canal, thus separating it into three minor canals.

Between the layers of the membranous spiral lamina are situated the so-called Rods of Corti. These were first discovered and described by the Marquis de Corti; and although since then many observers have studied the subject, yet scarcely two investigators are agreed as to their exact form.

Deiters has published the results of two investigations, in which the form of the rods is differently described; Kölliker, Henle, and others appear to agree with Deiters's later view, and most of our text-books have copied their drawings. Recent writers, such as Dr. A. Böttcher, Waldeyer, &c., give varying drawings, some of which are nearer the true form of the rods than that of Deiters, while others exhibit them in all kinds of extraordinary shapes.

In a general view of the rods from above, they appear similar to two rows of pianoforte-hammers, rather than like the keys of that instrument, to which they have been likened. In a lateral view, these two rows of rods are seen sloping towards each other, like the rafters of a gabled roof. The rods consist of a shaft and two enlarged extremities, but the two rows differ considerably in form; the inner rods are attached by their

lower extremities to the membrana basilaris at its junction with the lower lip of the limbus, and just external to the spot where the nerve-filaments emerge; they are directed outwards and upwards, with a slight undulation to meet the outer rods. The lower extremity is enlarged and rounded, gradually tapering to the shaft, which is cylindrical; the upper extremity is somewhat cuboid in form, but the outer surface is deeply concave, and the upper lip of the concavity is prolonged into a process.

The outer rods are attached to the membrana basilaris by a broad base, which also gradually tapers to a cylindrical shaft. Their upper extremity is less cuboid in form, and presents a convex internal surface, which articulates with the corresponding concavity in the inner rods just mentioned; from the outer and upper part there extends outwards a slender process.

One of the most important features with regard to these rods is their relative length. Most authors state that there is very little difference in the length of the two rods; in this, however, they are much mistaken; for not only do the two sets of rods differ in this respect, but the length of each varies according to its position on the cochlea. Thus, at the base, the outer rods are as nearly as possible equal in length to the inner; but proceeding upwards, both rows increase in length with great regularity, although not in the same ratio, the outer increasing with much greater rapidity, so that near the apex they are twice the length of the inner.

It was generally supposed, *à priori*, that these rods were graduated so as to distinguish the most minute variation of tone, but no one until now has been able to demonstrate this.

The rods, therefore, vary in length from about $\frac{1}{500}$ to $\frac{1}{200}$ of an inch. The number of rods in each row is not the same, there being about three of the inner to two of the outer; and, according to calculation, there are about 5200 inner rods and 3500 outer in the whole cochlea.

Most authors, with the exception of Deiters, describe nuclei situated in various parts of these rods, principally in the lower extremities; but although seen from above this appears to be the case, on closer observation these so-called nuclei of the rods are found to be nothing more than the nuclei of cells surrounding them.

The arrangement of the nerves.—The cochlear nerve-fibres from the portio mollis pass up the modiolus, and turn off at the lamina spiralis. Just at this junction we find in the bone itself a ganglion, from which the fibres proceed outward. Immediately before the end of the lower lip of the limbus, the nerve-filaments pierce its upper surface, and appear close to the base of the inner row of rods; concerning the termination of these nerve-filaments little is really known.

Corti and most other authors considered this system of rods to be the essential portion of the cochlea; they supposed that the rods received the vibrations conducted to them, and being set in motion, so affected the nerves as to cause the brain to appreciate the various sounds. Later German writers have attributed the appreciation of the various vibrations

to certain delicate cells which are attached to the under surface of the membrana reticularis. From this circumstance alone it appears very evident that these investigators had not suspected, much less discovered, the fact that the rods are most exquisitely graduated, for otherwise they could surely never have doubted that so beautiful and suitable an apparatus could have any other ostensible purpose than that of appreciating the various sounds. I consider, indeed, that the cochlea represents a musical instrument, similar in nature to a harp or musical box, the strings of the one and the teeth of the other being represented by the rods of Corti. The spiral bony lamina is simply a sounding-board; around the rods are placed the various nerve-cells and nerve-fibres, and from these cells the impressions are conveyed by the fibres to the brain itself.

It is possible, therefore, to trace very completely the course of sounds or vibrations from a musical instrument or any other source to the brain, through the medium of the ear. First the vibrations are caught and collected by the auricle, and transmitted through the external meatus to the drum of the ear, next across the middle to the internal ear. Here the sound is appreciated, merely as a sound, by the vestibule; the direction is discovered by means of the semicircular canals; but to distinguish the note of the sound, it must pass on to the cochlea. The vibration therefore passes through the fluid of the cochlea and strikes the lamina spiralis, which intensifies and transmits the vibration to the system of rods. There is doubtless a rod not only for each tone or semitone, but even for much more minute subdivisions of the same; so that every sound causes its own particular rod to vibrate, and this rod vibrating, causes the nerve-cells in connexion with it to send a nerve-current to the brain.

In conclusion, I feel it my duty to mention that I am greatly indebted to Professor Rutherford, of King's College, for suggesting the investigations which led to these results, as also for much valuable advice while prosecuting them in his laboratory at King's College.

Addition to Lieut.-Col. A. STRANGE's paper "On a new Great Theodolite to be used on the Great Trigonometrical Survey of India, with a short Note on the performance of a Zenith-Sector employed on the same work." (See p. 317.)

(Received June 15, 1872.)

Since my paper with the above title was read, it has occurred to me that some particulars as to the weight of the instrument might interest those engaged practically in Geodesical work. The following are the weights of the main parts as separated for carriage:—

		lbs.
(1)	*Stand,* with Levelling-apparatus complete	108
(2)	*Iron Circle of Stand* with 3 Centering-Screws	156
(3)	*Tribrach,* with Horizontal Circle, Guard ditto, Vertical Axis, and Relieving-apparatus	180
(4)	*Vertical-Axis Socket,* with 5 Horizontal-Microscope arms, Elliptical Table, Pillars, 2 Azimuthal Levels, 2 Vertical ditto, 2 Vertical Microscopes, 2 Pointer ditto, Horizontal Tangent-Screw plates	235
(5)	*Telescope,* with Vertical Circle	64
(6)	Miscellaneous appliances and spare parts (about)	130
	Weight of Instrument	873
(7)	Aggregate weight of field packing-cases, assumed at $\frac{2}{3}$ the weight of contents	582
	Total weight of Instrument in field-cases	1455

The heaviest package (4) packed in field order will be,—instrument 235 lbs. + case ($\frac{2}{3}$) 157 lbs.: total 392 lbs. This is less by about 100 lbs., according to my recollection, than the heaviest package of Troughton's Great Theodolite, long in use with the Indian Survey, and with which I myself worked for five years in a country parts of which were extremely rugged and mountainous. Expense apart, the portability of an instrument is of course defined by the weight of its heaviest portion.

Nevertheless the instrument is considerably heavier than I should wish. There are three ways of diminishing this inconvenience:—first, by diminished dimensions; second, by mode of construction; third, by choice of materials. I will say a few words on each.

When the designing of this instrument was placed in my hands by Sir Andrew Waugh, the late Surveyor-General of India, he left the question to my decision whether the Horizontal Circle (which governs the whole) should be 30 or 36 inches in diameter. With my five years' experience of the larger size, and with ample experience of smaller ones, I deliberately preferred and adopted the larger, and am responsible for that decision. I believe myself to be in the minority on this question; but can only say that, if I had to use the instrument myself, I should still prefer the larger size, attaching, as I do, great importance to the greater optical power and the superior stability which are secured by it: for these I would myself willingly sacrifice convenience to a great extent.

By "mode of construction," I mean particularly the degree in which the main parts are cast in single masses. That principle, when the design was commenced, had been notably illustrated by the present Astronomer Royal and President of this Society, in the new instruments designed by him for the Royal Observatory and now in use there. Like many others, I was at the time fascinated by it; but I am now disposed to think that this principle has been carried too far in my own design under notice. Discretion is requisite in applying even an unquestionable principle. And I now

think that it is not judicious to apply the same method to a portable as to a fixed instrument; to do so introduces the difficulty of carriage, and this must result in curtailing size, and with it power. Casting in masses professes to diminish or eliminate the strains and tensions supposed to be incident to building up the structure with separate pieces. Whatever may have been the case some years ago, I am disposed to believe that this superiority is now much less marked than it used to be, if, indeed, it exists at all at present. Self-acting shaping-tools have been brought to such perfection, that the fitting of contiguous surfaces to each other can now be made practically absolute, to the exclusion of those strains which imperfect fitting must of course formerly have introduced. Nor must it be forgotten that even casting in masses does not exclude irregular strains, and that the more unequal in thickness the different parts of a mass are, the more such strains are to be apprehended. Let any one cast even so symmetrical a thing as a ring, say 3 feet in diameter and 3 inches thick, divide it into two halves, and afterwards attempt to fix these halves together; he will find them infallibly distorted. Every one who has turned large irregularly shaped masses of metal in a very accurate lathe, knows that every cut of the tool alters the *general* form of the mass. In the instrument under discussion, the principle in question has, I now believe, been carried too far; it has greatly increased weight, whilst I doubt whether it has diminished unequal tensions.

As to choice of materials, I have already mentioned that the use of aluminium bronze has tended materially to diminish weight. I am of opinion, however, that steel can now be employed for such a purpose almost exclusively, since at the present day it can be cast and worked as readily as brass, though of course at rather greater expense, owing to its hardness. The various shaping-machines and drill-slotting machines give almost unlimited control over the forms that may be given to the solid masses, while sheet-steel is now an article of ordinary commerce available for the tubular parts. The nickelyzing process, now extensively employed in America, and becoming every day better known and appreciated here, effectually preserves steel from oxidation.

Practical surveyors are alive to the question of facility in dismantling, packing, and setting up so large an instrument as this. I am able to give some data on this head. After exhibiting the instrument to the Society at Burlington House, Piccadilly, it had to be removed to my Observatory in Belvedere Road, Lambeth; it was taken to pieces, packed, loaded on a van, conveyed to its destination, and again set up in the Observatory ready for adjustment and use in three hours. I should mention that the passages through which the various parts had to be carried from the Meeting-room to the cases in the outer hall are tortuous, narrow, and dim, that the distance from Burlington House, about 1¾ mile, was traversed at a foot's pace, in order that the men might be at hand in case of accidents, and that ten ordinary labourers of the India Store Department, with my assistant and myself, were employed in the operation.

Presents received May 2, 1872.

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John V. L. Pruyn, Esq., Chancellor of the University of New York, by the Earl of Rosse, V.P.R.S.

May 16, 1872.

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June 6, 1872.

The Annual Meeting for the election of Fellows was held this day.

Mr. WILLIAM SPOTTISWOODE, M.A., Treasurer and Vice-President, in the Chair.

The Statutes relating to the election of Fellows having been read, Sir James Alderson and Prof. A. W. Williamson were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present having been collected, the following Candidates were declared to be duly elected into the Society:—

Prof. William Grylls Adams, M.A.	Prof. William Stanley Jevons, M.A.
Andrew Leith Adams, M.B.	Prof. George Johnson, M.D.
Frederick Le Gros Clark, F.R.C.S.	Prof. Thomas Rupert Jones.
Prof. John Cleland, M.D.	Major Thomas George Montgomerie, R.E.
Prof. Michael Foster, M.D.	Edward Latham Ormerod, M.D.
Prof. Wilson Fox, M.D.	Edward John Routh, M.A.
Arthur Gamgee, M.D.	William James Russell, Ph.D.
Rev. Thomas Hincks, B.A.	

Thanks were voted to the Scrutators.

June 13, 1872.

Sir JOHN LUBBOCK, Bart., Vice-President, in the Chair.

Mr. F. Le Gros Clark, Dr. Wilson Fox, Dr. George Johnson, Dr. E. Latham Ormerod, Mr. E. J. Routh, Dr. W. J. Russell, and Colonel Tennant were admitted into the Society.

The following communications were read:—

I. "On the Spectrum of the Great Nebula in Orion, and on the Motions of some Stars towards or from the Earth." By WILLIAM HUGGINS, LL.D., D.C.L., F.R.S. Received May 2, 1872.

In my early observations of the spectrum presented by the gaseous nebulæ, the spectroscope with which I determined the coincidence of two of the bright lines respectively with a line of nitrogen and a line of hydrogen was of insufficient dispersive power to show whether the brightest

nebular line was double, as is the case with the corresponding line of nitrogen.

Subsequently I took some pains to determine this important point by using a spectroscope of greater dispersive power. I found, however, that the light furnished by the telescope of eight inches aperture, to which the spectroscope was attached, was too feeble, even in the case of the brightest nebulæ, to give the line with sufficient distinctness when a narrow slit was used. The results of this later examination are given in a paper I had the honour of presenting to the Royal Society in 1868. I there say * :— "I expected that I might discover a duplicity in the line in the nebula corresponding to the two component lines of the line of nitrogen, but I was not able, after long and careful scrutiny, to see the line double. The line in the nebula was narrower than the double line of nitrogen; this latter line may have appeared broader in consequence of irradiation, as it was much brighter than the line in the nebula." When the spark was placed before the object-glass of the telescope, the light was so much weakened that one line only was visible in the spectroscope. "This line was the one which agrees in position with the line in the nebula, so that under these circumstances the spectrum of nitrogen appeared precisely similar to the spectra of those nebulæ of which the light is apparently monochromatic. This resemblance was made more complete by the faintness of the line; from which cause it appeared narrower, and the separate existence of its two components could no longer be detected. When the line was observed simultaneously with that in the nebula, it was found to appear but a very little broader than that line." I also remark :—"The double line in the nitrogen-spectrum does not consist of sharply defined lines, but each component is nebulous, and remains of a greater width than the image of the slit. The breadth of these lines appears to be connected with the conditions of tension and temperature of the gas. Plücker† states that when an induction-spark of great heating-power is employed, the lines expand so as to unite and form an undivided band. Even when the duplicity exists, the eye ceases to have the power to distinguish the component lines, if the intensity of the light be greatly diminished." I state further :—"I incline to the belief that it [the line in the nebula] is not double."

One of the first investigations which I proposed to myself when, by the kindness of the Royal Society, I had at my command a much more powerful telescope, was the determination of the true character of the bright line in the spectra of the nebulæ which is apparently coincident with that of nitrogen. From various circumstances, chiefly connected with the alterations and adjustments of new instruments, I was not able to accomplish this task satisfactorily until within the last few months.

Description of Apparatus.

It seems to me desirable to give a description of the spectroscopic

* Phil. Trans. 1868, pp. 542, 543.

† Phil. Trans. 1865, p. 13.

apparatus with which the observations in this paper were made. In the former paper, to which I have already referred, I gave some reasons* to show that the ordinary method of comparison, by reflecting light into the spectroscope by means of a small prism placed before one half of the slit, is not satisfactory for very delicate observations unless certain precautions are taken. I then describe an arrangement for this purpose, which, with one or two modifications, is adopted in the collimator constructed for use with the Royal Society's telescope. I give the description from that paper† :—

"The following arrangement for admitting the light from the spark appeared to me to be free from the objections which have been referred to, and to be in all respects adapted to meet the requirements of the case. In place of the small prism, two pieces of silvered glass were securely fixed before the slit at an angle of 45°. In a direction at right angles to that of the slit, an opening of about $\frac{1}{10}$ inch was left between the pieces of glass for the passage of the pencils from the object-glass. By means of this arrangement the spectrum of a star is seen accompanied by two spectra of comparison, one appearing above and the other below it. As the reflecting surfaces are about 0·5 inch from the slit, and the rays from the spark are divergent, the light reflected from the pieces of glass will have encroached upon the pencils from the object-glass by the time they reach the slit, and the upper and lower spectra of comparison will appear to overlap to a small extent the spectrum formed by the light from the object-glass. This condition of things is of great assistance to the eye in forming a judgment as to the absolute coincidence or otherwise of lines. For the purpose of avoiding some inconveniences which would arise from glass of the ordinary thickness, pieces of the thin glass used for the covers of microscopic objects were carefully selected, and these were silvered by floating them upon the surface of a silvering solution. In order to ensure that the induction-spark should always preserve the same position relatively to the mirror, a piece of sheet gutta percha was fixed above the silvered glass; in the plate of gutta percha, at the proper place, a small hole was made of about $\frac{1}{20}$ inch in diameter. The ebonite clamp containing the electrodes is so fixed as to permit the point of separation of these to be adjusted exactly over the small hole in the gutta percha. The adjustment of the parts of the apparatus was made by closing the end of the adapting-tube, by which the apparatus is attached to the telescope, with a diaphragm with a small central hole, before which a spirit-lamp was placed. When the lines from the induction-spark, in the two spectra of comparison, were seen to overlap exactly, for a short distance, the lines of sodium from the light of the lamp, the adjustment was considered perfect. The accuracy of adjustment has been confirmed by the exact coincidence of the three lines of magnesium with the component lines of *b* in the spectrum of the moon."

The modifications of this plan consist in the substitution of a thin silver

* Phil. Trans. 1868, pp. 537, 538. † *Ibid.* 1868, p. 538.

plate polished on both surfaces for the pieces of silvered glass. The opposite side of the silver plate to that from which the terrestrial light is reflected to the slit reflects the images formed by the object-glass to the side of the tube where a suitable eyepiece is fixed. This arrangement forms a very convenient finder, for it is easy to cause the image of the star to disappear in the hole in the silver plate. When this is the case, the line of light formed by the star falls on the slit, and its spectrum is visible in the spectroscope. This collimator is so constructed that, by means of a coupling-screw, any one of three spectroscopes can be conveniently attached to it.

This apparatus performs admirably; but it seemed to me desirable, for observations of great delicacy, to be able to dispense with reflection, and to place the source of the light for comparison directly before the slit. Formerly I accomplished this object by placing the spark or vacuum-tube before the object-glass of the telescope. The great length of the present telescope renders this method inconvenient; but a more important objection arises from the great diminution of the light when the spark is removed to a distance of 15 feet from the slit. I therefore resolved to place the spark or vacuum-tube within the telescope at a moderate distance from the slit. For this purpose holes were drilled in the tube opposite to each other, at a distance of 2 feet 6 inches within the principal focus. Before these holes short tubes were fixed with screws; in these tubes slide suitable holders for carrying electrodes or vacuum-tubes. The spark is thus brought at once nearly into the axis of the telescope. The final adjustment is made in the following manner:—A bright star is brought into the centre of the field of an ordinary eyepiece; the eyepiece is then pushed within the focus, when the wires or vacuum-tube can be seen across the circle of light formed by the star out of focus. The place of discharge between the electrodes, or the middle of the capillary part of the vacuum-tube, is then brought into the centre of the circle of light. The vacuum-tubes are covered with black paper, with the exception of a space about a $\frac{1}{4}$ inch long in the middle of the capillary part; through this small uncovered space the light passes to reach the slit.

The accuracy of both methods of comparison, that by reflection and that by the spark within the tube, was tested by the comparison of the three bright lines of magnesium and the double line of sodium with the Fraunhofer lines *b* and D in the spectrum of the moon. I greatly prefer the latter method, because it is free from several delicate adjustments which are necessary when the light is reflected and which are liable to be accidentally displaced.

Spectroscope A is furnished with a single prism of dense glass with a refracting angle of 59° 42′, giving 5° 6′ from A to H.

Spectroscope B has two compound prisms of Mr. Grubb's construction, which move automatically to positions of minimum deviation for the different parts of the spectrum. Each prism gives about 9° 6′ for minimum deviation from A to H.

Spectroscope C is furnished with four similar prisms.

The small telescopes of the three spectroscopes are of the same size: diameter of object-glass 1¼ inch; each is furnished with three eyepieces magnifying 5·5, 9·2, and 16·0 diameters.

Spectrum of the Nebula of Orion.

With spectroscopes A and B four* lines are seen; they are represented in the diagram which accompanies this note. The scale in the diagram gives wave-lengths.

First line.—With spectroscope B and eyepiece 1 and 2, the slit being made very narrow, this line was seen to be very narrow, of a width corresponding to the slit, and defined at both edges, and undoubtedly not double. The line of nitrogen when compared with it appeared double, and each component nebulous and broader than the line of the nebula. This latter line was seen on several nights to be apparently coincident with the middle of the less refrangible line of the double line of nitrogen. This observation was on one night confirmed by observation with the more powerful spectroscope C.

The question suggests itself whether, under any conditions of pressure and temperature, the double line of the nitrogen-spectrum becomes single; and further, if this should be found to be the case, whether the line becomes single by the fading out of its more refrangible component, or in what other way the single line of the nebula comes to occupy the position in the spectrum, not of the middle of the double line of nitrogen, but that of the less refangible of the lines.

I stated in my former paper that when for any reason the light from the luminous nitrogen is greatly reduced in intensity, the double line under consideration is the last to disappear, and consequently a state of things may be found in which the light of nitrogen is sensibly monochromatic when examined with a narrow slit†. Under these circumstances the line of nitrogen appears narrower, and the separate components can be detected with difficulty, if at all.

I stated also that the breadth of the component lines appears to be connected with the conditions of density and temperature of the gas. As was to be expected from theoretical considerations, the lines become narrower and less nebulous as the pressure is diminished. My observations of this change seemed to show that the diminution of the breadth of the lines takes place chiefly at the outer sides of the lines; so that in the light from very rarefied gas the double line is narrower, but the space of separation

* The fourth line was first seen in nebula 18 H. IV. (Phil. Trans. 1864, p. 441).

† Phil. Trans. 1868, pp. 540–546. Observations on this point were subsequently made by Frankland and Lockyer (Proc. Roy. Soc. vol. xvii. p. 453). It should be stated that the authors make no reference to this observation, though they refer to a purely hypothetical suggestion contained in the same paper.

between the components is not as much wider as would be the case if the lines had decreased equally in width on the sides towards each other.

When the pressure of the gas is reduced to about 15 inches of mercury, the line-spectrum fades out to give place to Plücker's spectrum of the first order. During this process a state of things occurs when, for reasons already stated, the spectrum becomes *sensibly* monochromatic when viewed with a narrow slit and a spectroscope of several prisms. The line is narrower but remains double, and has the characters described in the preceding paragraph.

As the pressure is diminished, the double line fades out entirely, and the spectrum of the second order gives place to the spectrum of the first order. When, however, the pressure becomes exceedingly small, from 0·1 inch to 0·05 inch, or less, of mercury, there is a condition of the discharge in which the line again appears, while the other lines remain very faint. Under these conditions I have always been able, though with some difficulty, on account of the faint light when the necessary dispersive power (spectroscope B with second or third eyepiece) and a narrow slit are used, to see the line to be double, but it is narrower than when the gas is more dense, and may be easily mistaken for a single line. I have not yet been able to find a condition of luminous nitrogen in which the line has the same characters as those presented by the line in the nebula, where it is single and of the width of the slit.

Upon the whole I am still inclined to regard the line in the nebula as probably due to nitrogen.

If this should be found to be the case, and that the nebular line has originally the refrangibility of the middle of the double line of nitrogen, then we should have evidence that the nebula is moving from the earth. The amount of displacement of the nebular line from the middle of the double line of nitrogen corresponds to a velocity of 55 miles per second from the earth. At the time of observation the part of the earth's orbital motion, which was from the nebula, was 14·9 miles per second. From the remaining 40 miles per second would have to be deducted the probable motion from the nebula due to the motion of the solar system in space. This estimation of the possible motion of the nebula can be regarded as only approximate.

If the want of accordance of the line in the nebula with the middle of the double line of nitrogen be due to a recession of the nebula in the line of sight, there should be a corresponding displacement of the third line as compared with that of hydrogen. For reasons which will be found in a subsequent paragraph, I have not been able to make this comparison with the necessary accuracy.

In my former paper* I gave reasons against supposing so large a motion in the nebula; these were based on the circumstance that the nebular line falls upon the double nitrogen line, which the present observations

* Phil. Trans. 1868, pp. 542, 543.

confirm. I was not then able to use a slit sufficiently narrow to show that the nebular line is single and not coincident with the middle of the double line of nitrogen.

I am still pursuing the investigation of the parts of this inquiry which remain unsettled.

Second line.—This line was found by my former comparisons to be a little less refrangible than a strong line in the spectrum of barium. Three sets of measures give for this line a wave-length of 4957 on Ångström's scale; this would show that the line agrees nearly in position with a strong line of iron. At present I am not able to suggest to what substance this line belongs.

This line is also narrow and defined. I suspect that the brightness of this line relatively to the first line varies in different nebulæ.

Third and fourth line.—My former observations show that these lines agree in position with two lines of the spectrum of hydrogen, that at F and the line near G.

These lines are very narrow and are defined; the hydrogen therefore must be at a low tension.

The brightness of these lines relatively to the first and second lines varies considerably in different nebulæ; and I suspect they may also vary in the same nebulæ at different times, and even in different parts of same nebula; but at present I have not sufficient evidence on these points*. I regret that, in consequence of a continuance of bad weather, I have not yet been able to obtain decisive observations as to the possible

* Since writing this sentence, I have seen a note by Prof. D'Arrest in the 'Astronomische Nachrichten,' No. 1885. Speaking of the nebula H. IV. 37, he says:—"Sein Spectrum ist ausser von Huggins bisher nur noch von Dr. H. Vogel untersucht worden. In No. 1864, Ast. Nachr. theilt Letzterer mit, dass er im Jahre 1871, im Widerspruch mit Huggins' Angabe, die Linie Neb. (3)=(2), bisweilen sogar (2)<(3) gefunden habe. Nach Huggins war dagegen im Jahre 1864 positiv (2)>(3). Ist Vogel's Beobachtung, wie ich nicht bezweifle, zuverlässig, so wird seine Vermuthung einer Veränderung hier in der That begründet sein, denn diesen Winter, namentlich im Februar und März

motion of the nebula in the line of sight. With spectroscope B and eyepiece 2, the lines appear to be coincident with those of hydrogen. In consequence of the uncertainty of the character of the first line, which is single, while that of nitrogen is double, this determination can now only be made by means of the comparison of the third line with that of hydrogen. This third line becomes very faint from the great loss of light unavoidable in a spectroscope that gives a sufficient dispersive power, and the comparison can only be attempted when the sky is very clear and the nebula near the meridian.

(Received June 12, 1872.)

§ 2. *On the Motions of some Stars towards or from the Earth.*

In the early part of 1868 I had the honour of presenting to the Royal Society some observations on a small change of refrangibility which I had observed in a line in the spectrum of Sirius as compared with a line of hydrogen, from which it appeared that the star was moving from the earth with a velocity of about twenty-five miles per second, if the probable advance of the sun in space be taken into account *.

It is only within the last few months that I have found myself in possession of the necessary instrumental means to resume this inquiry, and since this time the prevalence of bad weather has left but few nights sufficiently fine for these delicate observations.

Some time was occupied in obtaining a perfectly trustworthy method of comparison of the spectra of stars with those of terrestrial substances, and it was not until I had arranged the spark within the tube, as described at the beginning of this note, that I felt confidence in the results of my observations.

It may be well to state some circumstances connected with these com-

1872, fand ich wiederum, so wie es Huggins früher gesehen hat, unzweifelhaft (2)>(3). Die relative Intensität der drei Lichtarten habe ich mehrfach in Zahlen geschätzt und erhielt z. B. in den letzten Nächten:

	März 6.	März 13.
(1)	100	100
(2)	58	63
(3)	49	52"

* Phil. Trans. 1868, pp. 529–550. As a curious instance in which later methods of observation have been partially anticipated, a reference may be made to an ingenious paper in the Philosophical Transactions for 1783, vol. lxxiv., by the Rev. John Michell, entitled "On the means of discovering the Distance, Magnitude, &c. of the Fixed Stars, in consequence of the Diminution of the Velocity of their Light." The author suggests that by the use of a prism "we might be able to discover diminutions in the velocity of light as perhaps a hundredth, a two hundredth, a five hundredth, or even a thousandth part of the whole." But he then goes on to reason on the production of this diminished velocity by the attraction produced on the material particles of light by the matter of the stars, and that the diminutions stated above would be "occasioned by spheres whose diameter should be to the sun, provided they were of the same density, in the several proportions of 70, 50, 30, and 22 to 1 respectively."

parisons which necessarily make the numerical estimations given further on less accurate than I could wish. Even when spectroscope C, containing four compound prisms, and a magnifying-power of 16 diameters are used, the amount of the change of refrangibility to be observed appears very small. The probable error of these estimations is therefore large, as a shift corresponding to five miles per second (about $\frac{1}{40}$ of the distance of D^1 to D^2), or even a somewhat greater velocity, could not be certainly observed. The difficulty arising from the apparent smallness of the change of refrangibility is greatly increased by some other circumstances. The star's light is faint when a narrow slit is used, and the lines, except on very fine nights, cannot be steadily seen, in consequence of the movements in our atmosphere. Further, when the slit is narrow, the clock's motion is not uniform enough to keep the spectrum steadily in view; for these reasons I found it necessary to adopt the method of estimation by comparing the shift with a wire of known thickness, or with the interval between a pair of close lines. I found that, under the circumstances, the use of a micrometer would have given the appearance only of greater accuracy. I wish it, therefore, to be understood that I regard the following estimations as provisional only, as I hope, by means of apparatus now being constructed, to be able to get more accurate determinations of the velocity of the motions.

Sirius.—The comparison of the line at F with the corresponding line of hydrogen was made on several nights from January 18 to March 5. Spectroscope C and eyepieces 2 and 3 were used. These observations confirm the conclusion arrived at in my former paper, that the star is moving from the earth; but they ascribe to the star a velocity smaller than that which I then obtained.

These observations on different days show a change of refrangibility corresponding to a velocity of from 26 miles to 36 miles per second. The part of the earth's orbital motion from the star varied on these days from 10 miles to 14 miles per second. We may take, therefore, 18 to 22 miles per second as due to the star.

The difference of this estimate, which is probably below rather than in excess of the true amount, from that which I formerly made may be due in part or entirely to the less perfect instruments then at my command. At the same time, if Sirius be moving in an elliptic orbit, as suggested by Dr. Peters, that part of the star's proper motion which is in the direction of the visual ray would constantly vary*.

* Dr. H. Vogel at Bothkamp seems to have repeated my observations on Sirius with the necessary care. He says (Astron. Nachr. No. 1864):—"Mit der eben beschriebenen Anordnung gelang es Herrn Dr. Lohse und mir am 22. März (1871) bei ganz vorzüglicher Luft die Nichtcoincidenz der drei Wasserstofflinien H α, H β, und H γ, der Geissler'schen Röhre mit den entsprechenden Linien des Siriusspectrum zu sehen...... mit Berücksichtigung der Geschwindigkeit der Erde zur Zeit der Beobachtung berechnet sich die Geschwindigkeit mit welcher sich Sirius von der Erde bewegt zu 10·0 Meilen in der Secunde, wogegen Procyon sich 13·8 Meilen in der Secunde von unserer Erde entfernen würde."

Betelgeux (*α Orionis*).—In the early observations of Dr. Miller and myself on this star, we found that there are no strong lines coincident with the hydrogen lines at C and F. The line H α falls on the less refrangible side of a small group of strong lines, and H β occurs in the space between two groups of strong lines where the lines are faint. On one night of unusual steadiness of the air, when the finer lines in the star's spectrum were seen with more than ordinary distinctness, I was able with the more powerful instruments now at my command to see a narrow defined line in the red apparently coincident with H α, and a similar line at the position of H β. These lines are much less intense than the lines C and F in the solar spectrum; there are certainly no bright lines in the star's spectrum at these places.

The most suitable lines in this star for comparison with terrestrial substances for ascertaining the star's motion are the lines of sodium and of magnesium. The double character of the one line agreeing exactly with that of sodium, and the further circumstance that the more refrangible of the lines is the stronger one, as is the case in spectrum of sodium and in the solar spectrum, and the relative distances from each other and comparative brightness of the three lines, which correspond precisely to the triple group of magnesium, can allow of no doubt that these lines in the star are really produced by the vapours of these substances existing there, and that we may therefore safely take any small displacement of either set of lines to show a motion of the star towards or from the earth. The lines due to sodium are perhaps more intense, but are as narrow and defined as the lines D_1, D_2 in the solar spectrum: they fall, however, within a group of very fine lines; this circumstance may possibly account for the nebulous character which has been assigned to them by some observers.

The bright lines of sodium were compared with spectroscope B and eyepiece 3; they appeared to fall very slightly above the pair in the star, showing that the stellar lines had been degraded by the star's motion from the earth. The amount of displacement was estimated at about one fifth of the distance of D_1 from D_2, which is probably rather smaller than the true amount. This estimation would give a velocity of separation of 37 miles per second. At the time of observation the earth was moving from the star at about 15 miles per second, leaving 29 miles to be due to the star.

When magnesium was compared, a shift in the same direction, and corresponding in extent to about the same velocity of recession, was observed; but, in consequence of other lines in the star at this place, the former estimation, based on the displacement of the lines of sodium, was considered to be more satisfactory.

Rigel.—The lines of hydrogen are strong in the spectrum of this star, and are suitable for comparison.

The line H β is not so broad as it appears in the spectrum of Sirius, but is stronger than F in the solar spectrum: this line was compared by

means of spectroscope C and eyepieces 2 and 3. The line of terrestrial hydrogen falls above the middle of the line in the star; the star is therefore receding from the earth. The velocity of recession may be estimated as rather smaller than Sirius, probably about 30 miles per second, the earth at the time of observation moving from the star with a velocity of 15 miles, leaving about 15 miles as due to the star. This estimate is probably rather smaller than the true velocity of the star.

Castor.—The spectra of the two component stars of this double star blend in the spectroscope into one spectrum. The line H β is rather broad, nearly as much so as the same line in the spectrum of Sirius.

The narrow line of rarefied hydrogen was compared in spectroscope B with eyepiece 3; it appeared to fall on the more refrangible side of the middle of the line in the star, leaving more of the dark line on the side towards the red. The shift seemed to be rather greater than that in Sirius, and may probably be taken at from 40 to 45 miles per second; but the earth's orbital motion was nearly 17 miles from the star, thu sleaving about 25 miles for the apparent velocity of the star. This result rests at present on observations on one night only, but they seemed at the time to be satisfactory.

Regulus.—The line at F rather broad. The corresponding line of hydrogen falls on the more refrangible side of the middle of the dark line in the star. The air was unfavourable on all the evenings of comparison; a rough estimate gives a velocity of from 30 to 35 per second. The earth's motion was 18 miles, leaving from 12 to 17 miles for the velocity of recession between the star and the sun.

β and δ Leonis.—These stars were compared with hydrogen; they appear to be moving from the earth, but the want of steadiness in the air prevented me from making a satisfactory estimate of their velocity. I suspected their motion to be rather smaller than that of Regulus.

β, γ, δ, ϵ, ζ Ursæ majoris.—All these stars have similar spectra, in which the line F is strong, though there are small differences in the breadth of the line. They were compared with hydrogen, and appear to be moving from our system with about the same velocity. Probably their motion may be taken to be not far from 30 miles per second. The earth's motion at the time of observation was from 9 miles to 13 miles from these stars, leaving a probable velocity of recession of 17 to 29 miles per second. In the case of the double star ζ, the spectrum consisted of the light of both stars.

η Ursæ majoris was also compared with hydrogen. I believe it shows a motion from the earth, but the observations of this star are at present less satisfactory.

α Virginis and α Coronæ borealis.—These stars were compared with hydrogen. I suspect that they are receding, but I have not had nights sufficiently fine to enable me to make satisfactory observations of these stars.

In addition to these stars some observations (which are less satisfactory

on account of the unfavourable state of the weather at the time) appear to show that the stars Procyon, Capella, and possibly Aldebaran, are moving from the earth.

The stars which follow have a motion of approach.

Arcturus.—In the spectrum of this star the lines of hydrogen, of magnesium, and of sodium are sufficiently distinct for comparison. I found the comparison could be most satisfactorily made with magnesium.

The bright lines of magnesium fall on the less refrangible side of the corresponding dark lines in the star's spectrum, showing that the star is approaching the earth. I estimated the shift at about $\frac{1}{5}$ to $\frac{1}{4}$ of the interval between Mg_2 and Mg_3; this amount of displacement would indicate a velocity of approach of 50 miles per second. To this velocity must be added the earth's orbital motion from the star of 5·25 miles per second, increasing the star's motion to 55 miles per second.

When I can get favourable weather, I hope to obtain independent estimations from the lines of sodium and of hydrogen.

α Lyræ.—In the spectrum of Vega the line corresponding to H β is strong and broad. Comparisons were made on several nights, but on one evening only was the air favourable. The observations are accordant in showing that the narrow bright line from a Geissler's tube falls on the less refrangible side of the middle of the line in the star, thus leaving more of the line on the side towards the violet. The estimations give a motion of approach between the earth and the star of from 40 to 50 miles per second, to which must be added 3·9 miles for the earth's motion from the star.

α Cygni.—The hydrogen line at F in the spectrum of this star is narrower than in the spectrum of Sirius and of α Lyræ, though probably rather broader than the same line in the solar spectrum. I have at present observations made on two evenings only, on both of which the state of the air was unfavourable for the comparison of this line with that of terrestrial hydrogen. They give to the star a motion of approach of about 30 miles per second, which would have to be increased by 9 miles, the velocity at the time of the earth from the star.

Pollux.—The lines of magnesium and those of sodium are very distinct in the spectrum of this star. As the air was not very steady at the time of my observations, I found it more satisfactory to use for comparison the lines of magnesium, which are rather stronger than those of sodium. The three lines of magnesium appeared to be less refrangible than the corresponding dark lines in the spectrum of the star by about one sixth of the interval from Mg_2 to Mg_3. This estimation would represent a velocity of approach equal to about 32 miles per second. The earth's motion from the star was 17·5 miles, which increases the apparent velocity of approach to 49 miles per second. On one evening only was the air favourable enough for a numerical estimate, but the observations were entered in my observatory-book as satisfactory.

α Ursæ majoris.—The spectrum of this star is different from the spectra of the other bright stars of this constellation. The line at F is not so strong, while the lines at *b* are more distinct, and are sufficiently strong for comparison with the bright lines of magnesium. The bright lines of this metal fall on the less refrangible side of the dark lines, and show a motion of approach of from 35 to 50 miles per second. The earth's motion of 11·8 miles from the star must be added.

γ Leonis and ε Boötis.—In both these double stars the compound spectrum due to the light of both component stars was observed. Both stars are most conveniently compared with magnesium. I do not consider my observations of these stars as quite satisfactory, but they seem to show a movement of approach; but further observations are desirable.

The stars *γ Cygni*, *α Pegasi*, *γ Pegasi*, and *α Andromedæ* were compared with hydrogen on one night only. It is probable that these stars are approaching the earth, but I wish to reobserve them before any numerical estimate is given of their motion.

γ Cassiopeiæ.—On two nights I compared the bright lines which are present in its spectrum at C and F with the bright lines of terrestrial hydrogen. The coincidence appeared nearly perfect in spectroscope C with eyepieces 2 and 3; but on the night of best definition I suspected a minute displacement of the bright line towards the red when compared with H β. As the earth's orbital motion from the star at the time was very small, about 3·25 miles per second, which corresponds to a shift that could not be detected in the spectroscope, it seems probable that γ Cassiopeiæ has a small motion of recession.

In the calculation of the estimated velocities the wave-lengths employed are those given by Ångström in his 'Recherches sur le spectre solaire' (Upsal, 1868). The velocity of light was taken at 185,000 miles per second.

The velocities of approach and of recession which have been assigned to the stars in this paper represent the whole of the motion in the line of sight which exists between them and the sun. As we know that the sun is moving in space, a certain part of these observed velocities must be due to the solar motion. I have not attempted to make this correction, because, though the direction of the sun's motion seems to be satisfactorily ascertained, any estimate that can be made at present of the actual velocity with which he is advancing must rest upon suppositions, more or less arbitrary, of the average distance of stars of different magnitudes. It seems not improbable that this part of the stars' motions may be larger than would result from Otto Struve's calculations, which give, on the supposition that the average parallax of a star of the first magnitude is equal to 0″·209, a velocity but little greater than one fourth of the earth's annual motion in its orbit.

It will be observed that, speaking generally, the stars which the spectro-

scope shows to be moving from the earth (Sirius, Betelgeux, Rigel, Procyon) are situated in a part of the heavens opposite to Hercules, towards which the sun is advancing, while the stars in the neighbourhood of this region, as Arcturus, Vega, α Cygni, show a motion of approach. There are in the stars already observed exceptions to this general statement; and there are some other considerations which appear to show that the sun's motion in space is not the only or even, in all cases, as it may be found, the chief cause of the observed proper motions of the stars*.

There can be little doubt but that in the observed stellar movements we have to do with two other independent motions—namely, a movement common to certain groups of stars, and also a motion peculiar to each star.

Mr. Proctor has brought to light strong evidence in favour of the drift of stars in groups having a community of motion by his graphical investigation of the proper motions of all the stars in the catalogues of Mr. Main and Mr. Stone†. The probability of the stars being collected into systems was early suggested by Michell and the elder Herschel‡. One of the most remarkable instances pointed out by Mr. Proctor are the stars β, γ, δ, ϵ, ζ of the Great Bear, which have a community of proper motions§, while α and η of the same constellation have a proper motion in the opposite direction. Now, the spectroscopic observations show that the stars β,

* As the velocities assigned to the stars are, for reasons already stated, provisional only, I feel some hesitation in drawing from them the obvious conclusions which they would suggest. The velocities given in the Tables for those stars which are moving in direction in accordance with the sun's motion towards Hercules do not bear to each other the relation which they should have if they were mainly produced by the sun's motion. Even for these stars, therefore, we must look elsewhere for the cause to which they are chiefly due.

† See "Preliminary Paper on certain Drifting Motions of the Stars," Proc. Roy. Soc. vol. xviii. p. 169.

‡ Sir William Herschel writes:—"Mr. Michell's admirable idea of the stars being collected into systems appears to be extremely well founded, and is every day more confirmed by observations, though this does not take away the probability of many stars being still as it were solitary, or, if I may use the expression, intersystematical ...... A star, or sun such as ours, may have a proper motion within its own system of stars, while at the same time the whole starry system to which it belongs may have another proper motion totally different in quantity and direction." Herschel further says, "And should there be found in any particular part of the heavens a concurrence of proper motions of quite a different direction, we shall then begin to form some conjectures which stars may possibly belong to ours, and which to other systems."—Phil. Trans. 1783, pp. 276, 277.

§ Mr. Proctor, speaking of these stars, says:—"Their drift is, I think, most significant. If, in truth, the parallelism and equality of motion are to be regarded as accidental, the coincidence is one of most remarkable character. But such an interpretation can hardly be looked upon as admissible when we remember that the peculiarity is only one of a series of instances, some of which are scarcely less striking."—Other Worlds than Ours, p. 269. See paper in Proc. Roy. Soc. vol. xviii. p. 170.

γ, δ, ϵ, ζ have also a common motion of recession, while the star α is approaching the earth. The star η, indeed, appears to be moving from us, but it is too far from α to be regarded as a companion to that star.

Although it was not to be expected that a concurrence would always be found between the proper motions which indicate the apparent motions at right angles to the line of sight and the radial motions as discovered by the spectroscope, still it is interesting to remark that in the case of the stars Castor and Pollux, one of which is approaching and the other receding, their proper motions also are different in direction and in amount; and further, that γ Leonis, which has an opposite radial motion to α and β of the same constellation, differs from these stars in the direction of its proper motion.

It scarcely needs remark that the difference in breadth of the line H β in different stars affords us information of the difference of density of the gas by which the lines of absorption are produced. A discussion of the observations in reference to this point, and other considerations on the physical condition of the stars and nebulæ, I prefer to reserve for the present.

TABLE I.—Stars moving from Sun.

Star.	Compared with	Apparent motion.	Earth's motion.	Motion from sun.
Sirius	H	26 to 36	−10 to 14	18 to 22
Betelgeux	Na	37	−15	22
Rigel	H	30	−15	15
Castor	H	40 to 45	−17	23 to 28
Regulus	H	30 to 35	−18	12 to 17
β Ursæ majoris	H			
γ " "	H			
δ " "	H	30	− 9 to 13	17 to 21
ϵ " "	H			
ζ " "	H			
β Leonis	H			
δ Leonis	H			
η Ursæ majoris	H			
α Virginis	H			
α Coronæ borealis	H			
Procyon	H			
Capella	H			
Aldebaran ?	Mg			
γ Cassiopeiæ	H			

TABLE II.—Stars approaching the Sun.

Star.	Compared with	Apparent motion.	Earth's motion.	Motion towards sun.
Arcturus	Mg	50	+ 5	55
Vega	H	40 to 50	+ 3·9	44 to 54
α Cygni.......................	H	30	+ 9	39
Pollux	Mg	32	+17	49
α Ursæ majoris................	Mg	35 to 50	+11	46 to 60
γ Leonis	Mg			
ε Boötis......................	Mg			
γ Cygni.......................	H			
α Pegasi......................	H			
γ Pegasi?	H			
α Andromedæ	H			

II. "On Blood-relationship." By FRANCIS GALTON, F.R.S.

Received May 7, 1872.

I propose in this memoir to deduce, by fair reasoning from acknowledged facts, a more definite notion than now exists of the meaning of the word "kinship." It is my aim to analyze and describe the complicated connexion that binds an individual, hereditarily, to his parents and to his brothers and sisters, and, therefore, by an extension of similar links, to his more distant kinsfolk. I hope by these means to set forth the doctrines of heredity in a more orderly and explicit manner than is otherwise practicable.

From the well-known circumstance that an individual may transmit to his descendants ancestral qualities which he does not himself possess, we are assured that they could not have been altogether destroyed in him, but must have maintained their existence in a latent form. Therefore each individual may properly be conceived as consisting of two parts, one of which is latent and only known to us by its effects on his posterity, while the other is patent, and constitutes the person manifest to our senses.

The adjacent and, in a broad sense, separate lines of growth in which the patent and latent elements are situated, diverge from a common group and converge to a common contribution, because they were both evolved out of elements contained in a structureless ovum, and they, jointly, contribute the elements which form the structureless ova of their offspring.

The annexed diagram illustrates my meaning, and serves to show clearly that the span of each of the links in the general chain of heredity extends from one structureless stage to another, and not from person to person :—

Structureless elements in Father	.........Adult Father ....Latent in Father.........	Structureless elements in offspring.

I will now proceed to consider the quality of the several relationships by which the above terms are connected together.

The observed facts of Reversion enable us to prove that the latent elements must be greatly more varied than those that are personal or patent. The arguments are as follows:—(1) there must be *room* for very great variety, because a single strain of impure blood will reassert itself after more than eight generations; (2) an individual has 256 progenitors in the eighth degree, if there have been no ancestral intermarriages, while under the ordinary conditions of social and neighbourly life he will certainly have had a considerable, though a smaller number of them; (3) the gradual waning of the tendency to reversion as the generations increase conforms to what would occur if each fresh marriage contributed a competing element for the same place, thus diluting the impure strain until its relative importance was reduced to an insignificant amount. It follows from these arguments that for each place among the personal elements there may exist, and probably often does exist, a great variety of latent elements that formerly competed to fill it.

I have spoken of the primary elements as they exist in the newly impregnated ovum, where they are structureless but contain the materials out of which structure is evolved; the embryonic elements are segregated from among them. On what principle are they segregated? Since for each place there have been many unsuccessful but qualified competitors, it must have been on some principle whose effects may be described as those of "*Class Representation,*" using that phrase in a perfectly general sense as indicating a mere fact, and avoiding any hypothesis or affirmation on points of detail, about most, if not all, of which we are profoundly ignorant. I give as broad a meaning to the expression as a politician would give to the kindred one, a "representative assembly." By this he means to say that the assembly consists of representatives from various constituencies, which is a distinct piece of information so far as it goes, and is a useful one, although it deals with no matter of detail; it says nothing about the number of electors, their qualifications, or the motives by which they are influenced; it gives no information as to the number of seats; it does not tell us how many candidates there are usually for each seat, nor whether the same person is eligible for, or may represent at the same time, more than one place, nor whether the result of the elections at one place may or may not influence those at another (on the principle of correlation). After these explanations there can, I trust, be no difficulty in accepting my definition of the general character of the relation between the embryonic and the structureless elements, that the former are the result of election from the latter on some method of Class Representation.

The embryonic elements are *developed* into the adult person. "Development" is a word whose meaning is quite as distinct in respect to form, and as vague in respect to detail, as the phrase we have just been con-

sidering; it embraces the combined effects of growth and multiplication, as well as those of modification in quality and proportion, under both internal and external influences. If we were able to obtain an approximate knowledge of the original elements, statistical experiences would no doubt enable us to predict the average value of the form into which they would become developed, just as a knowledge of the seeds that were sown would enable us to predict in a general way the appearance of the garden when the plants had grown up; but the individual variation of each case would of course be great, owing to the large number of variable influences concerned in the process of development.

The latent elements in the embryonic stage must be developed by a parallel, I do not say by an identical process, into those of the adult stage. Therefore, to avoid all chance of being misapprehended when I collate them, I will call, in the diagram I am about to give (see fig. 1, p. 398), the one process "Development *a*" and the other "Development *b*."

It is not intended to affirm, in making these subdivisions, that the embryonic and adult stages are distinctly separated; they are continuous, and it is impossible but that they should overlap, some elements remaining embryonic while others are completely formed. Nevertheless the two, speaking broadly, may fairly be looked upon as consecutive.

Again, the two processes are not wholly distinct; on the contrary, the embryo, and even the adult in some degree, must receive supplementary contributions derived from their contemporary latent elements, because ancestral qualities indicated in early life frequently disappear and yield place to others. The reverse process is doubtful; it may exist in the embryonic stage, but it certainly does not exist in a sensible degree in the adult stage, else the later children of a union would resemble their parents more nearly than the earlier ones.

Lastly, I must guard myself against the objection that though structure is largely correlated, I have treated it too much as consisting of separate elements. To this I answer, first, that in describing how the embryonic are derived from the structureless elements, I expressly left room for a small degree of correlation; secondly, that in the development of the adult elements from the embryonic there is a perfectly open field for natural selection, which is the agency by which correlation is mainly established; and, thirdly, that correlation affects groups of elements rather than the complete person, as is proved by the frequent occurrence of small groups of persistent peculiarities, which do not affect the rest of the organism, so far as we know, in any way whatever.

The ground we have already gained may be described as follows:—

Out of the structureless ovum the embryonic elements are taken by *Class Representation*, and these are *developed* (*a*) into the visible adult individual; on the other hand, returning to our starting-point at the structureless ovum, we find, after the embryonic elements have been segre-

gated, the large *Residue* is *developed* (*b*) into the latent elements contained in the adult individual. All this is summarily expressed in the first two columns of the diagram (fig. 1). I might have inserted vertical arrows to show the minor connexions between the corresponding stages in the two parallel processes, but it would have complicated the figure.

In what way do the patent and latent adult elements respectively contribute representatives towards the structureless stage of the next generation? We know that every quality they possess *may* be transmitted to it, but it does not follow that they *are* invariably transmitted. The contributions from the patent elements cannot be by "Class," because their own original elements have been themselves specialized, and therefore can contain no more than one or a few members of each class (which, it is true, must have been somewhat developed, both in numbers and variety, into what we may call "families"). Their contributions may therefore be justly described as being effected on some principle that has resulted in a "*Family representation,*" though whether in the representation of every family I do not profess to say.

As regards the large variety of adult latent elements, they cannot all be transmitted, for the following obvious reason—the corresponding qualities of no two parents can be considered exactly alike; therefore the accumulation of subvarieties, if they were all preserved as the generations rolled onwards, would exceed in multitude the wildest flights of rational theory. The heritage of peculiarities through the contributions of 1000 consecutive generations, even supposing a great deal of ancestral intermarriage, must far exceed what could be packed into a single ovum. The contributions from the latent adult elements are therefore no more than *Representative*; but they have to furnish all the various members of each Class whence its representatives have afterwards to be drawn. Therefore, bearing in mind what has been just argued, that it is impossible for the elements of every individual quality to be contributed, we are driven to suppose, as in the previous case, a "*Family representation,*" the similar elements contributed by the two parents ranking, of course, as of the same family. It is most important to bear in mind that this phrase states a fact and not an hypothesis; it does not mean that each and every Family has just one representative, for it is absolutely reticent on all such matters of detail as those I enumerated when speaking of Class Representation. To show the importance which I attach to this disclaimer, I may be permitted to mention what appears to me the most probable *modus operandi*, namely, that it is in reality a large selection made out of larger and not out of smaller constituencies than those I have called "classes," similar to that which would be obtained by an indiscriminate conscription: thus, if a large army be drawn from the provinces of a country by a general conscription, its constitution, according to the laws of chance, will reflect with surprising precision the qualities of the population whence it was taken; each village will be found

to furnish a contingent, and the composition of the army will be sensibly the same as if it had been due to a system of immediate representation from the several villages.

The diagram (fig. 1) expresses the whole of the foregoing results; it begins with the structureless elements whence the parent individual was formed, and ends with his contributions to the structureless elements whence his offspring is formed.

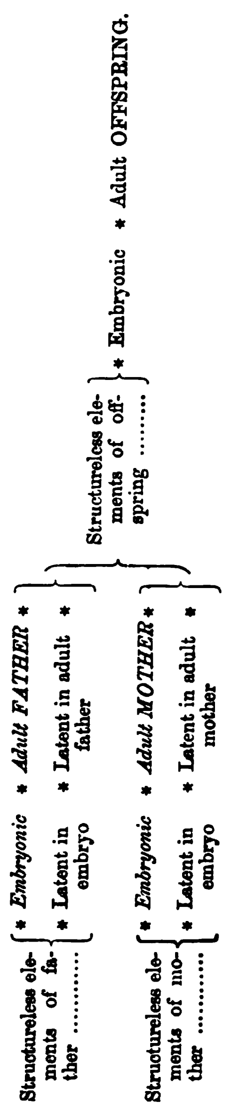
Fig. 1.

Structureless elements of parent ..	through "Class Representation" afford	Embryonic elements	which by "Development *a*" become	Adult elements	which by "Family Representation" contribute to ...	Structureless elements of offspring.
	"Residue" after the above have been abstracted, become	Latent elements in embryo ..	which by "Development *b*" become	Latent elements in adult	which by "Family Representation" contribute to ...	

I will now inquire what are, roughly speaking, the relative proportions of the contributions to the elements of the offspring made respectively by the patent and latent elements of the adult parent. It is better not to complicate the inquiry by speaking, at first, of these elements in their entirety, but rather of some special characteristic: thus, to fix the ideas, suppose we are speaking about a peculiar skin-mark in an animal; the peculiarity in question may be conceived (1) as purely personal, without the concurrence of any latent equivalents, (2) as personal but conjoined with latent equivalents, and (3) as existent wholly in a latent form. It can be shown that, in the first case, the power of hereditary transmission is exceedingly feeble; for, notwithstanding some exceptions (as in the lost power of flight in domestic birds), the effects of the use and disuse of limbs, and those of habit, are transmitted to posterity in only a very slight degree. Again, it can be fairly argued that many instances which seem at first sight to fall under case (1), that is, to be purely personal, and to prove a larger hereditary influence than what I assign to it, do really belong to case (2): thus, when individuals born with a peculiar mark are reputed to be the first of their race in whom it had ever appeared, it would be hazardous in the extreme to argue that the latent elements of that mark were wholly deficient in them. It is very remarkable (I was indebted for a knowledge of this fact to Mr. Tegetmeier) how nearly every bar or spot found in any species of an animal in its wild state may be bred into existence in the domesticated variety of that species, showing that the elements of all these bars and spots are universally present in all varieties of the species, though their manifestation may be overborne and suppressed. We therefore see that the hereditary influences of an

animal with respect to any particular spot are, I will not say in every case, but certainly on the *average of many cases*, much more numerous than if that spot had been purely a personal characteristic, without the concurrence of any latent elements. Bearing this argument in mind, we shall more justly estimate the import of the statistical evidence to be obtained from breeders of animals. I should judge, from the impression left by many scattered statistics, that it is perfectly safe to affirm that breeders, when they mate two animals, each having the same unusual characteristic, not through known hereditary transmission, but by supposed variation, would consider themselves fortunate if one quarter of the progeny inherited that quality. Now these successful cases are, as I have shown, on the average, the produce of parents having the peculiarity not only in a personal but also, to some degree, in a latent form. We may therefore reasonably conclude that, had the latter portion been non-existent, the ratio of successful cases would have been materially diminished.

Fig. 2.



I should demur, on precisely the same grounds, to objections based on the fact of the transmission of qualities to grandchildren being more frequent through children who possess those qualities than through children who do not; for I maintain that the personal manifestation is, on the average, though it need not be so in every case, a certain proof of the existence of some latent elements.

Having proved how small is the power of hereditary transmission of the personal elements, we can easily show how large is the transmission of the purely latent elements, in the case (3), by appealing to the well-known facts of Reversion; but into these it is hardly necessary for me to enter at length. The general and safe conclusion is, that the contribution from the patent elements is very much less than from the latent ones.

If we now combine our results into a diagram (fig. 2), showing the fainter streams of heredity by *italic lines*, and indicating those processes by asterisks (*) which were described at length in the previous figure, we shall easily recognize the complexity of hereditary

problems. We see that parents are very indirectly and only partially related to their own children, and that there are two lines of connexion between them, the one of large and the other of small relative importance. The former is a collateral kinship and very distant, the parent being descended through two stages (two asterisks) from a structureless source, and the child (so far as that parent is concerned) through five totally distinct stages from the same source; the other, but unimportant line of connexion, is direct and connects the child with the parent through two stages. We shall therefore wonder that, notwithstanding the fact of an average resemblance between parent and child, the amount of individual variation should not be much greater than it is, until we have realized how complete must be the harmony between every variety and its environments in order that the variety should be permanent.

We also infer from the diagram how much nearer, and yet how subject to variation, is the kinship between the children of the same parents; for only two stages are required to trace back their descent to a common origin, which, however, proceeds from four separate streams of heredity, namely the adult patent and latent elements of each of the two parents.

An approximate notion of the nearest conceivable relationship between a parent and his child may be gained by supposing an urn containing a great number of balls, marked in various ways, and a handful to be drawn out of them at random as a sample: this sample would represent the person of a parent. Let us next suppose the sample to be examined, and a few handfuls of new balls to be marked according to the patterns of those found in the sample, and to be thrown along with them back into the urn. Now let the contents of another urn, representing the influences of the other parent, be mixed with those of the first. Lastly, suppose a second sample to be drawn out of the combined contents of the two urns, to represent the offspring. There can be no nearer connexion justly conceived to subsist between the parent and child than between the two samples; on the contrary, my diagram shows the relationship to be in reality much more remote, and consisting of many consecutive stages, and therefore hardly to be expressed by such simple chances. Whenever the balls in the urns are much of the same pattern, the samples will be alike, but not otherwise. The offspring of a mongrel stock necessarily deviate in appearance from each other and from their parents.

We cannot now fail to be impressed with the fallacy of reckoning inheritance in the usual way, from parents to offspring, using those words in their popular sense of visible personalities. The span of the true hereditary link connects, as I have already insisted upon, not the parent with the offspring, but the primary elements of the two, such as they existed in the newly impregnated ova, whence they were respectively developed. No valid excuse can be offered for not attending to this fact, on the ground of our ignorance of the variety and proportionate values of the primary

elements: we do not mend matters in the least, but we gratuitously add confusion to our ignorance, by dealing with hereditary facts on the plan of ordinary pedigrees—namely, from the *persons* of the parents to those of their offspring.

It will be observed that, owing to the clearer idea we have now obtained of the meaning of kinship and of the consecutive phases of the chain of life, the various causes of individual variation can be easily and surely sorted into their proper places. I will mention a few of them, merely as examples.

Previous to the segregation of the embryonic elements, if the structureless ones be diverse without any strongly preponderating element, it is impossible to foresee the character of the embryo, just as it is impossible to foresee the character of a handful chosen from an urn containing a mixed assemblage of variously coloured balls; but if they be not diverse, then the embryonic elements will be a true sample of the structureless ones, the conditions of purity of blood are fulfilled, and the offspring will resemble its parents.

We also see, in the process by which the embryonic elements are obtained, how the curious phenomenon may occur of inheritance occasionally skipping alternate generations. The more that has been removed from the structureless group for the supply of the embryonic (which, as we have seen, is a nearly sterile destination) the less remains for the "residue," too little, it may be, to assert itself by that, the only prolific, line of transmission. In the supposed case it would recuperate itself during the succeeding generation, where the elements in question will have remained wholly latent, owing to their insignificance in the structureless stage of that generation, which would be sufficient to secure any portion of it from selection for the embryonic form.

Again, it is in the process of selection of elements, both latent and patent, from the adult parents for the structureless stage of the next generation, where I suppose the curious and unknown conditions usually to occur through which a change in the habits of life, after the adult age has been reached, is apt to produce sterility. I may be permitted to remark, hypothetically, that this view appears to be corroborated by the fact that many grains of pollen or many spermatozoa are required to fertilize each ovum, because, as it would seem, each separate one does not contain a sufficiently complete representation of the primary elements to supply the needs of an individual life, and that it is only by the accumulation of several separate consignments (so to speak) of the representative elements that the necessary variety is ensured. I argue from this that there is a tendency to a large individual variation in the constituents of each grain of pollen, or spermatozoon, and, by analogy, that there is a similar though smaller tendency in each ovum; also that changes in the habits of life may increase this variation to a degree that involves sterility.

Lastly, it is often remarked (1) that the immediate offspring of different races or even varieties resemble their parents equally, but (2) that great diversities appear in the next and in succeeding generations. In which stage does the variability occur? It cannot be in the first (class representation) nor in the second (development), else (1) could not have been true; therefore it must be in the third stage. A white parent necessarily contributes white elements to the structureless stage of his offspring, and a black, black; but it does not in the least follow that the contributions from a true mulatto must be truly mulatto.

One result of this investigation is to show very clearly that large variation in individuals from their parents is not incompatible with the strict doctrine of heredity, but is a consequence of it wherever the breed is impure. I am desirous of applying these considerations to the intellectual and moral gifts of the human race, which is more mongrelized than that of any other domesticated animal. It has been thought by some that the fact of children frequently showing marked individual variation in ability from that of their parents is a proof that intellectual and moral gifts are not strictly transmitted by inheritance. My arguments lead to exactly the opposite result. I show that their great individual variation is a necessity under present conditions; and I maintain that results derived from large averages are all that can be required, and all we could expect to obtain, to prove that intellectual and moral gifts are as strictly matters of inheritance as any purely physical qualities.

III. "Further Experiments on the Effect of Alcohol and Exercise on the Elimination of Nitrogen and on the Pulse and Temperature of the Body." By E. A. Parkes, M.D., F.R.S. Received April 25, 1872.

In the 'Proceedings of the Royal Society' (xviii. p. 362, xix. p. 73) are some observations by the late Count Wollowicz and myself on the effect of alcohol, brandy, and claret on the elimination of nitrogen. As the experiments were on one man, I have taken an opportunity of repeating them on another person; and as the late observations of Dr. Austin Flint (junior) on a man who walked 317 miles in five days have appeared to some persons to run counter to the now generally accepted view that exercise produces either no change or only insignificant changes in the urea, I have combined experiments on exercise with those on alcohol. With respect, however, to Dr. Austin Flint's experiments, it would appear that while the egress of nitrogen was determined with the greatest accuracy, the amount taken in was for the most part merely estimated by reference to Payen's Tables, and therefore there is no certainty that the ingress was what it is assumed to have been. The food also was very

varied, so that the difficulty of properly estimating the nitrogen was still more increased.

The following experiments were made on a soldier, W. D., aged 30. He is a powerfully built man, 5 feet 6 inches in height, and measuring 40 inches round the chest. As a young man, he had been employed in a distillery near Glasgow, and at that time drank largely of whisky, sometimes taking half a pint before breakfast. For the last ten years, since he has been in the army, he has been very temperate, taking chiefly beer in moderate quantities, and only occasionally spirits. He bears the character of a very steady soldier, and has always had perfect health, with the exception of an attack of "spotted typhus" six years ago. He has never served abroad.

As he is a Scotchman and had been brought up on oatmeal and milk, I placed him on a diet entirely composed of these two substances; and after a preliminary trial to see how much he required, he received every day 28 ounces of Scotch oatmeal and two pints of milk, the whole of which he took at regular hours. The oatmeal was all purchased at the same time and was well mixed, so that he received daily precisely the same amount of nitrogen. It would be impossible to keep the ingress of nitrogen more uniform than was done in this case. The milk was very good in quality; but to ensure, as far as possible, that it should be of equal nutritive value every day, water was added until its specific gravity, which was usually 1·032, was reduced to 1·028. He drank only water except on the days when brandy was added, and, with the exception of salt, took no other solid food for sixteen days except the oatmeal and milk. The daily amount of water (including that in the milk) was 135 fluid ounces; but some was lost in cooking the oatmeal. He was perfectly well and vigorous on this food, and his weight remained unchanged.

The oatmeal, burnt with soda-lime, was found to contain 2·023 per cent. of nitrogen; and the milk contained from ·35 to ·37 per cent. of nitrogen, and is taken at a mean of ·36. In the milk and the oatmeal together he received daily 20 grammes, or 308·6 grains, of nitrogen. The brandy contained 45 volumes per cent. of alcohol.

The course of experiment was as follows:—

For six days he remained quiet, taking only slow walking exercise to keep him in health; for three days he then worked hard at digging ground from eight to nine hours daily. It was intended that he should march thirty miles daily in heavy marching order; but after marching for eight miles he became footsore, and I was obliged to change his work to digging. He worked as hard as he could and felt fatigued in all his muscles, but it was impossible to calculate the exact amount of work: as far as could be done, he made it as uniform as he could from day to day.

After three days' exercise he was kept at rest for three days, and then resumed exercise of the same kind for three days, taking, however, during

the second period 12 fluid ounces of brandy (containing 5·4 fluid ounces of absolute alcohol) daily in three doses of 4 ounces each at 10, 2, and 6 o'clock.

After this he rested again, but being required for other duty there were only two days' observations during this last period.

1. *Weight of Body in kilogrammes.*

The weight was taken at 8 A.M., before breakfast and after the bladder was emptied. The machine was a new German balance, extremely delicate and turning distinctly with 1 gramme (15·43 grains).

First period. Comparative rest.		Second period. Exercise on water.		Third period. Rest.		Fourth period. Exercise on brandy.		Fifth period. Rest.	
Date.	Weight.	Date.	Weight.	Date.	Weight.	Date.	Weight.	Date.	Weight.
March 12.	66·420	March 20.	66·980	March 23.	66·54	March 26.	66·380	March 29.	66·19
13.	67·101	21.	66·420	24.	66·31	27.	66·360	30.	65·49
14.	66·631	22.	66·260	25.	66·15	28.	66·300		
15.	67·021								
16.	67·091								
17.	66·300								
18.	66·600								
19.	66·880								
Mean	66·68	......	66·553	......	66·333	......	66·346	.....	65·84

The weight was very uniform during almost the whole time; but it is plain he lost a little weight during the first exercise-period, and continued afterwards to lose it. The change was, however, inconsiderable, as there was a loss of only 2 lbs. in weight during the whole period.

2. *Elimination of Nitrogen.*

The nitrogen in the urine and fæces was determined every day by burning with soda-lime. The urine was collected from 8 A.M. to 8 A.M. The weights are given in grammes.

Date.	Nitrogen in urine.	Nitrogen in fæces.	Total by the two outlets.
First Period.			
March.			
13 to 14.	15·195	3·195	18·390
14 to 15.	15·307	4·199	19·506
15 to 16.	14·812	4·519	19·331
16 to 17.	14·878	4·207	19·085
17 to 18.	14·693	3·275	17·968
18 to 19.	16·314	3·194	19·508
Mean………	15·183	3·765	18·948
Second Period. Exercise on Water.			
19 to 20.	15·861	4·153	20·014
20 to 21.	16·367	5·182	21·549
21 to 22.	16·593	5·609	22·202
Mean………	16·274	4·981	21·255
Third Period. Rest.			
22 to 23.	15·504	4·026	19·530
23 to 24.	16·570	3·476	20·046
24 to 25.	15·487	2·250	17·737
Mean………	15·850	3·251	19·101
Fourth Period. Exercise on Brandy.			
25 to 26.	14·644	5·075	19·719
26 to 27.	15·484	4·250	19·734
27 to 28.	17·122	3·790	20·910
Mean………	15·750	4·372	20·121
Fifth Period. Rest.			
28 to 29.	15·007	3·215	18·212
29 to 30.	?	2·977	

During the sixteen days 19·59 grammes of nitrogen were discharged by the urine and bowels every day. The daily entrance of nitrogen during the same period was 20 grms., viz. 16 grms. in the oatmeal and 4 grms. in the milk.

To estimate the effect of exercise and alcohol on the elimination of nitrogen, the urinary and the alvine exits must be separately considered.

The mean daily discharge of the urinary nitrogen of the sixteen days is 15·6 grms. If the mean amounts of the different periods are looked at, it will be seen how little they deviate from the mean. There is, however, in

both the exercise periods a slight excess over the first period, amounting to 1 and ·5 grm. respectively. The differences are so slight that they might be disregarded, only the recurrence in the two exercise periods seems to show that the slight excess is not accidental.

The observations support the facts noted in the papers already communicated to the Royal Society, viz. that there is a slight excess of nitrogen in a mixed exercise and rest period over a period of entire rest, owing, it would appear from the former experiments in the rest period, to a slight increase in elimination *after* exercise.

The brandy had no effect on the elimination of nitrogen, for the mean of the two periods of exercise only differs by 0·5 grm., and the results are practically identical. And as a further proof that the brandy had no effect in either direction, it may be noticed that both the highest and lowest daily excretion occurred in the brandy period.

With regard to the alvine nitrogen, the elimination was increased during the exercise periods, the mean of the whole sixteen days being 3·976 grms.; it was 4·981 grms. and 4·372 grms. in the two exercise periods. This depended on the effect of the particular kind of exercise (digging) on the action of the abdominal muscles and on the bowels, as will be seen from the next Table, if the following point is taken into account. In the first period there was a loose alvine discharge during four or five days, arising apparently from the use of the oatmeal in a man who had lately been accustomed to other food. Under the microscope the envelopes of the oat and of barley or wheat which had been mixed with it were seen in quantities in the fæces; therefore in this period the large amount of solids and the smaller amount of nitrogen were given by considerable portions of undigested oatmeal passing out; in later periods the proper excreta of the intestines were probably in larger amount.

Mean daily Alvine Excreta, in grammes.

	First period. Rest.	Second period. Exercise on water.	Third period. Rest.	Fourth period. Exercise on brandy.	Fifth period. Rest.
Daily weight of the excreta	364	371	301	519	306
Percentage of solids in do.	21·08	20·9	21·5	22·1	22·2
„ water in do.	78·92	79·1	78·5	77·9	77·8
Daily weight of solids of excreta	76·73	77·54	64·7	110·5	67·9
Daily amount of alvine water	287·27	293·46*	236·3	408·5	238·1

* Adopting 294 cub. centims. of water as the daily alvine discharge, there passed daily by the bowels 10·35 fluid ounces, and by the urine 71 fluid ounces of water from this man, or 1 to 7 nearly. The amount is much more than in meat-eaters. As he took about 130 fluid ounces of water daily, about 49 fluid ounces must have passed off by the skin and lungs.

If we adopt the two last rest periods, which are nearly alike, as the standard of the usual alvine excreta under this diet, the muscular work (digging), by acting mechanically on the bowels, caused a greater discharge both of the solid parts of the excreta and of water; much of the difference of the nitrogen, and perhaps the whole, is thus accounted for.

It was very remarkable how uniform the excreta were in the percentage of water when the means of the periods are taken, though from day to day there was considerable variation. It will be seen how much larger the excreta were than is usual in meat-eaters, and how much nitrogen passed from the bowels.

3. *The amount of Urinary Water, Chlorine, Phosphoric Acid, and free Acidity.*

To save space I give only the means of the periods.

Daily Excretion, in grammes, by the Urine.

	First period. Rest.	Second period. Exercise on water.	Third period. Rest.	Fourth period. Exercise on brandy.	Fifth period. Rest.
Water of urine (in cub. centims.)......	2016	1823	2140	2006	2086
Chlorine	1·911	1·890	2·016	1·775	2·058
Phosphoric acid	2·857	2·875	2·731	2·935	2·576
Free acidity (taken as crystallized oxalic acid)	2·953	2·759	2·678	3·008	2·690

The phosphoric acid was not affected in the first exercise period; it was slightly increased in the alcoholic, but the difference may be too slight to be material. The chlorine was rather lessened in both exercise periods: the free acidity was slightly increased in the brandy period over the exercise period on water; but the change was so slight as to be within the range of daily variation. Exercise on water exerted no marked influence on the free acidity as compared with the periods before and after.

4. *The Pulse**.

The pulse was taken every two hours after the man had been in a recumbent position for at least fifteen minutes.

* The pulse and temperatures were taken with very great care by Serjeant Turner of the Army Hospital Corps.

Date. March.	Hours. 6 A.M.	8 A.M.	10 A.M.	12 P.M.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	Mean of day.
Before exercise.										
13.	61	61	71	52	67	62	62	61	60	61·9
14.	61	64	66	53	68	64	62	64	55	61·9
15.	55	56	67	54	60	66	73	60	62	61·4
16.	60	52	72	60	69	72	70	70	60	65·0
17.	56	60	66	61	64	64	66	60	56	61·4
18.	54	60	68	60	64	72	64	60	57	62·8
Mean of hours.	57·5	58·8	68·3	56·6	65·3	66·6	66·1	62·5	58·3	62·2
Exercise on Water.										
19.	58	54	72	92	80	72	68	64	63	69·8
20.	60	60	59	67	66	67	76	67	56	64·2
21.	66	60	69	64	66	62	58	68	60	63·6
Mean of hours.	61·3	58·0	66·6	74·3	70·6	67·0	67·3	66·3	59·2	65·8
After exercise on Water.										
22.	57	74	62	60	68	66	72	68	59	65·1
23.	62	60	64	60	62	70	62	64	61	62·7
24.	68	60	66	62	62	58	63	70	58	63·0
Mean of hours.	62·3	64·6	64·0	60·6	64·0	64·6	65·6	67·3	59·3	63·6
Exercise on Alcohol.										
25	59	62	66	70	78	79	74	82	64	70·44
26	64	66	76	72	73	68	76	74	68	70·8
27	65	68	68	58	72	79	78	72	68	69·8
Mean of hours.	62·6	65·3	70·0	66·6	74·3	75·3	76·0	76·0	66·6	70·35
After exercise on Alcohol.										
28	68	68	70	62	59	62	59	57	50	61
29	60	56	62	57	66	59	61	57	62	60
30	60	61	..	...	...	...	...	...	...	...
Mean of hours.	62·6	61·6	66·0	59·5	62·5	60·5	60·5	57·0	56·0	60·5

The pulse was very regular in the man, and was strong and slow. The mean of the first six days gave 62·2 beats per minute. Exercise on water

raised the mean daily beats from 62·2 to 65·8. In the next rest period the mean daily pulse sank to 63·6, but rose in the exercise and brandy period to 70·35. The effect of the brandy was therefore to cause a daily increase of 6552 pulsations of the heart over the exercise period with water, and an excess of 11304 pulsations over the first rest period. The effect of the brandy is seen at once in comparing the hours 2, 4, 6, 8, and 10 P.M. in the two exercise periods.

Mean pulse.

	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.
Exercise on water......	70·6	67·	67·3	66·3	59·2
Exercise on brandy ...	74·3	75·3	76	76	66·6

At 12 o'clock the same fact would have been noted, but that on the first exercise day the pulse was greatly augmented (to 92 per minute or 50 per cent.) by marching eight miles in heavy marching order from 10 to 12 o'clock. There is hardly any form of exercise which augments the pulse like quick movements, especially when weights are carried. In the second period the corresponding exercise was digging, which increases the pulse much less. Again, on the third day of the brandy period, he did no work from 11 to 12 o'clock, as it was raining; and consequently at 12 o'clock on that day his pulse, which in the two previous days was 70 and 72, was only 58. These two facts explain the only exception in the Tables to the rule of the excess of heart-beats, under the influence of alcohol, at every hour. The effect on the heart was also made evident in a way presently to be noticed.

5. *The Temperature of the Body.*

Axilla temperature (the thermometer was inserted for 20 minutes).

Before exercise.										
Date. March.	Hours.									Mean of day.
	6 A.M.	8 A.M.	10 A.M.	12 P.M.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	
13.	97·4	97·2	96·5	97·8	97·4	98·4	98·4	97·8	97·4	97·6
14.	97·0	97·8	98·2	97·8	98·2	98·3	98·4	98·0	98·8	98·0
15.	97·5	97·6	98·0	98·4	98·0	98·5	98·8	98·0	97·9	98·1
16.	97·0	97·4	98·8	98·9	98·4	98·4	98·4	98·7	97·2	98·1
17.	97·2	97·7	98·6	98·7	98·5	98·5	98·4	98·4	97·6	98·2
18.	97·2	97·4	98·0	98·4	98·2	98·2	98·3	97·2	97·2	97·8
Mean of hours.	97·2	97·5	98·0	98·3	99·1	98·4	98·5	98·0	97·7	97·98

Date. March.	6 A.M.	8 A.M.	10 A.M.	12 P.M.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	Mean of day.
Exercise on Water.										
19.	97·4	97·4	98·6	99·0	98·4	98·4	98·6	98·2	97·6	98·1
20.	97·0	97·5	97·8	97·6	97·4	98·0	98·8	98·2	97·8	97·7
21.	97·3	98·2	98·8	98·4	98·7	98·4	97·9	98·6	98·2	98·2
Mean of hours.	97·2	97·7	98·4	98·3	98·1	98·2	98·4	98·3	97·8	98·0
After exercise on Water.										
22.	97·6	98·2	97·6	98·4	98·8	98·4	98·4	98·4	97·2	98·1
23.	97·8	97·8	98·2	98·2	98·2	98·8	98·8	99·0	98·0	98·3
24.	97·4	97·6	98·2	98·8	98·6	98·6	98·8	97·6	97·6	98·2
Mean of hours.	97·6	97·8	98·0	98·5	98·5	98·6	98·7	98·3	97·6	98·2
Exercise on Brandy.										
25.	97·2	98·5	98·2	98·6	98·2	97·6	98·5	98·8	98·4	98·2
26.	97·3	98·2	98·4	98·0	98·0	98·2	98·0	98·0	98·2	98·0
27.	98·1	98·4	98·4	98·6	98·6	98·6	98·2	98·0	98·2	98·3
Mean of hours.	97·5	98·4	98·3	98·4	98·3	98·1	98·2	98·6	98·6	98·2
After exercise on Brandy.										
28.	98·2	98·5	98·2	98·2	98·0	98·6	98·4	98·0	97·8	98·2
29.	97·6	97·8	98·2	98·6	98·8	98·8	98·8	98·6	98·2	98·3
30.	98·2	98·2	...	...	...	...	...	...	...	...
Mean of hours.	98·0	98·1	98·2	98·4	98·4	98·7	98·6	98·3	98·0	98·25

The effect of exercise and water on the axilla temperature was imperceptible. The effect of exercise and brandy was also quite negative. The mean daily temperature, as obtained by observations every two hours, from 6 A.M. to 10 P.M., was 98° and 98°·2, or practically the same.

If the hours 12, 2, 4, 6, and 10, when the brandy was chiefly acting, are compared in the two periods, it appears quite certain that 12 fluid ounces (=341 cub. centims.) of brandy caused no diminution in temperature.

Mean Axillary Temperature.

	Hours.					
	12 noon.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.
Exercise and water	98·3	98·1	98·2	98·4	98·3	97·8
Exercise and 4 ounces of brandy at 10, 2, and 6 o'clock	98·4	98·3	98·1	98·2	98·26	98·26

In three of the six hours the temperature was in excess in the water period and in three in the brandy period; but the differences between the two series are not greater than between the hours of successive days in any period, and are in opposite directions.

Temperature of Rectum.

First period.						
Date. March.	Hours.					Mean of day.
	6 A.M.	10 A.M.	2 P.M.	6 P.M.	10 P.M.	
13.	98·0	99·4	99·3	99·2	98·4	98·86
14.	98·0	99·0	99·2	99·2	98·6	98·8
15.	97·9	99·6	99·1	100·2	98·6	99·1
16.	97·8	99·5	99·3	99·4	98·9	98·98
17.	97·5	98·9	99·4	99·6	98·5	98·8
Mean of hours.	97·84	99·28	99·26	99·52	98·6	98·91
Exercise and brandy period.						
25.	98·6	99·2	99·15	99·3	99·2	99·1
26.	98·2	99·7	99·0	99·0	99·2	99·0
27.	98·6	99·0	99·0	99·4	99·2	99·0
Mean of hours.	98·4	99·3	99·05	99·2	99·2	99·0

The thermometer was inserted 4 inches into the rectum, and, unfortunately, gave rise to some irritation, viz. a feeling of pain and heaviness and the discharge of a little blood. We were obliged to discontinue the observations for several days, and can therefore only compare the first five days with the alcoholic period.

The comparison shows that alcohol causes no depression of the rectal temperature; there is even a slight elevation at 10 o'clock P.M., but as the reverse is the case at 6 o'clock P.M., no weight can be given to it. As

these results accord with the former experiments, it cannot, I think, be doubted that alcohol, in the amounts given in these trials, has no influence on the bodily temperature of healthy men.

6. *The elimination of Alcohol.*

As only qualitative experiments were made, it is not necessary to do more than state that before the brandy was given nothing could be found passing off by the skin, lungs, or kidneys which had the slightest reducing effect on Masing's bichromate-of-potassium test; while after the brandy a substance which at once reduced the test was passing off by all these channels, and especially by the skin, but the amount was not determined.

7. *The effect of Brandy on the work done.*

As the amount and kind of work done in the two exercise periods was nearly the same, I requested the man to observe as closely as he could whether he did the work better with or without the brandy. He commenced the exercise and brandy period with a belief that the brandy would enable him to perform the work more easily, but ended it with the opposite conviction. As already stated, the brandy was taken in 4-ounce doses at 10 A.M., 2 P.M., and 6 P.M., in an equal quantity of water, and the work was chiefly done in the two hours immediately succeeding each dose, and from 6 to 8 A.M.

The two hours' work from 10 A.M. to 12 noon, immediately after the first four fluid ounces of brandy, was, he thought, done equally well with and without the brandy. The man affirmed that he could tell no difference, except that, to use his own words, "the brandy seemed to give him a kind of spirit which made him think he could do a great deal of work; but when he came to do it, he found he was less capable than he thought."

After the second four ounces of brandy he felt hot and thirsty, but on the two first days he thought he worked as well as on the water days; on the third day, however, he had palpitation of the heart, and was surprised to find he was obliged to stop from time to time, because, to use his own words, "of his breathing not being so good."

The third four fluid ounces of brandy at 6 P.M. produced on all three days very marked narcotic effects. Immediately after taking it he became heavy, felt the greatest indisposition to exert himself, and could hardly refrain from throwing down his spade and giving up work. He worked with no vigour, and on the second evening thought his muscular power decidedly lessened. On the third evening, as it was raining, he could not dig, but took walking and running exercise under cover. On attempting to run, he found, to his great surprise, as he is a particularly fast and good runner, that he could not do so. He had palpitation and got out of breath, and was obliged to stop; so that he stated on the next day, "that if he had had his accoutrements on and been ordered to 'double,' he could not have obeyed the order."

After coming in from work on each evening he fell into a heavy sleep, from which he was roused with difficulty. This lasted for three or four hours, after which he was restless and sleepless.

The man's own judgment was, at the end of the trial, that he would prefer to do the work without the brandy; and when asked for his reasons, he mentioned "the increased thirst, the heaviness in the evening, and the fluttering at the heart."

His appetite was not affected.

Conclusions.

1. The elimination of nitrogen during exercise was unaffected by brandy; and since the experiments led to the same result in the former series during comparative rest, it seems certain that in healthy men on uniform good diet alcohol does not interfere with the disintegration of nitrogenous tissues.

2. The heat of the body, as judged of by the axilla and rectum temperatures, was unaffected by the amount given.

3. The pulse was increased in frequency by 4 ounces of brandy, and palpitation and breathlessness were brought on by larger doses, to such an extent as to greatly lessen the amount of work the man could do, and to render quick movements impossible. As the effect of labour alone is to augment the strength and frequency of the heart's action, it would appear obviously improper to act on the heart still more by alcohol. In this effect on the heart, and through it on the lungs, is perhaps to be found the explanation of the trainer's rule, which prohibits alcohol during exertion. Whether in a heart exhausted by exertion alcohol would be good or bad is not shown by these experiments; but it can hardly be supposed that to urge a heart which requires rest, as would then be the case, can be proper.

4. It seems clear, from the suddenness with which marked narcotic symptoms came on after the third dose was taken on each day, that the eight hours from 10 to 6 o'clock were not sufficient to get rid of the brandy taken at 10 and at 2, and that in fact the body must have been still saturated at 6 o'clock.

The exact amount of brandy which commenced to lessen the labour the man could perform is not shown by these observations, and would require more careful modes of investigation. It was evidently some quantity more than 4 ounces which produced effects sufficiently marked to attract his attention; but I should not wish to affirm that even 4 ounces produced no effect in this direction. The man himself was of opinion that 4 ounces had no influence either way. He was quite certain it did not aid his work, but he could not see that it injured it. The second 4 ounces decidedly produced a bad effect.

5. Neither exercise on water or on alcohol produced any effect on the phosphoric acid of the urine. The result is in accordance with that of the

experiments recorded in No. 89 of the 'Proceedings of the Royal Society' (vol. xv. p. 339).

The effect on the free acidity of the urine was inconsiderable. The free acidity may have been a little increased in the brandy period, but the change was slight.

The effect on the chlorine was not certain, as its ingress was not sufficiently constant, but it seems to be lessened in the exercise period.

As the action of alcohol in dietetic doses on the elimination of nitrogen and on the bodily temperature is so entirely negative, it seems reasonable to doubt if alcohol can have the depressing effect on the excretion of pulmonary carbon which is commonly attributed to it. It can hardly depress, one would think, the metamorphosis of tissues, or substances furnishing carbon, without affecting either the changes of the nitrogenous structures or bodily heat. It seems most important that fresh experiments should be made with respect to its effect on carbon elimination, as without a perfect knowledge on that point the use of alcohol as an article of diet in health cannot be fairly discussed.

IV. "Report on Scientific Researches carried on during the Months of August, September, and October, 1871, in H.M. Surveying-Ship 'Shearwater.'" By WILLIAM CARPENTER, LL.D., M.D., F.R.S. Received June 13, 1872.

[This paper will appear in full in a future Number of the 'Proceedings.']

June 20, 1872.

Sir JAMES PAGET, Bart., D.C.L., Vice-President, in the Chair.

Prof. William Grylls Adams, Dr. Andrew Leith Adams, Dr. John Cleland, Dr. Michael Foster, Prof. William Stanley Jevons, and Dr. William James Russell were admitted into the Society.

The following communications were read:—

I. "Preliminary Note on the Reproduction of Diffraction-gratings by means of Photography." By the Hon. J. W. STRUTT, M.A. Communicated by Prof. G. G. STOKES, Sec. R. S. Received May 23, 1872.

During the last autumn and winter I was much engaged with experiments on the reproduction of gratings by means of photography, and met with a considerable degree of success. A severe illness has prevented my pursuing the subject for some months, and my results are in consequence still far from complete; but as I may not be able immediately to resume my experiments, I think it desirable to lay this preliminary note before the

Royal Society, reserving the details and some theoretical work connected with the subject for another opportunity.

It is some years since the idea first occurred to me of taking advantage of the minute delineating power of photography to reproduce with facility the work of so much time and trouble. I thought of constructing a grating on a comparatively large scale, and afterwards reducing by the lens and camera to the required fineness. I am now rather inclined to think that nothing would be gained by this course, that the construction of a grating of a given number of lines and with a given accuracy would not be greatly facilitated by enlarging the scale, and that it is doubtful whether photographic or other lenses are capable of the work that would be required of them.

However this may be, the method that I adopted is better in every respect, except perhaps one. Having provided myself with a grating by Nobert, with 3000 lines ruled over a square inch, I printed from it on sensitive dry plates in the same way as transparencies for the lantern are usually printed from negatives.

In order to give myself the best chance of success, I took as a source of light the image of the sun formed by a lens placed in the shutter of a dark room. I hoped in this way that, even if there should be a small interval between the lines of the grating and the sensitive surface, still a *shadow* of the lines would be thrown across it. Results of great promise were at once obtained, and after a little practice I found it possible to produce copies comparing not unfavourably with the original. A source of uncertainty lay in the imperfect flatness of the glass on which the sensitive film was prepared, though care was taken to choose the flattest pieces of patent plate. The remedy is, of course, to use worked glass, which is required in any case if the magnifying-power of a telescope is to be made available.

Almost any of the dry processes known to photographers may be used. I have tried plain albumen, albumen on plain collodion, and Taupenot plates. The requirements of the case differ materially from those of ordinary photography, sensitiveness being no object, and hardness rather than softness desirable in the results. After partial development, I have found a treatment with iodine, in order to clear the transparent parts, very useful. In proceeding with the intensifying, the deposit falls wholly on the parts that are to be opaque. It is more essential that the transparent parts should be quite clear than the dark parts should be very opaque.

The performance of these gratings is very satisfactory. In examining the solar spectrum, I have not been able to detect any decided inferiority in the defining-power of the copies. With them, as with the original, the nickel line between the D's is easily seen in the third spectrum. I work in a dark room, setting up the grating at a distance from the slit fastened in the shutter, and using no collimator. The telescope is made up of a single lens of about thirty inches focus for object-glass, and an ordinary eyepiece held independently. I believe this arrangement to be more

efficient than a common spectroscope, with collimator and telescope all on one stand; at any rate, the magnifying-power is considerably greater, and it seems to be well borne.

I have also experimented on the reproduction of gratings by a very different kind of photography. It will be remembered that a mixture of gelatine with bichromate of potash is sensitive to the action of light, becoming insoluble, even in hot water, after exposure. In ordinary carbon printing the colouring-matter is mixed with the gelatine and the print developed with warm water, having been first transferred so as to expose to the action of the water what was during the operation of the light the hind surface. In my experiments the colouring-matter was omitted, and the bichromated gelatine poured on the glass like collodion and then allowed to dry in the dark. A few minutes' exposure to the direct rays of the sun then sufficed to produce such a modification under the lines of the gratings that, on treatment with warm water, a copy of the original was produced capable of giving brilliant spectra. In these gelatine-gratings all parts are alike transparent, so that the cause of the peculiar effect must lie in an alternate elevation and depression of the surface. That this is the case may be proved by pressing soft sealing-wax on the grating, when an impression appears on the wax, giving it an effect like that of mother of pearl. It is known that the effect of water on a gelatine print is to make the protected parts project in consequence of their greater absorption, but it might have been expected that on drying the whole would have come flat again. It is difficult to say exactly what does happen; and I am not even sure whether the part protected by the scratch on the original is raised or sunk. Gelatine can scarcely be actually dissolved away, because the uppermost layer must have become insoluble under the influence of the light. I do not at present see my way to working by transfer, as in ordinary carbon printing.

I have not yet been able to reduce the production of these gelatine-gratings to a certainty, but can hardly doubt the possibility of doing so. One or two of considerable perfection have been made, capable of showing the nickel line between the D's, and giving spectra of greater brightness than the common photographs. Not only so, but the gelatine copy surpasses even the original in respect of brightness. The reason is that, on account of the broadening of the shadow of the scratch, a more favourable ratio is established between the breadths of the alternate parts.

Theory shows that with gratings composed of alternate transparent and opaque parts the utmost fraction of the original light that can be concentrated in one spectrum is only about $\frac{1}{10}$, and that this happens in the first spectrum when the dark and bright parts are equal. But if instead of an opaque bar stopping the light, a transparent bar capable of retarding the light by half an undulation can be substituted, there would be a fourfold increase in the light of the first spectrum. I accordingly anticipate that the gelatine-gratings are likely to prove ultimately the best, if the conditions of their production can be sufficiently mastered.

With regard to the application of the photographs, I need not say much at present; it is evident that the use of gratings would become more general if the cost were reduced in the proportion, say, of 20 to 1, more particularly if there were no accompanying inferiority of performance.

The specimens sent with this paper are both capable of showing the nickel line and give fairly bright spectra, but they must not be supposed to be the limit of what is possible. From their appearance under the microscope I see no reason to doubt that lines 6000 to the inch can be copied by the same method, a point which I hope shortly to put to the test of experiment.

II. "On the 26-day Period of the Earth's Magnetic Force." By J. A. Broun, F.R.S. Received June 3, 1872.

The Astronomer Royal's communication to the Royal Society on this subject (*suprà*, p. 308) has drawn my attention to the investigation made by the Director of the Prague Observatory.

Dr. Hornstein having remarked the uncertainty of the result for the time of the sun's rotation as deduced from the movement of the spots in different zones on its surface, thought it would be desirable to consider other phenomena associated with the sun's rotation; and the apparent connexion of the frequency of the solar spots with the amount of the magnetic oscillations induced him to seek for a period in the daily mean values of the magnetic elements. For this end he grouped the daily means of observations made at Prague and Vienna in 1870 in periods varying from 16 to 28 days; and subjecting the resulting means to calculation for the term

$$a \sin(\theta + c)$$

in the usual formula of sines, he considered the most probable of the periods to be that for which a had the greatest value.

The most carefully calculated result from the declination at Prague gave 26·7 days nearly, but a graphic interpolation from the same observations indicated 26·2 days; the declination for the same year at Vienna giving by calculation 26·4 days, while the inclination at Prague showed 26 days.

Dr. Hornstein concludes that the mean of these four values, "26·33 days, may be considered provisionally as the most probable value [of the period], and as the result of the first experiment to determine the time of the sun's synodical rotation by means of the magnetic needle. The true time of the sun's rotation derived from this = 24·55 days, almost exactly coinciding with the value found by Spoerer from astronomical observations for the time of rotation of the sun's spots in the equatoreal zone" *.

In a letter to the late Sir David Brewster, published in the 'Philosophical

* Sitzungsb. der k. Akad. d. Wissensch. zu Wien, Band lxiv. S. 73.

Magazine' for August 1858, I made the following remark:—"The result which I have now obtained from three years' observations near the magnetic equator, it appears to me, is wholly independent of the moon, and is due to the sun's rotation on its axis. If we could suppose that the solar magnetic poles are fixed, it might then be possible to determine accurately the time of the sun's rotation by means of the movement of our magnets. If, on the other hand, the poles are in motion, as I conceive they are, we shall have to employ another period than 25·325 days, as obtained from the solar spots. The period to be employed will of course be found by careful examination of the observations and by trial" [of different times]. These views resulted from a discussion of observations of the horizontal force made at Trevandrum in the years 1855, 1856, and 1857.

In February 1861 a paper by me was read to the Royal Society of Edinburgh, containing conclusions derived from an examination of all the observations of the horizontal force which had been published, made between 1842 and 1848, the whole having been recorrected for temperature by my own method, for the purpose of this and other investigations. Founding chiefly on the simultaneous daily means of observations made at Makerstoun, Trevandrum, Singapore, and Hobarton during the years 1844 and 1845 (the only years for which a complete diurnal series existed for Makerstoun), I arrived at the conclusion, from an examination of a series of successive periods, shown nearly equally well at all the four stations, that there was a period of nearly 26 days, probably due to the sun's rotation on its axis, the mean of the whole number of periods being 25·96 days *. The observations, however, seemed to show that the single periods had a variable length, though this might be due to some extent to irregular disturbing causes, yet dependent, perhaps, on the period of the year, and it might be on other arguments, such as the position of the planets.

In the examination of observations made by me in India, I had sought for the greatest amplitude on trial of periods of 26 to 27 days, but postponed a complete investigation till the observations of a greater number of years should enable me to follow more certainly the apparent variations in the length of the single periods; this other investigations have hitherto prevented, and I am glad that Dr. Hornstein has taken up the subject, though evidently ignorant of my previous conclusions.

Mr. Airy's communication on this subject is very important; its conclusion refers to a period of 26⅓ days; and although the evidence may be imperfect for such a period, it seems to me that the Greenwich results, when projected, give considerable grounds for concluding that a period of *near* 26 days exists. In what follows, I limit myself wholly to the observations of the horizontal force, as I have found that element, when accurately corrected for temperature, best fitted to show the period in question.

The Astronomer Royal has noted a probable inaccuracy in the amount of the correction employed for the secular change; as this correction is of

* Trans. Roy. Soc. Edinb. vol. xxii. pl. xxvii. and p. 543.

importance in the investigation, I have sought to determine it from the means for the 26⅓-day period in each year. A consideration of the mean value of the horizontal force at Greenwich will show that it varies irregularly from year to year, and that no single value employed as a correction due to a supposed uniform change from the beginning to the end of a year would satisfy all the years. I adopted the following method to obtain this correction (not being in possession of the Greenwich observations):—by comparing the mean value for the 1st day with that for the 14th, that of the 2nd day with the mean for the 15th, and so on, 13 values of the change for 13 days are obtained for any gradual change, which will be approximately true whether there be a period of 26 days or no period whatever. This method gave the mean change during 13 days for each year as follows:—

1850.	1851.	1852.	1868.	1869.	1870.
+24	−4·5	−9	−26	+7·5	−15

the unit being in all cases ·00001 of the whole horizontal force. Having applied the proportional parts of these quantities with the signs changed to the mean values of horizontal force for the respective years (Table, p. 310, *suprà*), I then calculated the values of the constants for the term

$$a \sin(\theta + c).$$

The results are as follow:—

			d.
1850......	$6·2 \sin(\theta + 64°)$	with maximum at	1·9
1851......	$5·0 \sin(\theta + 274°)$	„	12·8
1852......	$20·6 \sin(\theta + 266°)$	„	13·2
1868......	$17·2 \sin(\theta + 121°)$	„	23·7
1869......	$11·2 \sin(\theta + 256°)$	„	14·0
1870......	$43·5 \sin(\theta + 278°)$	„	12·4

Mean of group:

1850–52....	$7 \sin(\theta + 282°)$	with maximum at	12·1
1868–70....	$12 \sin(\theta + 258°)$	„	13·8

It will be seen that the four years 1851, 1852, 1869, and 1870 give, within a day and half, the same epoch for the maximum. I shall return to these results immediately, but shall first consider the observations already referred to, made at Makerstoun in 1844 and 1845, which gave a period of 25·96 days.

The secular change for each year was first determined as follows:—the mean horizontal force for December 1843 minus the mean for December 1844, and the mean for January 1844 minus the mean for January 1845*, gave two results, the mean of which was taken as the change during 1844; in a similar way the change for 1845 was found: the corrections

* Table xxii., Trans. Roy. Soc. Edinb. vol. xix. pt. 2, p. xxxi.

for 26 days were found for 1844 = −20, for 1845 = −7. The daily means from the Tables * were then arranged in groups of $25\frac{2}{3}$, 26, and $26\frac{1}{3}$ days, as follows :—

$25\frac{2}{3}$ days	1844, Jan. 1 to 1844, Dec. 29 = 14 periods.
	1844, Dec. 25 to 1846, Jan. 14 = 15 ,,
26 days	1844, Jan. 1 to 1844, Dec. 29 = 14 ,,
	1844, Dec. 30 to 1845, Dec. 28 = 14 ,,
$26\frac{1}{3}$ days	1844, Jan. 1 to 1845, Jan. 3 = 14 ,,
	1845, Jan. 4 to 4846, Jan. 6 = 14 ,,

The corrections for secular change were applied to the means derived from these groups, and in the calculation for $26\frac{1}{3}$ days the fractional part of a day in excess was omitted, while in that for the $25\frac{2}{3}$ days the 26th day of each third period served also as the 1st day of the next. The resulting means are given in the following Table :—

Mean Horizontal Force for each day in periods of $25\frac{2}{3}$, 26, and $26\frac{1}{3}$ days, as deduced from the Makerstoun Observations for 1844 and 1845.

Period.	Year.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13
d.														
$25\frac{2}{3}$......	1844	99	95	95	102	101	88	83	84	80	85	91	70	41
	1845	243	259	262	241	246	236	218	210	231	242	213	227	236
26	1844	82	100	92	98	88	95	79	90	67	70	59	48	49
	1845	234	192	218	227	228	238	240	236	244	201	198	210	214
$26\frac{1}{3}$......	1844	92	97	102	87	72	81	68	71	68	65	51	30	55
	1845	242	249	204	212	218	221	216	233	243	235	247	255	242

Period.	Year.	14.	15.	16.	17.	18.	19.	20.	21.	22.	23.	24.	25.	26.
d.														
$25\frac{2}{3}$......	1844	59	48	56	64	51	72	60	69	76	90	108	93	102
	1845	250	236	226	234	224	219	221	223	222	232	237	237	246
26	1844	55	50	56	69	71	81	87	83	100	111	106	107	94
	1845	220	225	231	241	238	246	256	251	230	236	244	234	231
$26\frac{1}{3}$......	1844	69	73	78	82	90	84	97	104	112	101	95	90	90
	1845	247	249	237	221	250	216	236	231	217	227	245	251	244

The unit = 0·000014 of the whole horizontal force.

A constant quantity 0·00300 was subtracted from the means deduced from the Tables to obtain the above quantities.

Having, as for the Greenwich means, calculated the constants for the term $a \sin(\theta + c)$, they were found as follows :—

* Table xxii. Makerstoun Observations, 1844, Trans. Roy. Soc. Edinb. vol. xviii. p. 355; and Tables xviii. and lvii. Makerstoun Observations, 1845, Trans. Roy. Soc. Edinb. vol. xix. pt. 2, pp. 11, 32.

Period.	1844.		1845.		Difference, 1845–1844.
	$a \sin(\theta + c)$.	Max.	$a \sin(\theta + c)$.	Max.	
d.		d.		d.	d.
$25\frac{2}{3}$ ..	$22{\cdot}9 \sin(\theta + 69^\circ)$	1·5	$10{\cdot}5 \sin(\theta + 288^\circ)$	11·7	+10·2
26 ..	$24{\cdot}1 \sin(\theta + 101^\circ)$	25·0	$10{\cdot}6 \sin(\theta + 159^\circ)$	21·0	− 4·0
$26\frac{1}{3}$ ..	$2{\cdot}22 \sin(\theta + 132^\circ)$	22·9	$10{\cdot}4 \sin(\theta + 252^\circ)$	15·7	− 7·2

It will be seen that though the constant a varies little, it is greatest for each year by the period of 26 days; on the other hand, the constant c varies considerably; and this constant may be made the most delicate test for the true mean period. By the period of $25\frac{2}{3}$ days, the maximum happens between the 1st and 2nd days in 1844, but ten days later in 1845; by the $26\frac{1}{3}$-day period, on the other hand, it happens seven days earlier in 1845 than in 1844. The 26-day period shows a difference in the same direction of four days.

It will be evident that if the time employed for the calculation be less than the true period, the maximum resulting from the superposition of a succession of periods will be thrown later than the maximum derived from the exact period; if, on the other hand, the time employed be longer than the true time, the maximum resulting from the superposition will occur earlier. The difference between the epochs of maximum derived from two successive series of groups will thus give an approximation to the error of the period employed. If we apply this to the difference of epochs in the preceding Table, remembering that 14 periods intervene between the means from the two years (excepting for the $25\frac{2}{3}$ days, when there are $14\frac{1}{2}$ periods), we shall obtain the following corrections to the times employed:—

$$
\begin{aligned}
25\tfrac{2}{3} + \frac{10{\cdot}2}{14{\cdot}5} &= \overset{\text{d.}}{26{\cdot}36} \\
26 - \frac{4}{14} &= 25{\cdot}71 \\
26\tfrac{1}{3} - \frac{7{\cdot}2}{14} &= 25{\cdot}82 \\
\text{Mean} &= 25{\cdot}96
\end{aligned}
$$

which is exactly the period found in my paper, already referred to, from an examination of the intervals from minimum to minimum throughout the two years.

It is not unlikely, however, that this exact agreement is accidental; the constant c requires for its determination, not only an exact correction for the secular change, but also for the annual period; though any slight change which this additional correction might produce would probably have little effect on the final result.

With these results in view, we may now return to the Greenwich values.

If we suppose, as the above calculations indicate, that the interval from maximum to maximum is somewhat less than $26\frac{1}{3}$ days, we obtain from the differences of the epochs for 1852–1850 and 1870–1868 the following values of the period :—

$$1852 \text{ minus } 1850 \,..\, 26{\cdot}33 - \frac{(26{\cdot}33 + 1{\cdot}9) - 13{\cdot}2}{28} = 25{\cdot}79 \text{ days.}$$

$$1870 \text{ minus } 1868 \,..\, 26{\cdot}33 - \frac{11{\cdot}3}{28} = 25{\cdot}90 \;\;,,$$

The result from 1870 and 1868 is that which merits the greatest value, owing to the largeness of the coefficient a for these two years. If we assume, then, that the true period is 26 days, we may, by applying a correction to the epochs of maximum for 1870 and 1852 of $+\frac{14}{3}$ days $= 4\frac{2}{3}$, find the epochs for 1869 and 1851: these would be for 1869, 17·1 days, or three days later than results from the calculation; and for 1851, 17·9 days, five days later than from the calculation; and the smallness of the coefficient for 1851 would show that an error of this amount is quite possible.

We have still, however, another means of determining which of the values, 26 or $26\frac{1}{3}$, is the most probable. The two best-marked results from the Greenwich observations are those for 1852 and 1870; we may compare the epochs of maximum from these years with that derived from the Makerstoun observations for 1844. The terms for these three years having the largest value of the constant a are (in 100,000ths of the force):—

1844...... $33{\cdot}7 \sin(\theta + 101°)$ max. 25 days.
1852...... $20{\cdot}6 \sin(\theta + 266°)$ max. 13 ,,
1870...... $43{\cdot}5 \sin(\theta + 278°)$ max. 12·4 ,,

Now as the period began in 1844 with the 1st of January, the first maximum in that year corresponded to the 25th January. In 1852 (the first day of the period being the 31st December, 1851) the maximum happened on the 12th January, 1852. In 1870 the first day of the period was the 8th January, and the maximum occurred on the $20\frac{1}{2}$ January, 1870 *; and we have for the epochs of maximum, with the intervals in days, as follow :—

	Intervals.
1844, Jan. 25 to 1852, Jan. 12 ..	2909 days = 112 × 26 − 3 days.
1852, Jan. 12 to 1870, Jan. 20 ..	6583 days = 253 × 26 + 5 ,,
1844 to 1870....................	9492 days = 365 × 26 + 2 ,,

If, however, we assume the period to be $26\frac{1}{3}$ days, we should have

for 2909 days = 110 × $26\frac{1}{3}$ + 12 days.
6583 days = 250 × $26\frac{1}{3}$ + 0 ,,
9492 days = 360 × $26\frac{1}{3}$ + 12 ,,

* This is also the date obtained by Dr. Hornstein from the declination observations for 1870.

The results for 1844 and 1870 are, I think, very near the truth, and they confirm, within 2 days, the epoch with a period of 26 days; and whatever value may be given to the epoch for 1852 compared with that for 1844, will diminish the probability of the value $26\frac{1}{3}$. On the other hand, 1852 compared with 1870 satisfies exactly the period of $26\frac{1}{3}$ days; but it would also be satisfied equally well by a period of 26·02 days; so the interval is, in this case, too great to decide between the two values. The other results, however, seem to me sufficient, and, neglecting the long intervals, we have from

1844–45, period = 25·96 days.
1852–50, period = 25·79 „
1868–70, period = 25·90 „

and the mean 25·88 days is probably within a tenth of a day of the truth.

As far, then, as the existence of a period of near 26 days is concerned, I think there cannot be the slightest doubt; the examination of great masses of observations has confirmed my belief in it, as it has Dr. Hornstein's. But we know nothing certainly as to its cause: it appears to be most probably connected with the sun's rotation; but in what way this may act nothing is known. The single periods show great breaks, and what may be termed *accidental* minima, in opposition to the minima belonging to the period: these accidental minima are connected with great disturbances, probably allied to the solar eruptions or to causes which generally produce spots and protuberances. It is to these accidental minima that the smallness of the coefficient a in the term for 1845 is due. We might suppose that the sun during its rotation produces an action on the magnetic or electric ether in motion, which, as far as it acts on our magnets, may be supposed in greater quantity or more condensed in certain parts of the earth's orbit and in certain years; and, as has been supposed in the case of the frequency of the solar spots, this ether may also be acted on by the planets, and produce an irregularity in the length of a few successive periods. These suppositions are made merely to show that we are perhaps not in possession of all the conditions of the problem, without which perfect exactness in the calculations is impossible.

In conclusion, I refer again those interested in the subject to plate xxvii. in the Transactions of the Royal Society of Edinburgh, vol. xxii., where the daily means of horizontal force are projected for four stations on the earth's surface, all of which agree in showing the same movements, some of which have an amplitude of ·002 of the whole horizontal force (the Astronomer Royal's result for 1870 gives a *mean* value of nearly the half of this), and with intervals of about 26 days.

I hope to be able to confirm this result hereafter by ten years' observations at Trevandrum *.

* I regret that the papers containing my calculations for the first three years are not at present in my possession, or I should have stated the result here.

Postscript I. Received June 6, 1872.

In determining the interval between the maximum of January 1844 and that of January 1852, the epoch of maximum for 1844, deduced from the 26-day groups, was compared with that from the $26\frac{1}{3}$-day group in 1852. Either the latter should be corrected by $+\frac{1}{2}\cdot\frac{14}{3}$ day $= +2\frac{1}{3}$ days, or the epoch found for the $26\frac{1}{3}$-day group in 1844 (22·9 day instead of 25 days) should be taken. In either case the number of days intervening becomes 2911 days, which is only one day less than 112 periods of 26 days; or it is equivalent to 112 periods of 25·99 days.

If we try 113 periods, we obtain 25·76 days for the length of one, which is less than that by any of the one- and two-year comparisons. The longest interval, from January 1844 to January 1870, becomes (the above correction made) 9494 days, which number is equivalent to 365 periods of 26·01, or 366 periods of 25·94 days.

The most probable values for the moment appear to be

1844 to 1852 .... 25·99 days.
1852 to 1870 .... 26·02 „

Postscript II. Received July 4, 1872.

Since writing the preceding note, I have examined the observations of the bifilar magnetometer made at different stations on the earth's surface during the years 1842, 1843, 1846, and 1847, and found, as shown in my paper already cited for the years 1844 and 1845, that the variations of the daily mean horizontal force are the same at all stations at the same time: this I consider a test essential in the first instance before seeking from any observations other results which depend on the changes of mean values from day to day.

In the case of the observations for the years just mentioned, as in those for 1844 and 1845, great disturbances are indicated always by a great diminution of the mean magnetic force; these diminutions are frequently so great, that in the calculations for a single year they decide the epoch of minimum.

In my paper on the Horizontal Force of the Earth's Magnetism, I stated that "a careful investigation of a much larger series of observations leads me to believe that the period is variable within certain limits" *; whether this variation is due to a change of the solar meridian producing the maximum after certain intervals, or to superposed regular or irregular causes, I hoped, and hope yet, to determine.

If the variation is one about a mean value, then that value will be best determined by comparing the result of two well-marked groups of periods separated by an interval of several years, as has been done in the preceding

* Trans. Roy. Soc. Edin. vol. xxii. p. 544.

note. If, on the other hand, the sun's meridian producing the maximum varies, the mean period should be sought for each year by the trial of periods of different lengths, as has been done by Dr. Hornstein for Prague, and as I have done for Makerstoun.

III. "Contributions towards the History of the Ethylene Bases." By A. W. Hofmann, Ph.D., F.R.S., Professor of Chemistry in the University of Berlin. Received May 20, 1872.

The manufacture of chloral, which, since the discovery of the remarkable physiological properties of this compound, is conducted on a daily increasing scale, gives rise to a variety of secondary products which have not failed to attract the attention of chemists. Some time ago I showed that the most volatile fraction ot these by-products consists almost entirely of chloride of ethyl, constituting a very valuable material for the preparation of an abundant quantity of the ethylated ammonias. The fraction boiling between 70° and 100° is chiefly bichloride of ethylene, which, when submitted to the action of alcoholic ammonia at 100°, furnishes a supply of ethylene bases such as would be difficult to obtain from other sources. Dr. Schering, one of the principal manufacturers of chloral in Berlin, has lately placed at my disposal between 30 and 40 kilograms of these latter by-products, which the kindness of my friends Drs. Martius and Mendelssohn, by placing at my disposal one of their magnificent enamelled autoclaves, has permitted me to treat in one single operation with alcoholic ammonia.

The product of this operation was a large proportion of sal-ammoniac deposited in crystals, and a dark alcoholic mother-liquor which, after the alcohol had been distilled off, yielded on evaporation a brown crystalline residue, consisting of salts of ethylene bases. Large quantities of ethylenediamine chlorhydrate in a state of absolute purity were separated from this mixture by systematic crystallization. An additional portion was procured by distilling the mother-liquor, after it had ceased to crystallize, with an alkali, collecting apart the first products, as long as they yielded with chlorhydric acid the easily crystallizable ethylenediamine salt. In this manner more than a kilogram of the perfectly pure chlorhydrate was obtained, not to speak of quantities of the more complex bases, which I have not yet endeavoured to separate.

A new source of ethylenediamine having thus been opened, I was induced to resume the study of this remarkable substance, the history of which still presents a great number of gaps. I was more particularly anxious to ascertain whether some of the reactions, to which the ordinary alcohol bases have of late been found to lend themselves, might be with equal success applied to the ethylene bases.

1. Action of Carbon Bisulphide on Ethylenediamine.

Having repeatedly been engaged during the last few years with the study

of the mustard-oils, my attention was in the first place directed towards the production of the ethylene term of this class of compounds, and I was thus led to make some experiments on the behaviour of the diamine towards carbon bisulphide. Indeed, if the reaction between these two bodies ensued in a manner analogous to that which obtains in the case of carbon bisulphide and ethylamine, the formation of an ethylenediamine ethylene disulphocarbamate,

$$\left.\begin{matrix}(CS)_2\,(C_2H_4)\,H_2\,N_2\\(C_2H_4)\,H_4\,N_2,\,H_2\end{matrix}\right\}S_2,$$

was to be expected, which, under the influence of metallic salts, might have split up in ethylenediamine, sulphhydric acid, and ethylene mustard-oil,

$$\left.\begin{matrix}(CS)_2\,(C_2H_4)\,H_2\,N_2\\(C_2H_4)\,H_4\,N_2,\,H_2\end{matrix}\right\}S_2=2H_2S+(C_2H_4)\,H_4\,N_2+(CS)_2\,(C_2H_4)\,N_2.$$

Ethylenediamine sulphocarbonate.—If carbon bisulphide be mixed with a solution of ethylenediamine in alcohol, a clear liquid is obtained, which after a few moments becomes troubled by the separation of a white, almost amorphous compound. This substance rapidly augments, so that after the lapse of a quarter of an hour the liquid is converted into a white or pale yellow solid mass.

The new compound is nearly insoluble in alcohol and ether, and may therefore be readily purified by washing with these solvents. It is soluble in hot water, undergoing, however, at the same time slight decomposition; if care be taken not to heat the solution to the boiling-point, the compound separates on cooling in prismatic crystals. In the dry state also it is decomposed at a temperature of 100°, and must therefore for analysis be dried *in vacuo* or over sulphuric acid. Analysis shows that the substance is formed by the union of one molecule of ethylenediamine and one molecule of carbon bisulphide, its composition being expressed by the formula

$$C_3H_8N_2S_2=(C_2H_4)\,H_4\,N_2,\,CS_2.$$

The behaviour of ethylenediamine to carbon bisulphide is therefore so far analogous to that of ethylamine that we have, in both instances, a double molecule of ammonia uniting with one molecule of carbon bisulphide. The ethylenediamine derivative, however, is distinguished from that of ethylamine by its chemical characters; for whilst the latter is evidently the ethylamine salt of ethylsulphocarbamic acid, a similar view of the constitution of the former is untenable; for neither do dilute acids cause the separation from it of ethylene sulphocarbamic acid, nor do alkalies liberate ethylenediamine, reactions which would certainly take place if the new body were constituted like the ethylamine compound. Under these circumstances there was but little expectation of meeting amongst the products of its decomposition an ethylene mustard-oil having the composition of ethylene sulphocyanate described by Buff. In fact I may state at once that hitherto all my attempts to prepare such a compound have entirely failed.

Ethylene sulphocarbamide, ethylene urea.—When a solution of the bisulphide of carbon compound of ethylenediamine is boiled with a metallic salt (mercury chloride for example), sulphuretted hydrogen is eliminated, whilst at the same time a small quantity of carbon bisulphide distils, and there remains in solution a salt of ethylenediamine and the metallic compound of a new body still containing sulphur.

A similar change is produced by boiling with dilute acids, but in this instance much carbon bisulphide is evolved. Finally, the decomposition may also be effected by boiling with water; in this case scarcely any thing but sulphuretted hydrogen is given off, the product of the reaction consisting almost entirely of the above-mentioned new sulphur body, which can by this means be easily obtained in a pure state.

If the aqueous solution be allowed to cool, when sulphuretted hydrogen has ceased to be evolved, beautiful white prismatic crystals having an extremely bitter taste are deposited; they are soluble with difficulty in ether, but easily so in alcohol. Their melting-point is 194°; at a higher temperature they are decomposed. This substance still contains sulphur which cannot be removed even by boiling with lead oxide in an alkaline solution, and is only detected by fusion with potassium nitrate. Analysis of the body dried at 100° leads to the formula

$$C_3H_6N_2S = \left.\begin{matrix} CS \\ C_2H_4 \\ H_2 \end{matrix}\right\} N_2.$$

The new substance, which may be considered as *ethylene sulphocarbamide* or *ethylene sulpho-urea*, is thus seen to be formed from the carbon bisulphide compound by the elimination of one molecule of sulphhydric acid,

$$C_3H_8N_2S_2 = C_3H_6N_2S + H_2S.$$

The same body, as already mentioned, is produced by boiling the carbon bisulphide compound with metallic salts or with acids, the by-products ethylenediamine and carbon bisulphide, always formed in larger or smaller quantity, belonging to a secondary reaction, in which the original compound is resolved into its constituents. Ethylene sulphocarbamide crystallizes unchanged from its solution in acids, but combines with silver nitrate and mercury chloride, forming crystalline compounds. The mercury salt having been frequently prepared in the attempts to obtain the mustard-oil has been repeatedly analyzed. Probably several double salts exist; but under the conditions in which I have operated a compound of two molecules of the urea with three molecules of mercury chloride,

$$2C_3H_6N_2S, 3HgCl_2,$$

appeared to be the principal product.

Platinum chloride, even in very dilute solutions, gives rise to a bright yellow amorphous precipitate, which can be heated to 100° without undergoing decomposition. Its composition is

$$2C_3H_6N_2S, PtCl_4.$$

A second platinum-salt exists which only differs from that just mentioned by containing two molecules of hydrochloric acid. This salt was accidentally obtained in an attempt to decompose the ethylene sulpho-urea by sulphuric acid. The urea was heated with the concentrated acid until sulphurous acid began to be evolved; on adding platinum chloride to the solution after it had been diluted with water, a difficultly soluble platinum-salt was formed, crystallizing in beautiful long needles. At the first glance this appeared to be the platinum-salt of a new base, but on examination it was found to contain the original sulphur compound. It is represented by the formula

$$2(C_3H_6N_2S, HCl), PtCl_4.$$

On decomposing this platinum-salt by sulphuretted hydrogen a chlorhydric solution was formed, from which platinum chloride precipitated the original amorphous salt, and which on evaporation left the original ethylene sulpho-urea; this was identified by its bitter taste and by a determination of its melting-point. It is not easy to form an idea of the manner in which sulphuric acid acts in this case, as the needle-shaped platinum-salt is not produced in the presence of chlorhydric acid alone, even when very concentrated and in large excess.

On comparing the composition of ethylene sulphocarbamide with that of ethylenediamine on the one side and that of ethylene mustard-oil on the other, it will at once be seen that the new body occupies a position intermediate between the two:

$$\left.\begin{matrix} C_2H_4 \\ H_2 \\ H_2 \end{matrix}\right\} N_2 \qquad \left.\begin{matrix} C_2H_4 \\ CS \\ H_2 \end{matrix}\right\} N_2 \qquad \left.\begin{matrix} C_2H_4 \\ CS \\ CS \end{matrix}\right\} N_2.$$

The idea naturally suggested itself of procuring the mustard-oil by the further action of carbon bisulphide on ethylene sulphocarbamide; but although numerous experiments have been made in this direction under varied conditions, hitherto all these attempts have been without success. Digestion with carbon bisulphide alone or with carbon bisulphide and lead oxide produces no result; nor is the sulphur compound changed by treatment with potassium xanthate under pressure. In every respect the body exhibits remarkable stability; indeed all endeavours to remove the sulphur by the action of metallic oxides or of ammonia, even under pressure at a very high temperature, have hitherto failed.

An experiment may here still briefly be mentioned by which the formation of the body sought for was aimed at in another way. As is well known, ethylic mustard-oil may also be procured from diethylsulpho-urea when ethylamine is eliminated from this compound, and thus a further reaction appeared to present itself which might be made available for the production of ethylene mustard-oil. An ethylene sulpho-urea corresponding to four molecules of ammonia and having the formula

$$\left.\begin{matrix} (CS)_2'' \\ (C_2H_4)'' \\ H_6 \end{matrix}\right\} N_4$$

was not unlikely to be generated from ethylenediamine sulphocyanhydrate by atomic interchange within the molecule.

Ethylenediamine sulphocyanhydrate.—I first endeavoured to prepare this salt by decomposing the chlorhydrate with silver sulphocyanide, but no action takes place between the two salts even under pressure. It can, however, be readily obtained by saturating free sulphocyanhydric acid with ethylenediamine. The compound crystallizes in large transparent prisms which soon become opaque. The crystals, which are anhydrous, are exceedingly soluble in water, somewhat less so, but still very soluble, in alcohol, insoluble in ether. The formula

$$C_4H_{10}N_4S_2=(C_2H_4)H_4N_2, (HCNS)_2$$

was verified by the analysis of the substance dried at 100°.

On heating the sulphocyanhydrate, it immediately undergoes profound decomposition. Even below its melting-point, which lies at 145°, the salt becomes changed into the ethylene sulpho-urea already described, ammonium sulphocyanide being simultaneously separated.

$$(C_2H_4)H_4N_2, (HCNS)_2=(CS)(C_2H_4)H_2N_2+H_3N, HCNS.$$

This transformation is perfectly analogous to that which, as I pointed out many years ago*, aniline sulphocyanhydrate undergoes under the influence of heat when it is changed into sulphocarbanilide and ammonium sulphocyanide. No indication of the formation of a mustard-oil could be observed. I must not leave unmentioned that experiments made in my laboratory by M. Jul. Strakosch, with the view of preparing the mustard-oil of benzidine, have had no better results. New methods, then, must be sought for the production of the mustard-oils corresponding to diamines.

2. Ethylenediamides.

In order to complete the history of ethylenediamine I may here be allowed to append some further observations, partly new and partly old, which have not yet been published.

Action of benzoyl chloride on ethylenediamine.—The reaction, which is a very energetic one, takes place precisely in the manner indicated by theory. The hot liquid solidifies on cooling to a crystalline mixture of ethylenediamine chlorhydrate, and a new compound to which the name *ethylene dibenzoyldiamide* belongs. After washing away the ethylenediamine salt, the residue only requires to be crystallized once or twice from alcohol in order to obtain it in fine needles of perfect purity. These dissolve very sparingly in cold, more readily in boiling alcohol, but are insoluble in water. The composition of the new substance is

$$C_{16}H_{16}N_2O_2=\left.\begin{matrix}C_2H_4\\(C_7H_5O)_2\\H_2\end{matrix}\right\}N_2;$$

it is formed according to the equation

$$2[(C_2H_4)H_4N_2]+2C_7H_5OCl=(C_2H_4)(C_7H_5O)_2H_2N_2+(C_2H_4)H_4N_2, (HCl)_2.$$

* Ann. Chem. Pharm. vol. lxx. p. 143.

Action of chloral on ethylenediamine.—In order to become acquainted also with an ethylenediamide belonging to the series of fatty acids, I have prepared the formyl compound. The basic properties of ethylenediamine being, indeed, similar to those of potash and soda, it appeared appropriate to treat the base with chloral in order to produce ethylene diformyldiamide. Experiment has confirmed this anticipation; the two substances act on one another with great energy, torrents of chloroform being given off which may be readily condensed: on evaporating the residuary liquid on the water-bath, ethylene diformyldiamide remains behind as a transparent syrup.

$$(C_2H_4)H_4N_2+2[CCl_3 . CHO]=(C_2H_4)(CHO)_2H_2N_2+2CHCl_3.$$

Both acids and alkalies, especially on boiling, readily change this amide into ethylenediamine and formic acid. In passing I may mention that chloral may also be conveniently used for the preparation of other formamides. When anhydrous ethylamine and chloral are brought together, a white crystalline mass is immediately formed by the direct union of the two substances. This, on distillation, gives considerable quantities of pure ethylformamide boiling at 199°, chloroform being eliminated at the same time.

Formamide also may be obtained in this manner, but less advantageously.

Action of oxalic ether on ethylenediamine.—If a concentrated alcoholic solution of ethylenediamine be mixed with oxalic ether, the liquid becomes warm and solidifies after a few minutes to a white amorphous mass, which is almost insoluble both in water and in alcohol. On evaporating the alcoholic filtrate from the insoluble body on the water-bath, a white crystalline substance is left, which is soluble both in water and in alcohol.

The white amorphous substance is swollen up like starch-paste, and can only be washed with difficulty; moreover, as no solvent for it was to be found, it could not be further purified, a circumstance which was not without a slight influence on the analytical results. As might be expected, the amorphous body is ethylene oxamide,

$$C_4H_6N_2O_2=\left.\begin{matrix}C_2H_4\\C_2O_2\\H_2\end{matrix}\right\}N_2.$$

The substance crystallizing in white scales, which is obtained on evaporating the filtrate from ethylene oxamide, was found on analysis to be ethylic ethylene oxamate:

$$C_{10}H_{16}N_2O_6=\left.\begin{matrix}(C_2H_4)(C_2O_2)_2H_2N_2\\(C_2H_5)_2\end{matrix}\right\}O_2.$$

The action of ethylenediamine on oxalic ether is thus seen to be precisely what theoretically might be expected.

IV. "Colouring-matters derived from Aromatic Azodiamines." By A. W. HOFMANN, Ph.D., F.R.S., and A. GEYGER, Ph.D. Received June 12, 1872.

I. *Azodiphenyl Blue.*

About three years ago one of us* laid before the German Chemical Society the results of some experiments on the naphthaline dye commercially called Magdala Red. These experiments showed that the dye in question represents three molecules of naphthylamine which have lost three molecules of hydrogen, and that it is formed by the action of naphthylamine on azodinaphthyldiamine with separation of one molecule of ammonia.

$$\underset{\text{Azodinaphthyldiamine.}}{C_{20}H_{15}N_3} + \underset{\text{Naphthylamine.}}{C_{10}H_9N} = \underset{\text{Naphthaline red.}}{C_{30}H_{21}N_3} + H_3N.$$

The idea occurred to us of studying this reaction in other series and in other combinations. Preliminary experiments showed at once that aniline, as well as toluidine salts, readily act on azodinaphthyldiamine, forming perfectly similar red colouring-matters; and the question finally presented itself, whether to this group of dyes might not belong also the blue body which MM. Martius and Griess† produced by treating azodiphenyldiamine with salts of aniline, as mentioned in their interesting paper on Amidodiphenylimide, but which has not as yet been more minutely investigated.

The solution of this question by experiment appeared all the more desirable, since the composition of a compound likely thus to be formed would in all probability coincide with that of violaniline which MM. Girard, De Laire, and Chapoteaut‡ obtained by the oxidation of pure aniline. We have performed these experiments.

For the preparation of the blue colouring-matter derived from azodiphenyldiamine, which, for the sake of shortness, we will call *azodiphenyl blue*, equal weights of the pure azo base and aniline chlorhydrate with twice the weight of alcohol were heated in hermetically sealed tubes to 160° C. for four or five hours. After the lapse of this time a dark blue pasty mass had been formed; no gaseous compounds were produced in the reaction. For the purpose of purification, the crude product, which was at once recognized to be a chlorhydrate, was treated with boiling water in order to remove unaltered aniline salt and sal-ammoniac formed in the process; it was then dissolved in alcohol with addition of chlorhydric acid, and precipitated by soda. The base thus separated was carefully washed with water to free it from soda, and then dissolved in boiling alcohol; the liquid was then mixed with chlorhydric acid and submitted to distillation. As soon as half the alcohol had passed over, a dark

* Hofmann, 'Berichte,' 1869, pp. 374, 412.

† Monatsberichte der Berliner Akademie, 1865, p. 640.

‡ Comp. Rend. tom. lxiii. p. 964.

blue and slightly crystalline salt was found to be deposited on cooling. This salt is insoluble in water, but readily dissolves in alcohol, especially when heated. The solution is of a deep violet-blue colour, which is fixed by wool and silk, but cannot possibly be compared to the tints of the substituted rosanilines for brilliancy or beauty. The blue chlorhydrate is perfectly insoluble in ether. When soda is added to the concentrated alcoholic solution of the salt, the base is separated as a dark-brown powder which is insoluble in water, but dissolves in alcohol and ether with a red-brown colour. On addition of chlorhydric acid, the alcoholic solution assumes a pure blue tint; the ethereal solution, on the other hand, becomes perfectly colourless, the blue salt being precipitated. Treated with granulated zinc in presence of alcohol and chlorhydric acid, the solution of the salt is decolorized, but becomes blue again on exposure to the air. An attempt to prepare a leuko base in this manner failed; nor was an experiment made with ammonium sulphide more successful.

Analysis of the chlorhydrate just described, as well as of some other salts derived from it, showed that the reaction between azodiphenyldiamine and aniline indeed takes place, exactly as our knowledge of the analogous experiment in the naphthaline series had led us to expect. In forming the blue body one molecule of azodiphenyldiamine and one molecule of aniline unite with separation of one molecule of ammonia,

$$\underset{\text{Azodiphenyldiamine.}}{C_{12}H_{11}N_3} + C_6H_7N = \underset{\text{Azodiphenyl blue.}}{C_{18}H_{15}N_3} + H_3N.$$

Chlorhydrate.—This salt, the preparation of which we have described, has been repeatedly examined. Analyses of the compound dried at 100° C. led to the formula

$$C_{18}H_{15}N_3, HCl.$$

The salts of azodiphenyl blue show but little stability. The chlorhydrate just mentioned loses a portion of its acid even by recrystallization from alcohol. The salt with 11·28 per cent. chlorine, after a single recrystallization from alcohol, contained only 9·85 per cent. In fact the normal salt cannot be obtained except in presence of an excess of chlorhydric acid. When we tried to purify the crude product of the reaction by washing out the soluble salts and recrystallizing it without addition of acid, the percentage of chlorine fell in one case to 5·9, and in another even as low as 2·1 per cent. By merely drying at a higher temperature, the salt loses a portion of its acid. The normal salt, which for a considerable time had been dried at a temperature of 150° C., had lost nearly all its acid.

Iodhydrate.—This salt is obtained like the chlorhydrate, viz. by treating the free base with iodhydric acid. In its properties it scarcely differs from the chlorhydric salt. To establish the formula, we have limited ourselves to an estimation of the iodine in the salt dried at 100° C. The formula of the compound is

$$C_{18}H_{15}N_3, HI.$$

We have finally analyzed the

Picrate.—It is easily formed when the alcoholic mother-liquor of the chlorhydric salt of azodiphenyl blue is precipitated by an alcoholic solution of picric acid. The picrate is a blue powder perfectly insoluble in water and ether, and only very slightly soluble in boiling alcohol. For analysis, the precipitated salt was carefully washed with water and dried at 100° C. Its composition is represented by the formula

$$C_{24}H_{18}N_6O_7 = C_{18}H_{15}N_3, C_6H_3(NO_2)_3O.$$

As has been already stated in the beginning of this note, the colouring-matter which we call Azodiphenyl blue possesses exactly the same composition as violaniline, which MM. Girard, De Laire, and Chapoteaut have prepared by oxidizing pure aniline,

$$3C_6H_7N-3HH=C_{18}H_{15}N_3.$$

Are these two bodies identical? We had hoped to be able to decide this question by experiment; our researches have, however, for the present taken a different direction, so that the question remains an open one.

If instead of an aniline salt the chlorhydrates of toluidine and naphthylamine are allowed to act on azodiphenyldiamine, as might have been expected, blue dyes with properties perfectly similar to those of azodiphenyl blue are produced. It is extremely probable that these compounds are respectively

$$C_{19}H_{17}N_3 \text{ and } C_{22}H_{17}N_3.$$

These formulæ must, however, still be established by analysis.

The attempts here briefly sketched at producing new colouring-matters were capable of expansion in another direction. Indeed, instead of submitting azodiphenyldiamine to the action of toluidine and naphthylamine, the idea suggested itself of starting from azoditolyldiamine, which would then be treated with aniline and naphthylamine salts.

An azoditolyldiamine exactly corresponding to azodiphenyldiamine remains as yet unknown. In all his experiments Dr. Martius, who, as he informs us, has repeatedly attempted the preparation of this compound, has invariably obtained only the isomeric body diazoamidotoluol, which is decomposed on boiling. We have not been more fortunate in our efforts to produce the true analogue of azodiphenyldiamine. On passing a rapid current of nitrous acid through toluidine heated to fusion on a saturated solution of common salt, the liquid base after some time solidifies to a crystalline mass, which may be easily purified by washing with water and recrystallizing from alcohol. In this way well-formed dark lemon-coloured needles are obtained. The analysis of this body, dried *in vacuo*, shows that it certainly possesses the composition

$$C_{14}H_{15}N_3.$$

However, on boiling this substance with chlorhydric acid, it is readily decomposed to cressol and toluidine, giving off quantities of nitrogen,

$$C_{14}H_{15}N_3 + H_2O = C_7H_8O + C_7H_9N + 2N.$$

The body formed in our experiments is thus found to be *diazoamidotoluol* and not *azoditolyldiamine*, the latter, as before, still remaining to be discovered. Since, in consequence of the operations just mentioned, we had in our possession an appreciable quantity of the diazo body, we were not willing to pass by this opportunity of testing, however cursorily, its action on aromatic monamines. Dyes are in fact produced by treating diazoamidotoluol with the chlorhydrates of aniline and naphthylamine in alcoholic solution at 150° C. The numerous by-products, however, which are formed in these processes sufficiently show that the reaction is by no means a simple one. It is more particularly worthy of remark that in its reaction with aniline salts no trace of the easily recognizable rosaniline was formed, which might have been the case if the true azoditolyldiamine could have been employed. We have not further studied the products generated in these reactions.

V. "On the present amount of Westerly Magnetic Declination [Variation of the Compass] on the Coasts of Great Britain, and its Annual Changes." By Staff Captain FREDERICK J. EVANS, R.N., F.R.S., Hydrographic Department of the Admiralty (in charge of Magnetic Department). Received June 15, 1872.

(Abstract.)

The rapidly accelerating value within the last few years of the annual decrease of the westerly magnetic declination over the whole area of the United Kingdom and the adjacent seas, as observed at the fixed magnetic observatories of Greenwich, Kew, Brussels, Paris, and also at Christiania in Norway, is a subject of importance in practical navigation as affecting the compass-bearings derived from charts and those laid down for the guidance of pilots.

The attention of the Hydrographic Department of the Admiralty has been constantly directed to this interesting physical fact; and as the duties of Her Majesty's surveying-vessels employed on our shores between the years 1866 and 1870 embraced nearly the whole extent of coast-line, advantage was thus taken, under the orders of Rear-Admiral Richards, C.B., F.R.S., the Hydrographer, to determine, with great attention to accuracy, the magnetic declination at widely spread and favourable localities.

The observations thus made by the surveying officers of Her Majesty's Navy are given in detail, with the corrections for secular change, to the 1st January 1872*, for which epoch a chart of the British Islands, exhibiting the lines of magnetic declination of equal value, is also appended. By comparing these lines with the corresponding lines given in the Declination chart for 1842·5, Phil. Trans. for 1870, art. xiv., "Contributions to

* A mean value of 19° 40′ being assumed for the westerly magnetic declination at Greenwich Observatory for this epoch.

Terrestrial Magnetism, No. XII.," by General Sir Edward Sabine, K.C.B., and President of the Royal Society, the annual decrease of the westerly declination, in the interval 29·5 years, over various geographical districts is thus shown:—

Shetland Islands and N.E coast of Scotland, between 60th and 56th parallels	′ 8·24
East coast of England, between 56th and 51st parallels	7·78
South coast of England, between 51st and 49th parallels.. Dungeness to Scilly Islands with the Channel Islands....	7·34
(*Greenwich Observatory*	7·14)
Irish Channel, between 52nd and 54th parallels	7·10
Hebrides and west coast of Scotland, between 56th and 58th parallels	6·85
Ireland, S.W., West, and N.W. coasts, between 52nd and 55th parallels	6·26

It is thus seen that in the area included by the shores of the United Kingdom the change was greater on the eastern than on the western side; as also that in the higher parallels of latitude of this area the change was greater than in the lower parallels.

By a further comparison of results as observed within the last ten to twelve years, at the same stations within the same geographical districts, the following approximate values of the present rate of annual change (westerly declination decreasing) are obtained:—

Shetland Islands and N.E. coast of Scotland	′ 11·2
East coast of England (Bridlington)	10·3
South coast of England (Plymouth)	7·9
Scotland, W. and N.W. coasts	9·5
Ireland, S.W. coast	6·6

These values are in satisfactory accordance with those obtained in the interval (1865–71) at the following fixed magnetic observatories:—

Greenwich..	′ 8·33	mean annual decrease of westerly declination.
Kew.... ..	8·08	,, ,, ,, ,,
Stonyhurst	7·85	,, ,, ,, ,,

VI. "Notice of further Researches among the Plants of the Coal-measures." By Prof. W. C. Williamson, F.R.S. Received June 1, 1872.

Fallowfield, May 3, 1872.

My dear Dr. Sharpey,—In my memoir on Calamites, published in the last volume of the 'Philosophical Transactions,' I gave two figures of sections of a plant (plate 25. fig. 16 and plate 28. fig. 39) supposed to be a Calamite, but respecting the Calamitean nature of which I expressed my doubts in a

note at the foot of page 488. I have now got numerous examples of this plant, and it proves, as I surmised, to belong to a distinct type. It has a branching stem, *not* jointed, and having a remarkable pith. Since the latter organ, when divided transversely, gives a star-shaped section, closely resembling that of a Calamite, except that it has not been fistular, I propose to give to the plant the generic name of *Astromyelon*. I have further examined a series of curious stems which I described briefly at the Edinburgh Meeting of the British Association under the name of *Dictyoxylon radicans*; this plant I also find must be placed in a new genus. It is characterized by possessing an exogenous, woody, branching stem, composed of reticulated vessels. It has no pith, and its bark consists of cells arranged in columns perpendicular to its surface. I think it not improbable that this has been the subterranean axis of some other plant, since I have succeeded in tracing its ultimate subdivisions into rootlets. I propose for the present to recognize it by the generic name of *Amyelon*. My specimens of this plant are very numerous, some of them having been kindly supplied to me by Messrs. Butterworth and Whittaker, of Oldham. They may prove to be rhizomes and roots of the Asterophyllite described in my last letter to you.

Of this last genus I have just got an additional number of exquisite examples, *showing not only the nodes but verticils of the linear leaves so characteristic of the plant*. These specimens place the correctness of my previous inference beyond all possibility of doubt, and finally settle the point that *Asterophyllites* is *not* the branch and foliage of a Calamite, but an altogether distinct type of vegetation having an internal organization peculiarly its own. This organization is identical in every essential point with that of my *Volkmannia Dawsoni* already referred to in my previous letter, and which I do not now hesitate to designate *Asterophyllites Dawsoni*. The peculiar triquetrous form of the young vascular axis of this genus is too remarkable and too distinct from that of all other Carboniferous types to be mistaken for any of them, and especially for that of Calamites, with which it has not one single feature of real affinity.

I have also obtained, partly through the assistance of Messrs. Butterworth and Whittaker, but especially the latter, an instructive series of specimens of the genus *Zygopteris*, which has recently been made the subject of an important memoir by M. B. Renault, published in tome xii. of the 'Annales des Sciences Naturelles.' Our Lancashire specimens are of the type which he describes under the name of *Z. Lacattii*. The French *savant* has found these plants, in one instance, connected as petioles to a rhizome, which he believes to be that of a fern. Our specimens supply some information additional to that published by M. Renault; they appear to me to sustain his idea that they are petioles, and I have traced in them the origin of the two vascular bundles which he refers to as pores existing in the bark. I find much reason for concluding that they are, as he surmises, the vessels going to the secondary rachis of the pinnules. Our

Lancashire specimens are covered with sparse, but very distinct hairs, that, unlike the ramentaceous form common amongst ferns, are perfectly cylindrical. Whilst I am thus inclined to express my conviction that M. Renault is correct in his views respecting *Zygopteris*, I find it increasingly difficult to distinguish fragments of ferns from those of Lycopods, as also fragments of petioles from those of roots.

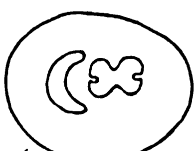
Mr. Nield and Mr. Whittaker, of Oldham, have just supplied me with two magnificent stems of Calamites of large size. The pith is absent from both, except some slight traces at the node of one of the specimens. I find on dissecting these matured stems that the remarkable arrangements of the vascular structure seen in plate 23. figure 2 of my memoir on Calamites almost entirely disappear in the more external of the exogenous growths. The conspicuous vertical laminæ of cellular parenchyma (my primary medullary rays), which separate the woody wedges, rapidly diminish in size as they proceed from within outwards, becoming more or less like the *secondary* or ordinary medullary rays represented in my fig. 5. Many of them, however, retain the evidence of their primary medullary origin in their unusual length, and by consisting of two, or even three, vertical series of cells instead of one, as is usual with the secondary rays. The vessels pursue their longitudinal course across the node undeflected in any direction, save where they bend aside to allow the passage outwards of vascular bundles going off to the aërial branches*, as represented in my figures 13 and 38. Thus in the exterior parts of these large stems the ligneous zone exhibits little or no indication of the presence of a node, except what these divergent bundles afford. I find that these bundles slightly increase in size as they proceed from within outwards, showing that they share in the exogenous additions made to the exterior of the ligneous zone; in one of my stems that zone has a circumference of seven inches, and the other of six and three quarters. It is in the former one that I find the nodal bundles; but I have not seen one of these organs whose actual diameter exceeds three sixteenths of an inch, confirming my previous statements respecting the comparatively small size of the aërial branches. As in my previously described examples, these bases of branches exhibit no separation of the vessels into a circle of wedges like those of the parent stem. The persistent growth of the vascular bundles just described seems to indicate more permanent relations between them and the central stem than I once thought probable. There appears to be a close approximation to uniformity in the number of the woody wedges of these large stems; one of mine contains 85, and the other 83 such. Mr. Binney counted 73 in his large specimen (*loc. cit.* pl. 2. fig. 1). In the thin, young, woody cylinder represented in my fig. 19, the mean diameter of which was slightly over an inch, the number was also about 80. This close resemblance between stems so different in age and size again illustrates another of my previous statements, viz. that age

* This condition is very correctly represented in plate 3. fig. 3 of Mr. Binney's memoir on Calamites (Palæont. Soc.).

produces no increase in the number of the woody wedges, but that each one of the latter enlarges by successive additions to its peripheral portions of new laminæ, which latter partly fill up the increasing area of the enlarging circle, and partly encroach upon the primary medullary rays, as represented in my figure 17, in addition to some interstitial growth.

We thus learn that as the ligneous cylinder of a Calamite increased in age and size it gradually exhibited less and less of the Calamitean peculiarities seen in young stems; its external portions assumed a generalized, unsulcated form, which recurs with remarkable uniformity in several otherwise different plants of the Coal-measures.

Amongst the Burntisland fossils sent to me by Mr. Grieve, I find two very curious stems, probably of the same general nature as *Zygopteris*. Both have a dense outer cortical layer, with vascular bundles in the interior. In the simpler of these plants the transverse section of this bundle is crescentic; but in the concave border of the crescent are two small projecting capes dividing it into three minor bays (fig. 2). In the other the vascular axis is a double one, lodged in a somewhat elliptical stem: one of these is a simple crescent, the concavity of which is directed inwards; the other has a very elegant transverse section (fig. 1). It is shaped like a dumb-bell, one head of which rests within the concavity of the crescentic bundle, and the other turns in the opposite direction; at each of these two extremities the margin of the dumb-bell is excavated into a small bay, as if a vertical canal had existed at each point; but these seem to have been merely columns of cellular tissue encroaching upon the rounded outline of the vascular structures. I propose provisionally to recognize these two forms under the generic name of *Arpexylon*.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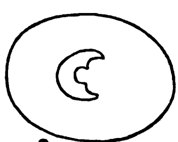
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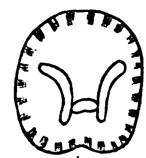
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Fig. 1. *Arpexylon duplex*. Fig. 2. *Arpexylon simplex*. Fig. 3. *Edraxylon*.

Fig. 3 represents a stem or petiole in which the section of the vascular bundle presents the form of a chair or seat, and to which I propose to assign the name *Edraxylon*. This form exhibits numerous modifications of the pattern represented in the outline down to a single central vascular bundle. It may prove to belong to *Dictyoxylon Oldhamium*.

VII. "Volcanic Energy: an attempt to develope its true Origin and Cosmical Relations." By Robert Mallet, F.R.S. Received May 13, 1872.

(Abstract.)

The author passes in brief review the principal theories which in modern times have been proposed to account for volcanic activity.

The chemical theory, which owed its partial acceptance chiefly to the fame of Davy, may be dismissed, as all known facts tend to show that the chemical energies of the materials of our globe were almost wholly exhausted prior to the consolidation of its surface.

The mechanical theory, which finds in a nucleus still in a state of liquid fusion a store of heat and of lava &c., is only tenable on the admission of a *very thin* solid crust; and even through a crust of but 30 miles thick it is difficult to see how surface-water is to gain access to the fused nucleus, yet without water there can be no volcano. More recent investigation on the part of mathematicians has been supposed to prove that the earth's crust is not *thin*. Attaching little value to the calculations as to this based on precession, the author yet concludes, on other grounds, that the solid crust is probably of great thickness, and that, although there is evidence of a nucleus much hotter than the crust, there is no certainty that any part of it remains liquid; but if so, it is in any case too deep to render it conceivable that surface-water should make its way down to it. The results of geological speculation and of physico-mathematical reasoning thus oppose each other, so that some source of volcanic heat closer to the surface remains to be sought. The hypothesis to supply this, proposed by Hopkins and adopted by some, viz. of isolated subterranean lakes of liquid matter in fusion at no great depth from the surface remaining fused for ages, surrounded by colder and solid rock, and with (by hypothesis) access of surface-water, the author views as feeble and unsustainable.

A source, then, for volcanic heat remains still to be found; and if found under conditions admitting to it water, especially of the sea, all known phenomena of volcanic action on our earth's surface are explicable.

The author points out various relations and points of connexion between volcanic phenomena, seismic phenomena, and the lines of mountain elevation, which sufficiently indicate that they are all due to the play of one set of cosmical forces, though different in degree of energy, which has been constantly decaying with time.

He traces the ways in which the contraction of our globe has been met, from the period of its original fluidity to the present state: first, by deformation of the spheroid, forming generally the ocean-basins and the land; afterwards by the foldings over and elevations of the thickened crust into mountain-ranges &c.; and, lastly, by the mechanism, which he points out as giving rise to volcanic action. The theory of mountain-elevation proposed by C. Prevost was the only true one—that which ascribes this to tangential pressures propagated through a solid crust of sufficient thickness to transmit them, those pressures being produced by the relative rate of contraction of the nucleus and of the crust; the former being at the higher temperature, and having a higher coefficient of contraction for equal loss of heat, tends to shrink away from beneath the crust, leaving the latter partially unsupported. This, which during a much more rapid rate of

cooling from higher temperature of the whole globe and from a thinner crust gave rise in former epochs to mountain-elevation, in the present state of things gives rise to volcanic heat. By the application of a theorem of Lagrange, the author proves that the earth's solid crust, however great may be its thickness, and even if of materials far more cohesive and rigid than those of which we must suppose it to consist, must, if even to a very small extent left unsupported by the shrinking away of the nucleus, crush up in places by its own gravity and by the attraction of the nucleus.

This is actually going on, and in this partial crushing, at places or depths dependent on the material, and on conditions pointed out, the author discovers the true cause of volcanic heat. As the solid crust sinks together to follow down after the shrinking nucleus, the *work* expended in mutual crushing and dislocation of its parts is *transformed into heat*, by which, at the places where the crushing sufficiently takes place, the material of the rock so crushed and of that adjacent to it are heated, even to fusion. The access of water to such points determines volcanic eruption. Volcanic heat, therefore, is one result of the secular cooling of a teraqueous globe subject to gravitation, and needs no strange or gratuitous hypothesis as to its origin.

In order to test the validity of this view by contact with known facts, the author gives in detail two important series of experiments completed by him:—the one on the actual amount of heat capable of being developed by the crushing of sixteen different species of rocks, chosen so as to be representative of the whole series of known rock formations from Oolites down to the hardest crystalline rocks; the other, on the coefficients of total contraction between fusion and solidification at existing mean temperature of the atmosphere of basic and acid slags, analogous to melted rocks.

The latter experiments were conducted on a very large scale, and the author points out the great errors of preceding experimenters, Bischoff and others, as to these coefficients.

By the aid of these experimental data, he is enabled to test the theory produced when compared with such facts as we possess as to the rate of present cooling of our globe, and the total annual amount of volcanic action taking place upon its surface and within its crust.

He shows, by estimates which allow an ample margin to the best data we possess as to the total annual vulcanicity of all sorts of our globe at present, that less than one fourth of the total heat at present annually lost by our globe is upon his theory sufficient to account for it; so that the secular cooling, small as it is, now going on is a sufficient *primum mobile*, leaving the greater portion still to be dissipated by radiation. The author then brings his views into contact with various known facts of vulcanology and seismology, showing their accordance.

He also shows that to the heat developed by partial tangential thrusts within the solid crust are due those perturbations of hypogeal increment of

temperature which Hopkins has shown cannot be referred to a cooling nucleus and to differences of conductivity alone. He further shows that this view of the origin of volcanic heat is independent of any particular thickness being assigned to the earth's solid crust, or to whether there be at present a liquid fused nucleus, all that is necessary being a *hotter* nucleus than crust, so that the rate of contraction is greater for the former than the latter. The author then points out that, as the same play of tangential pressures has elevated the mountain-chains in past epochs, the nature of the forces employed sets a limit to the height of mountain possible of the materials of our globe.

That volcanic action due to the same class of forces was more energetic in past time, and is not a uniform but a decaying energy now. Lastly, he brings his views into relation with vulcanicity produced in like manner in other planets, or in our own satellite, and shows that it supplies an adequate solution of the singular and so far unexplained fact that the elevations upon our moon's surface, and the evidences of former volcanic activity, are upon a scale so vast when compared with those upon our globe.

Finally, he submits that if his view will account for all the known facts, leaving none inexplicable, and presenting no irreconcilable conditions or necessary deductions, then it should be accepted as a true picture of nature.

VIII. "On some Properties of Anhydrous Liquefied Ammonia." By G. Gore, F.R.S. Received May 15, 1872.

(Abstract.)

This investigation was made for the purpose of ascertaining the general solvent properties of the liquid, and to detect any manifest chemical reactions between it and various substances. The method employed was precisely similar to that used in the examination of liquid cyanogen (see Proc. Roy. Soc. No. 131, 1871), the tubes being charged with anhydrous chloride of calcium previously saturated with the ammonia vapour.

Two hundred and fifty substances were submitted to contact with the liquid, and the general results in each case recorded. The only elementary substances soluble in it were the alkali-metals proper, also iodine (bromine was not tried), sulphur, and phosphorus. The more frequently soluble inorganic salts were nitrates, chlorides, bromides, and iodides; whilst oxides, fluorides, carbonates, sulphides, and sulphates were very generally insoluble. Many saline substances, especially certain chlorides, bromides, iodides, and sulphates, absorbed ammonia freely, and swelled greatly, but did not dissolve. The behaviour of the chlorides of mercury was peculiar.

Various compounds of carbon were submitted to the action of the solution of potassium in the liquefied vapour; the free potassium disappeared, but no elementary carbon was liberated.

IX. "On the Physical Nature of the Coagulation of the Blood." By ALFRED HUTCHISON SMEE, F.C.S., F.S.S. Communicated by ALFRED SMEE, F.R.S. Received May 27, 1872.

(Abstract.)

After passing in review the principal opinions hitherto entertained on the real nature of the coagulation of the blood, the author states that he is led to refer the solidification of fibrin in the process of coagulation to the same class of phenomena as the pectization of colloid liquids, as already hinted by the late Professor Graham and others. He points out at considerable length the chief circumstances which influence the change in colloidal fluids, and proceeds to compare the properties of colloid silica with an organic body of the nature of fibrin.

X. "On the Detection of Organic and other Nitrogenized Matter existing in the Atmosphere." By ALFRED HUTCHISON SMEE, F.C.S., F.S.S. Communicated by ALFRED SMEE, F.R.S. Received June 1, 1872.

(Abstract.)

In this communication the author describes a method which he has devised, and which he names "distillation by cold," by which he believes the detection and determination of ammonia and other organic impurities existing in the atmosphere will be greatly facilitated.

A glass funnel (usually of 8 or 9 inches) is drawn to a point and closed. It is supported in an ordinary stand, and filled with ice. Condensation of the watery vapour of the atmosphere then takes place; the dew collects into drops, which trickle down the outside of the funnel, and at last fall from the point, under which a small receiver is placed to catch them. The total quantity of liquid collected in a given time is measured, and the quantity of ammonia determined by Nessler's test.

By the method of distillation by cold, the author found it possible to distil many substances which are decomposed at a high temperature. Thus many delicate odours of flowers were distilled by placing the flowers under a bell-glass sufficiently large to cover the funnel containing the ice. The odours were found to be more rapidly and completely abstracted by placing a dish with a little ether under the bell-glass at the time of distillation.

The paper was accompanied by Tables giving the results obtained in 107 experiments, together with the atmospheric conditions prevailing at the time. The experiments were made in a garden, in a bed-room, in hospital wards, in the open country, &c. A few of the numbers obtained are here given by way of example:—

Fluid collected in minims.	Ammonia in grains per gallon.	Source.
150	1·9712	Erysipelas.
120	·1791	Garden.
55	6·8807	Drains.
90	2·1000	Bed-room.
420	2·9568	Stables.
150	·0985	Victoria Park.

XI. "Contributions to Terrestrial Magnetism.—No. XIII." By General Sir EDWARD SABINE, K.C.B., V.P.R.S. Received June 19, 1872.

(Abstract.)

The author presents this paper as the companion of No. XI. of his Contributions to Terrestrial Magnetism, which contained the Magnetic Survey of the Southern Hemisphere from 40° south latitude to the extreme limit towards the southern pole, as does the present memoir, No. XIII. of the same series, the three magnetic elements from 40° north latitude to the furthest attained limit towards the northern pole. In both papers the mean epoch is the same, viz. 1842·5. Where it has been possible to do so, corrections to this mean epoch have been obtained and applied to earlier and later observations.

The determinations are derived from observers of all countries, and are arranged in zones, each of 5° of latitude, passing round the globe. The Table thus formed contains between 3000 and 4000 stations at which the magnetic elements have been determined. The observers are named, and references are made to the sources from whence their observations are taken. The paper is accompanied by maps of the resulting Isogonic, Isoclinal, and Isodynamic Lines, executed at the Hydrographic Office.

XII. "On the Law of Extraordinary Refraction in Iceland Spar." By G. G. STOKES, M.A., Sec. R.S. Received June 20, 1872.

It is now some years since I carried out, in the case of Iceland spar, the method of examination of the law of refraction which I described in my report on Double Refraction, published in the Report of the British Association for the year 1862, p. 272. A prism, approximately right-angled isosceles, was cut in such a direction as to admit of scrutiny, across the two acute angles, in directions of the wave-normal within the crystal comprising respectively inclinations of 90° and 45° to the axis. The directions of the cut faces were referred by reflection to the cleavage-planes, and thereby to the axis. The light observed was the bright D of a soda-flame.

The result obtained was, that Huygens's construction gives the true law

of double refraction within the limits of errors of observation. The error, if any, could hardly exceed a unit in the *fourth* place of decimals of the index or reciprocal of the wave-velocity, the velocity in air being taken as unity. This result is sufficient *absolutely to disprove* the law resulting from the theory which makes double refraction depend on a difference of inertia in different directions.

I intend to present to the Royal Society a detailed account of the observations; but in the mean time the publication of this preliminary notice of the result obtained may possibly be useful to those engaged in the theory of double refraction.

XIII. "On a Voltaic Standard of Electromotive Force." By Latimer Clark, M.I.C.E. Communicated by Prof. Sir William Thomson, F.R.S. Received May 30, 1872.

(Abstract.)

In the year 1861 a Committee was appointed by the British Association for the Advancement of Science to report on standards of electrical resistance, and subsequently on other standards of electrical measurements. Reports were presented in 1862, 1863, 1864, 1865, and 1867.

They recommended the adoption of a system of electromagnetic units based on the metre and gramme, the relations of the units being such that the unit of electromotive force acting through the unit resistance should give the unit current, and that the unit current flowing for the unit time should give the unit quantity.

They issued standards of resistance (known as the B. A. unit or ohm) and standards of electrostatic capacity, or condensers of such magnitude that when charged with the unit electromotive force they contained a submultiple of the unit quantity of electricity (known as the farad).

No material standard of electromotive force has yet been issued. Much difficulty has, in fact, been found in devising such a standard. Mechanical means, such as the rotation of a conductor in a magnetic field of known intensity, are too complicated for ordinary use; thermoelectric couples are extremely variable, and voltaic elements, which would constitute the most convenient form of standard, have been hitherto found singularly inconstant, and therefore inapplicable. The Daniell's element, which has been most frequently used for this purpose, commonly varies five per cent. or more without apparent cause.

From a conviction that if similar conditions could be ensured similar combinations would always give the same electromotive force, the author was led to institute a series of experiments, extending over four years, which led to the discovery of a form of battery that is sensibly constant and uniform in its electromotive force.

The battery is composed of pure mercury as the negative element, the mercury being covered by a paste made by boiling mercurous sulphate in

a thoroughly saturated solution of zinc sulphate, the positive element consisting of pure zinc resting on the paste. The best method of forming this element is to dissolve pure zinc sulphate to saturation in boiling distilled water. When cool, the solution is poured off from the crystals and mixed to a thick paste with pure mercurous sulphate, which is again boiled to drive off any air; this paste is then poured on to the surface of the mercury previously heated in a suitable glass cell; a piece of pure zinc is then suspended in the paste, and the vessel may be advantageously sealed up with melted paraffine-wax. Contact with the mercury may be made by means of a platinum wire passing down a glass tube, cemented to the inside of the cell, and dipping below the surface of the mercury, or more conveniently by a small external glass tube blown on to the cell, and opening into it close to the bottom. The mercurous sulphate ($Hg_2 SO_4$) can be obtained commercially *; but it may be prepared by dissolving pure mercury in excess in hot sulphuric acid at a temperature below the boiling-point: the salt, which is a nearly insoluble white powder, should be well washed in distilled water, and care should be taken to obtain it free from the mercuric sulphate (persulphate), the presence of which may be known by the salt turning yellowish on the addition of water.

The electromotive force of the elements thus formed is remarkably uniform and constant, provided the elements be not connected up and allowed to become weak by working. A long series of comparisons was made between various elements, some of which had been made many months, and it was found that the greatest variation among them all did not differ from the mean value more than one thousandth part of the whole electromotive force; such a large difference was, however, very unusual, and might have been due to slight differences of temperature.

Several experiments were made to determine the variation of the electromotive force produced by temperature, from the mean of which it appears that the electromotive force decreases with increased temperature in the ratio of about ·06 per cent. for each degree Centigrade; for example, an element gave relative values of ·9993 at 0° Cent. and ·9412 at 100° C., between which limits the decrease appeared nearly proportional to the increments of temperature. These results, however, might be verified with advantage.

The element is not intended for the production of currents, for it falls immediately in force if allowed to work on short circuit. It is intended to be used only as a standard of electromotive force with which other elements can be compared by the use of the electrometer or condenser, or other means not requiring the use of a prolonged current. The author finds that the most delicate method of making these measurements is by means of his potentiometer†.

* The author has obtained it from Messrs. Hopkin and Williams, 5 New Cavendish Street.

† See 'A Treatise on Electrical Measurement,' by Latimer Clark. London, 1868, p. 106.

As it was desirable to determine the value of the force of the element in absolute measure and in terms of the British-Association units, a very careful series of measurements was made by the electrodynamometer constructed for the British-Association Committee, and referred to in their Report for 1867, and also by means of a sine galvanometer of somewhat novel form.

The following Tables give the results obtained:—

I. By the Electrodynamometer.

Date.	Value of E in volts.	Remarks.
1871.		
Dec. 8	1·4573	3 cells.
9	1·4651	3 cells.
14	1·4616	3 cells.
15	1·4561	3 cells.
15	1·4579	2 cells.
16	1·4586	3 cells.
16	1·4517	3 cells, coil turned 180°.
16	1·4552	2 cells, coil turned back 180°.
16	1·4555	3 cells.
16	1·4535	2 cells.
16	1·4564	3 cells.
18	1·4649	3 cells.
19	1·4562	3 cells, coil turned 180°.
19	1·4558	3 cells, coil turned back 180°.
20	1·4615	3 cells.
20	1·4539	3 cells.
20	1·4551	2 cells.
21	1·4549	3 cells.
Mean	1·45735	Temperature 15°·5 Cent.

II. By the Sine Galvanometer.

Date.	Value of H.	Value of E.	Remarks.
1872.			
Feb. 9	1·788	1·45605	Galvanometer wound with 8 turns of German-silver wire.
9	1·788	1·45457	
9	1·788	1·45400	
10	1·788	1·45809	
10	1·788	1·45669	
11	1·788	1·45799	
18	1·787	1·45566	Rewound with 28 turns of German-silver wire.
19	1·787	1·45671	
19	1·787	1·45680	Rewound with 27 turns of German-silver wire.
20	1·787	1·45752	
20	1·787	1·45645	
24	1·786	1·45522	
24	1·786	1·45492	
	Mean......	1·45621	Temperature 15°·5 Cent.

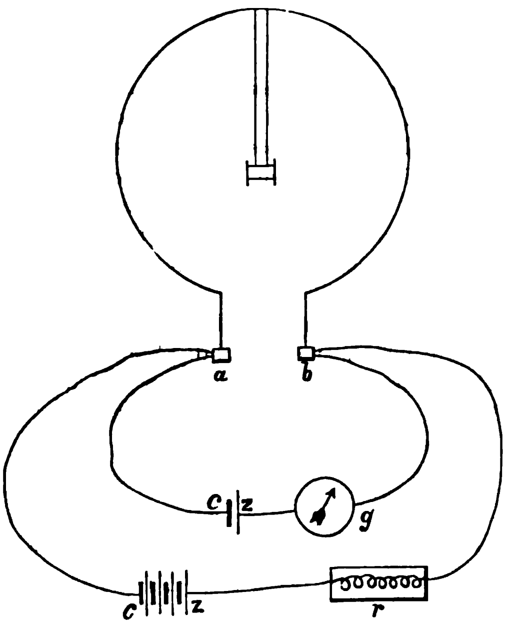
We have therefore the mean value of the electromotive force of the standard-cells, as determined by the electrodynamometer, 18 observations	Volt. 1·45735
As determined by the sine galvanometer, 13 observations..	1·45621
Mean value........	1·45678

Or, since no importance can be attached to the figures beyond the third place of decimals, 1·457 volt or British-Association unit of electromotive force, equal to 145700 absolute electromagnetic units.

The value of H, the horizontal component of the earth's magnetic intensity, a knowledge of which is necessary for the determination by the sine galvanometer, was kindly supplied for each day by the Astronomer Royal.

A novel feature in both these series of determinations is the use of an arrangement by which the cells under comparison are not allowed to perform any work or produce any current.

The arrangement is shown in the subjoined diagram.



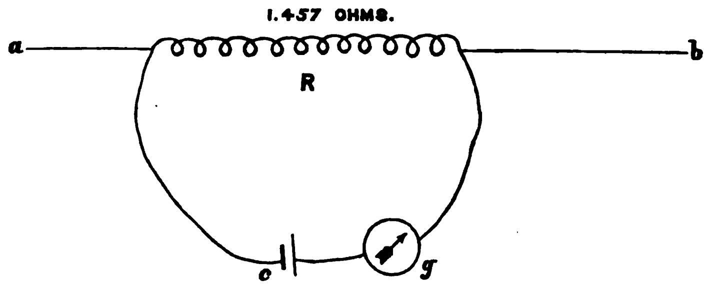
a and *b* are the terminals of the instrument; the standard cell (C Z) is connected to these terminals with an intervening galvanometer, *g*; an auxiliary battery (*c z*) is also connected with similar poles to the same terminals, so that both tend to send a current through the instrument in the same direction.

The strength of the auxiliary battery is, however, so regulated, by means of the rheostat (*r*) and by varying the number of cells, that it just balances the force of the standard cell, so that no current flows through the galvanometer (*g*), or, in other words, the terminals (*a*, *b*) are kept at a difference of potential equal to the electromotive force of the standard (C Z), the current which flows through the instrument being entirely supplied by the auxiliary battery.

This method has also the advantage of being quite independent of the resistance of the standard cell.

The uses of this standard element to practical electricians are sufficiently obvious. It may be used for determining the electromotive force of other elements by the use of an electrometer or by the discharge from a condenser. Or a condenser having a capacity of $\frac{1}{1 \cdot 457}$ farad charged by the standard cell would contain the British-Association unit quantity of electricity (one veber), or $\frac{1}{10}$ of the absolute unit of quantity.

It is also of great value for maintaining a current of known strength in any circuit for the purposes of experimental research.



Thus, if it be desired to produce in any circuit, *a b*, a current equal to the British-Association current ($\frac{1}{10}$ absolute unit), it is only necessary to insert in the circuit a wire (R) having a resistance of 1·457 ohm, and to connect to each end of this wire the poles of a standard cell (*c*) with a galvanometer (*g*), and to vary the strength of the current in *a b* until no deflection is produced on the galvanometer; the current through *a b* will then be equal to one British-Association unit of current, or one veber per second, whatever its length or resistance.

By varying the resistance of R, or by varying the number of elements (*c*), any given current can be steadily maintained through *a b* at pleasure; on the other hand, the value of any given current can be measured by so varying the resistance R that no deflection is produced on the galvanometer. The value of the passing current will then be

$$C = \frac{1 \cdot 457}{R} \text{ veber per second.}$$

It is also evident that, knowing the value of E, we may determine the horizontal intensity of the earth's magnetism (H) in any place quickly and simply by means of an ordinary sine or tangent galvanometer.

In fact, the standard of electric potential is second only in importance to that of the standard of electric resistance; and the use of such a standard, combined with an auxiliary battery in the manner above described, admits of a variety of applications which it is believed will be found of great value in electrical research.

XIV. "Pyrology, or Fire Analysis." By Capt. W. A. Ross, R. A. Communicated by Prof. Stokes, Sec. R. S. Received May 29, 1872.

1. Pyrology, as distinguished from ordinary blowpipe manipulation, may be described as the art of inducing chemical changes in substances from which their *composition** can be concluded, by the scientific application to them of fire, and the use of *acid* as well as alkaline fluxes for purposes of solution and of separation.

2. The term "Pyrocone" is used instead of that of "flame," employed by writers on the blowpipe, which last expression is here only applied to flames without a definite shape (*vide* paragraph 25), both because such a distinction is evidently a more correct phraseology, and because it prevents otherwise unavoidable confusion when both kinds of fire are produced in the same operation. For similar reasons the word "blowpipe," which seems a coarse and inexpressive appellation, equally applicable to a pea-shooter and the tubes of an organ, will be relinquished in these pages for the term "Pyrogene."

Pyrocones.

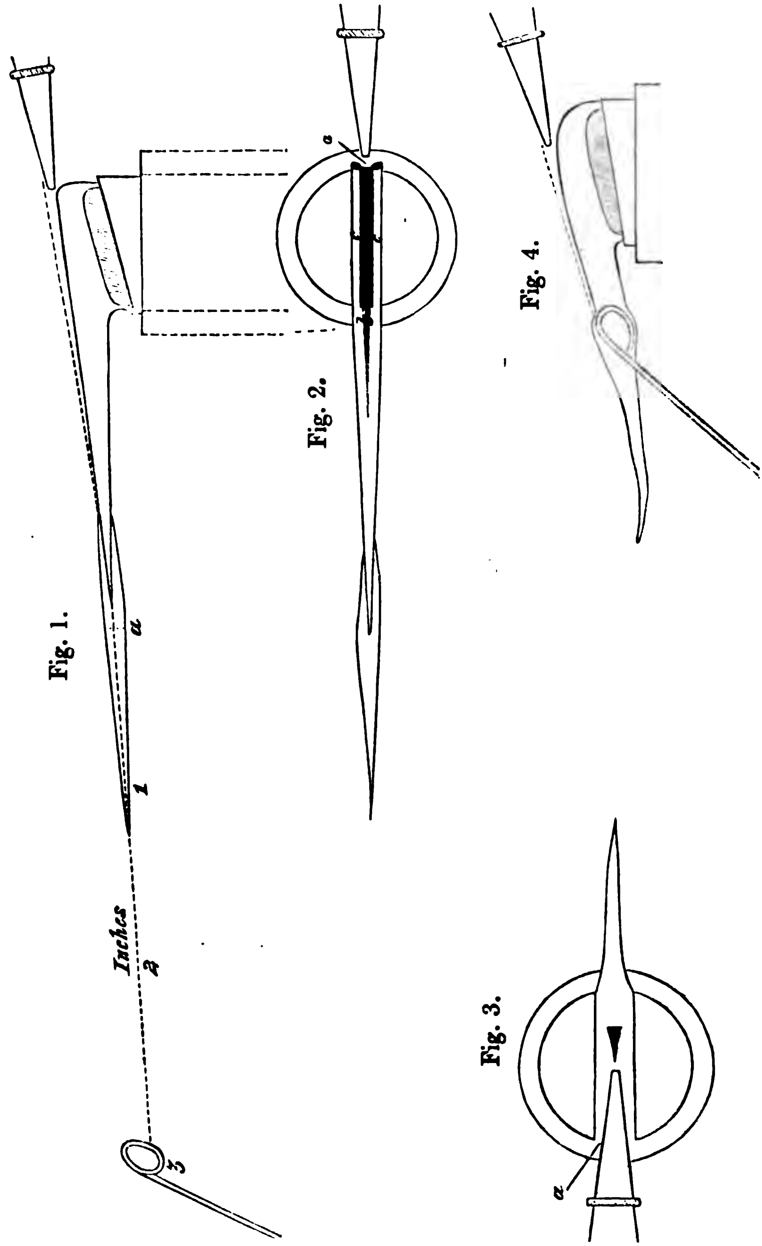
3. Pyrocones are divided into two classes, (*a*) natural and (*b*) artificial; (*a*) is the shape candle and other flames assume in air when left to themselves; (*b*) is that formed by treating (*a*) artificially as follows:—

4. On the application of a fine jet of air or breath, such as is impelled by operators with the pyrogene, to one side of the base of the natural pyrocone, the unburned gases in the centre are apparently expelled; the luminous cone, unless the blast is too weak, entirely vanishes, and what now appears is a long *solid* tongue of blue light, terminating in a point of needle-like fineness with a violet-coloured cone enveloping the apex, and extending, with a more obtuse termination beyond it, to a distance commensurate with the strength of the blast.

5. If we take the natural pyrocone afforded by ignited spirits or other light hydrocarbon producer, and blow into the centre of it with a mouth pyrogene, the jet of which is kept at some distance from one side of the cone, we observe two *synaxial* pyrocones formed by the blast, the bases of which are contracted or enlarged proportionately with its strength or weakness. If we approach the jet of the pyrogene so as to touch the side of the spirit-lamp pyrocone, and blow with greater violence, the inner or blast cone becomes invisible from the accelerated movement of the air; but we must analogically conclude that its basic diameter is contracted, and its length extended proportionately with those changes in the outer or *visible* pyrocone.

* In the term *composition* is included the quantitative as well as qualitative estimation.

6. That the air or breath from the pyrogene is driven *through* the longitudinal axis of the artificial spirit-pyrocone is also easily proved by the



operator blowing with still greater violence, when the apex of the cone will

be observed to be broken up by the blast at the under extremity, the sides remaining intact, so that the form is now that of a hollow cylinder.

7. Very different is the pyrocone produced by the attempt to blow into the natural one afforded by the flame of ignited oil*, wax, coal-gas (not previously mixed with air as in the Bunsen burner), or other dense hydrocarbon. The blast cone no longer penetrates the blue flame, but moves *above* it, drawing out, as it were telescopically, a *solid* pyrocone from under its base, so that the two cones are no longer synaxial but conjunctive (fig. 1).

8. If we view this pyrocone from above, the diagram (fig. 2) is something like what we see. All ascending heat from the lamp is completely stopped; whereas we could not view the spirit-pyrocone from a like position without burning the face, because in the first case the natural pyrocone is bent, as it were, under the blast, but in the second merely bored through by it.

9. It will be seen from fig. 2 that the hollow nucleus of the natural pyrocone, which we have assumed to be filled with unburned gases, is traversed over its whole length by the blast cone from the jet, showing the wick of the lamp underneath as a black band, the breadth of which is directly proportional to the diameter of the base of the blast cone, and therefore inversely so to the strength of the blast, which, blowing in the short side *a* of the blue or hydrocarbonous perimeter *into* its long sides *c*, *c*, causes these latter to rise slightly like flame-walls on either side, and draws them along with it until they combine at *b* to form a solid pyrocone underneath it, as above described.

10. If the short side *a* be burst by the insertion of the jet, instead of by the blast, the shape of the latter, as seen from above over the wick, is that represented in fig. 3, the size and power of the resulting pyrocone being immensely diminished.

11. If, therefore, the blast in pyrological operations were driven *through* the long axis of the oil- or gas-lamp artificial pyrocone, as is generally assumed, and as is undoubtedly the case with the *spirit*-cone, we could not (1) see the lamp-wick as through a *transparent* medium like air, and (2) the heat ascending from the upper part of the pyrocone would be felt upon the face of the operator stooping over it; but it must be here observed that, to produce these effects, the volume and strength of the blast must bear a certain proportion to the size of the natural pyrocone.

12. It would be reasonable to expect from the above that the spirit- and oil-lamp pyrocones should possess different properties, and this is the fact. The pyrogenical or artificial cone of the former cannot, or can but feebly, attain the results produced by the latter. Filled with breath-vapour, instead of being solid, it has too much heating-power, while the enclosed gases interfere both with the oxidation and the reduction of the subject of analysis.

* Cocoa-nut oil (if it be perfectly pure) affords by far the best pyrocone, and coal-gas almost the worst, for the purposes detailed in paragraph 15.

13. It follows also, from a consideration of fig. 2, and the facts detailed in paragraph 9, that the central portion of the wick or fuel in ordinary pyrological lamps is unutilized; and in fact, if an oil-lamp be used, having two thin wicks instead of one thick one, and these be slightly pressed apart in the front (as at *a*, fig. 2), it will be found that a pyrocone of nearly double power will be produced by a similar blast and expenditure of fuel*.

14. Keeping the assumed fact of the superincumbency of a blast cone, and the consequent solidity of the blue pyrocone underneath in remembrance, we can readily understand that a roundish object placed in the latter about the centre of its longitudinal axis, which has a diameter equal to or less than that of the pyrocone, will be wholly enveloped by the ignited gas or gases of which the cone is composed, so as to form a kind of bulb or jacket round the front, *i. e.* that side towards the base of the pyrocone whence the current proceeds. The object is thus apparently preserved from communication, not only with atmospheric oxygen, but with unignited gas of any kind. Such an envelopment is termed

The Hydrocarbonous Pyrocone (Symbol H. P.) (fig. 4).

15. The behaviour of different substances when held steadily in the hydrocarbonous pyrocone causes it to be a synthetical and analytical agent of great value to the pyrologist. Substances of a viscid nature (not salts), as phosphoric or boric acid, become coated, after a few minutes' insertion, with a shining lustrous film having an extraordinary resemblance to a metal†, which, when gold or silver oxides are previously dissolved in the bead, becomes tinged with yellow in the first case, and with a silvery shade in the second, much as alloys of those metals would. This film evidently increases in thickness according to the length of time it is immersed in the hydrocarbonous pyrocone, for after a short immersion the glass is still semitransparent; but when held a longer time it becomes opaque.

16. The film thus formed is very hard, being unsusceptible to the point of a penknife. It has no taste, or, if any, that of a metal, while the taste of the oxidized phosphoric acid is sharp and acidulous. After the application of the tongue, an iridescent tarnish is left like that of sulphur upon silver; in fact none of these films will stand long exposure to a damp atmosphere. They are so very thin and hard that it seems impossible to remove any portion from the glass (which remains vitreous in the inside) with the forceps, or even by breaking up the whole bead.

17. Sulphur in the viscid or red and resinous state is also changed by this treatment to a metallic appearance on its outer surface; but to produce this reaction, the pyrocone must be very perfect and of an unmodified blue colour, or the sulphur will be at once ignited and burn away. When

* Messrs. Price and Co. have manufactured, to the order of the writer, pyrological candles with a double wick on this principle.

† These films are difficult to produce with coal-gas, on account of its general impurity. *Vide* paragraph 4.

once prepared in this way, sulphur has no further tendency to burn, and it then has the remarkable property of giving, in a glass of phosphoric acid, reactions similar to those of copper, viz. green hot, and blue-green cold after treatment with a peroxidating pyrocone (to be hereafter described); but green hot and cold after a reducing pyrocone has been applied*. Ultramarine might owe its blue colour to this fact.

18. When a roundish mass of silica or alumina, or of both combined, is held in the hydrocarbonous pyrocone, it becomes quite black; and as this blackness is not merely on the surface, but throughout the mass, it would appear to be due to a decomposing effect exerted by the latter upon the pyrocone itself, and not a mere deposition of soot, which might have been supposed to have been mechanically carried along by the blast upon its surface. Alumina, however, appears to become partially fused, and thus forms into roundish or botryoidal swellings, while silica presents a steel black mass to the lens with shining metallic-looking points in it. These two omnipresent and almost universally combined earths, therefore, may be thus pretty correctly and extremely rapidly distinguished†.

Alkaline Earths in H. P.

19. Lime, strontia, and, to some extent, baryta and magnesia are not thus carbonized by treatment with the hydrocarbonous pyrocone, and may therefore when pure, be easily thus distinguished from the two first-mentioned earths. For lime, especially, a quantitative assay may be approximately made by slaking the mass thus treated in distilled water, when it will remain dark or grey or white, according as the lime exists in lesser or greater proportion. Above 80 per cent. of lime will cause the mass to remain perfectly white. The oxide of iron does not interfere with this reaction.

20. This property, which lime possesses, of remaining perfectly white and of resisting all tendency to reduction during such treatment, renders it an excellent medium for the detection of chlorides and fluorides, which seem to separate after a time from the lime, and to form some combination with the carbon of the pyrocone, the lime having no such tendency; for instance, in chloride of calcium, after a few minutes of this treatment, a small black patch, round in proportion to the sphericity of the mass, is formed on the side next the current of blue flame, which can be easily seen through the lens to be not soot, and seems to have a sweet taste; but if such a mass be often quenched in distilled water, and as often re-treated in the hydrocarbonous pyrocone, the black patch will shortly assume a metallic and white appearance.

21. Fluorides after the above treatment exhibit an irregularly shaped patch, also next the direction of the hydrocarbonous current; but this patch, instead of being black, has a changeable green colour like some of

* These effects are only in part producible by a gas-pyrocone. *Vide* paragraphs 4 & 7.

† The writer, by compressing the tip of the platinum jet so as to form a slit there, instead of a round orifice, produced a very perfect H. P.

the aniline compounds, and, after some quenching with water, a metallic appearance, but still green.

22. A very curious result is obtained by the treatment of bicarbonate of soda in the hydrocarbonous pyrocone. After a short time violent ebullition commences in the melted bead; bubbles of some (carbonic acid?) gas are seen to rise with great rapidity through it while red-hot. In this state of violent ebullition, fragments are projected from the mass, which, when examined through a lens, are found to be black hollow spheres like microscopic shells. Notwithstanding the loss occasioned by the ejectment of these projectiles, the mass, if now carefully examined by a lens, will be found to consist partly of caustic soda, and partly of a black substance, solid and even angular and shining like a piece of coal. This substance is proved to be carbon by its deflagration when heated with nitre, and its formation is proved to be not due to a deposition of soot from the lamp-flame, by the fact that the similar treatment of chloride of sodium will produce no such result.

23. If any of the "earths" be held on platinum-wire after being made into a paste with a little distilled water in a hydrocarbonous pyrocone, those which carbonize in such a situation, as alumina and silica, being slightly heated in an oxidating pyrocone for a few seconds so as to just burn off the carbon, and if the mass be then saturated with cobalt solution, lime and strontia will immediately turn a distinct blue; and of these two, if allowed to remain exposed to the air for a time, the lime will slowly turn green, the strontia brown*. The previous addition or existence of iron sesquioxide will cause these to turn, instead of blue, green in the first instance. All the other earths, if pure and not in a chemically caustic state, become pink with cobalt solution; and if then they are approached carefully to the natural pyrocone of a spirit-lamp, this pink colour deepens to a rich carmine†. Of these earths, alumina and magnesia will (as is known), when treated with a peroxidizing pyrocone, change, the first to a deep blue, the second to a pale flesh or salmon-colour. Silica in this case turns a distinct purple, even in presence of oxide of iron; lime, baryta, and strontia a grey or grey-black. We thus obtain a nearly new chromatic series between the common "earths" of considerable value in analysis, as follows:—lime (*a*) and strontia (*b*) *blue*, changing on exposure to air (*a*) to green and (*b*) to brown. Alumina (*a'*), silica (*b'*), baryta (*c'*), and magnesia (*d'*) *pink*; changing on treatment with a peroxidizing pyrocone (*a'*) to blue, (*b'*) to purple, (*c'*) to grey-black, and (*d'*) remaining pink.

24. Berzelius, and after him Plattner and other writers on blowpipe analysis, tell us we "are not to take notice of any changes of colour in a substance to which cobalt solution has been applied *previous* to the further application to it of an oxidating flame; for the colour imparted, blue, red,

* Fluoride of calcium *remains* blue.

† Oxide of *zinc* thus treated affords a beautiful peach-colour; oxide of *tin*, after P. P., a green.

or black, proceeds from the cobalt solution only, and not from any of the ingredients;" from which statement it is evident that we have hitherto lost some of the most valuable qualitative indications of cobalt, due to the important fact that lime and strontia are rendered caustic by much less heat than the other "earths," and therefore dehydrate the cobalt solution after treatment with a hydrocarbonous pyrocone, rendering it blue, which none of the other earths will do. If the colours "proceed from the cobalt solution only," how is it that lime turns *blue* and baryta *red* when it is thus applied?

Fish-tail Flames.

25. If a pyrochromatic substance be held on the loop of a platinum-wire in a rapid hydrocarbonous current, produced by blowing strongly, the current is broken upon it so as to form a kind of fish-tail flame at its rear, *i. e.* the side turned *from* the base of the pyrocone, in the blue matter of which its front is enveloped as usual. The inner sides of this fish-tail flame will, after a short time, be observed to be deeply and continuously tinged with the colour which is the chief characteristic of the substance burned*. A far stronger pyrochromatic reaction is thus obtained than by holding the substance in the position usually adopted, of what is called "the point of the outer flame," or, in fact, in an oxyhydrogen pyrocone; for here the superposed blast is too violent, and carries away the colour as soon as formed.

The Oxyhydrogen Pyrocone. (O. P.)

26. In which the object is held as at *a*, fig. 1. It is commonly called "the oxidating, oxidizing, or outer flame;" but that the two first of these appellations are incorrect is shown by the fact that when some metallic oxides, as those of gold, silver, or mercury, dissolved in a flux more delicately sensible to oxidation and reduction than borax or microcosmic salt, viz. phosphoric acid, are held in this position, the bead, so far from being further oxidized, immediately precipitates its contents, and becomes dim or opaque in consequence.

27. This pyrocone appears to be caused by the intermingling of the two currents—of air and ignited hydrocarbonous matter, its broadest part being at *a*, where they may be supposed to cross each other, giving it a slightly oblate appearance.

The Peroxidizing Pyrocone. (P. P.)

28. In order to produce the full effect of this pyrocone, the object must be held at a distance of three inches from the point of the blue. It can be produced, but not long sustained, by the mouth, as a very strong blast is necessary to impart sufficient heat to an object at such a distance (3, fig. 1).

The Bunsen Blowpipe.

29. Before leaving the subject of pyrocones, it is necessary to mention

* The substance should occasionally be dipped in distilled water.

that the ingenious blowpipe of Bunsen, by which the breath is *forced* into a jet of ignited gas itself, is utterly useless for the purposes, and to produce the results detailed in these pages. The pyrocone thus formed is indeed the counterpart of that produced by blowing into the pyrocone of a spirit-lamp, except that its temperature is perhaps higher, with the deleterious results mentioned in paragraph 12.

The Fluxes.

30. These are invariably supported on platinum-wire in the admirable and perfect manner invented by Gahn, and, as soon as a pyrocone is applied, assume the form of a spheroidal bead, which revolves or spins round upon its centre with a rapidity proportional to the fluidity of the matter of which it is composed. The "glasses" or "beads" thus formed, with the oxides dissolved in them, may be quantitatively determined, as to their weight and size, by means to be presently described.

31. Berzelius informs us that "Cronstedt used but three reagents—basic carbonate of soda, borate of soda, and the double salt of phosphate of soda and ammonia. These reagents are still in use; and among the great number of those which have been tried since that time, not one has been found to replace either of these. It is singular enough that in the very beginning of the art, the very best reagents should have been hit upon." (Berzelius on the Blowpipe, p. 32.)

32. One of the objects of this paper is to attempt to show that the two last fluxes mentioned in the above paragraph are not only *not* "the very best reagents," but that they have, by the complicated and obscure results obtained necessarily from their compound nature, seriously retarded the progress of pyrognostic examination. For instance, speaking of the third flux mentioned, the metaphosphate of soda, produced from what is commonly called microcosmic salt, Berzelius says (p. 39), "its efficiency as a reagent depends principally on its free phosphoric acid; and it is preferred to this because the phosphoric acid cannot be kept without deliquescing, while at the same time it is much dearer, and is also easily absorbed by the charcoal. The salt of phosphorus shows, therefore, the action of an acid upon the substance to be tested."

Phosphoric Acid (Symbol $\overset{\cdot\cdot}{P}$).

33. Now, if glacial phosphoric acid be heated until it melts into a substance of viscid or gum-like appearance, and be thus poured hot into a wide-mouthed stoppered bottle (which should also be hot when receiving it), it can not only be kept without deliquescing, but, when solidified by cooling in the bottle, may be carried about in the pocket without fear, kept for years in the most rainy climate, and allowed to remain even for hours with the stopper of the bottle off. It has also the great advantage of being now-a-days far more easily procured pure enough for the purpose from most respectable chemists even in out-of-the-way stations, as in India, in conse-

quence of its use in therapeutics, than microcosmic salt can be. It is used by simply dipping the red-hot platinum-wire loop and the glass, of whatever description, formed upon it into the bottle, when a fresh supply of phosphoric acid adheres to the hot bead, without the supply in the bottle being at all adulterated.

34. That the metaphosphate of soda does *not*, when heated, exert upon substances added to it the reactions of an acid, unless the basic soda be displaced by another base for which the acid possesses a greater attraction, must be evident to an ordinary chemist; and there would appear to be few substances for which phosphoric acid has a greater attraction than it has for soda; in fact, the most valuable pyrognostic reactions *primâ facie* of the acid upon substances have been lost to operators precisely because the salt it forms with soda *fails* to give us those acid reactions, as follows:—The acid effervesces violently with all carbonates, and with some of the metallic oxides, the salt does nothing of the sort; and the necessity felt by many mineralogists and geologists of carrying about in their pockets a phial of the unpleasant and dangerous hydrochloric or nitric acid is thus at once obviated.

35. The *acid* used to dissolve cobalt oxide in any considerable proportion is blue hot, but assumes on cooling a magnificent red-violet colour*, to which the modern word "magenta" is partly applicable. When soda or potash is applied to this glass in sufficient proportion (about 17 per cent.) to form the metaphosphate of those bases, the glass remains blue, and a standard of alkali for purposes of calculation is thus evidently obtained. But as cobalt oxide produces with this flux many shades of colour according to the quantity in which it is added, from a pale and scarcely perceptible pink to the deep crimson-violet above mentioned, and as these degrees of red are exactly azurized† by a corresponding strength or quantity of the alkali added, it is plain that a kind of chromatic scale or table of colours might thus be made, of great use in the quantitative measurement of alkalies on the one hand, or of cobalt oxide on the other, instead of the unvarying "blue" which we find opposite oxide of cobalt in all blowpipe tables.

36. Instead, therefore, of superfluously multiplying illustrations of the difference between the reactions of the pure acid used as a flux and of the assumed "free acid" in microcosmic salt, it will be better to give here in slight detail some of the more important of the former with various oxides.

Gold.

37. Is dissolved and oxidized by this flux in an O. P. (as at *a*, fig. 1), when added in minute portions of the leaf—a fact which suggests that P under these conditions is more powerful as a solvent than any one of the mineral acids. As stated in paragraph 26, the position (*a*, fig. 1) will precipitate the dissolved gold oxide, rendering the bead of a dirty or muddy

* Discovered by the writer on 12th July, 1869.

† Lithia and its salts will not apparently azurize this cobaltine glass, but afford with it a very pretty purple violet colour.

appearance, which the application of a P. P. (as at 3, fig. 1) will soon remove, the bead then appearing not merely diaphanous, but highly refractive. If this brilliant auriferous glass be now treated with a good H. P., the white metallic-looking film referred to in paragraph 15 is formed, with a slight but distinct shade of yellow like pinchbeck, which is apparently due to the gold in solution; and this bead may thus be made alternately diaphanous and metallic-looking as often as is desired.

38. If the auriferous transparent bead be carefully kept for some time about half an inch from the point of the whole pyrocone, or two inches from the blue, as at 2, fig. 1, a beautiful shade of bluish rose-colour flushes over it just as it is becoming cold*; and the production of this tint, which cannot be confounded by the dullest observer with the red violet of cobalt, or the amethystine tinge of titanic acid or manganese, is an excellent test of the skill of the operator, as well as of the delicacy of the pyroconical reactions in this flux, for a hair's breadth too far towards (3, fig. 1) will cause the glass to be diaphanous and colourless on cooling; while a corresponding error in the other direction towards 1 will, as has been mentioned, produce a muddy appearance.

39. Gold-leaf is more rapidly dissolved, and the above reactions more easily produced in a glass of *phosphate of lime*, which appears to be, under pyrological conditions, a more powerful solvent of metallic oxides than any other known flux. It will be afterwards described; but it has the disadvantage in analysis, referred to in paragraph 32, of being a salt.

40. The ruby colour bestowed by gold upon glass and fluxes would thus appear, by the experiment above detailed, to be due to an *exact* amount of oxidation. The oxides of tin and antimony, added with it to colour glass under the name of "purple of Cassius" &c., seem not to have any thing to do with the production of the colour†.

Silver.

41. The most infinitesimal trace of the oxide, or of a salt of silver, added to a bead of P, gives a copious yellow precipitate like cream, accompanied at *first*, if the bead be held in a P. P., as at 2 or 3, fig. 1, by a very beautiful but very transient rose-colour. This is such a delicate reaction for silver, that it will be at once obtained from most galenas; and although thus a most important test *qualitatively*, is too fine to admit of being used as a quantitative standard for the estimation of rich ores.

42. There are two ways of effecting this. First, for an ore supposed to contain only a small percentage of silver. If the slightly argentiferous glass be retained in the position 3, fig. 1, the yellow precipitate soon disappears, and the glass becomes clear, highly refractive, and brilliant. On

* The addition of fresh P at this stage brings out this beautiful reaction still more decidedly.

† No metal, not even gold, has any tendency to alloy the platinum-wire in this flux when kept under a P. P. [Phosphofluate of lime gives with gold oxide a bead as blue and brilliant as a sapphire.—September 14th, 1872.]

changing its position in the pyrocone to that of *a*, fig. 1, at present called the "oxidizing flame," the yellow precipitate immediately and copiously reappears; but there is no visible mark or signification by which the operator can thus judge of the quantity of silver oxide added. When, however, this amounts to 5 per cent. of the whole glass, and the latter, rendered diaphanous by the first position, is suddenly and momentarily brought into the second one indicated above, or, better, to just the tip of the blue, from whence, however, it must be instantaneously removed, a very remarkable and very beautiful appearance results. It is that of an almost perfect imitation of a pearl, produced apparently by the reduction of the oxide *near* the surface to the metallic state, while a vitreous glaze or gloss is still retained *upon* the surface.

43. This, then, may be called the first standard of silver for ores containing that oxide up to 5 per cent., though of course it may be used for richer ones; but the following method is more rapid for a rich ore, provided there are no chromatic oxides present to interfere with the clearness of the glass.

44. Second, for rich argentiferous ores. If we continue adding oxide of silver to a weighed Ᵽ glass, and dissolving it carefully as at 3, fig. 1, we shall find the glass remain diaphanous until 20 per cent. of the oxide has been added, when the yellow creamy precipitate again begins to appear, causing, for rich ores, 20 to be the standard of silver. Of course, in calculating results from these "standards," the ratio deducible from them must be of the *inverse* kind; that is, for instance, if we find an argentiferous ore requires to be added to the extent of 40 per cent. in order to produce the yellow precipitate in a P. P. as at 3, fig. 1, or just double the quantity of the pure oxide of silver to effect the same result, we take the proportion of Ag as just half of purity, or 50 per cent.

Mercury and the Volatilizable Metals in Ᵽ.

45. If these oxides are taken upon the hot glass, and the mass inserted into a good H. P. as in fig. 4, they are neither volatilized nor dissolved. The volatile oxides under such conditions form part of the metallic-looking crust or film, which is invariably formed over the surface, and can thus be added in large quantity with a very trifling loss. If the mass be now treated with a P. P. as at 3, fig. 1, these oxides are rapidly dissolved, all of them bestowing on the Ᵽ glass a brilliant golden yellow, especially arsenic acid, by which a glass is thus produced quite equalling in appearance the finest topaz.

46. If this glass be now returned to an O. P., as at *a*, fig. 1, the oxide is immediately precipitated with a dim, and often an opaque grey or grey-black appearance; and although mercuric oxide (for instance) is usually presumed to be of so volatile a nature that its reactions are not given in blowpipe tables, this mercurial oxide is so difficult to volatilize that the strongest O. P. will not clear the Ᵽ bead from it, but only burn both slowly away.

Sulphur.

47. If sulphur be added to a P bead as described in paragraph 45, and then treated with a P. P., the curious result of chromatic reactions exactly similar to those of copper, *i. e.* green hot and blue cold, is produced. The addition of plumbic oxide heightens this effect; and the resulting blue bead is quite indistinguishable from a cupreous one placed alongside.

Nitrogen.

48. If a P bead be constantly dipped in the strongest possible solution of ammonia or in concentrated nitric acid, and immersed as often in a H. P., as fig. 4, numerous black specks will be found on the surface like carbon, but much more difficultly burned away. After a time these appear to combine with the metallic-looking film which is formed by the H. P., and the substance is then by no means easily volatilized. The glass thus impregnated with nitrogen will be found to be clear hot, yellow and gelatinous on cooling, therein exactly differing from those of the alkalies, the volatizable oxides, and some of the earths, which are yellow hot and clear cold.

Oxide of Copper in P.

49. If we add pure cupric oxide to a weighed P bead, and treat it with a P. P. (2, fig. 1), we find that it takes exactly 5 per cent. of the whole bead to produce distinctly the peculiar *blue* of copper. The glass must be carefully held in the position indicated, as O. P. would leave it *green* *: 5 per cent., then, may be taken to represent the standard of copper for quantitative measurements as described in paragraph 44; but in such cases as Cu pyrites, where there is a chromatic interference of other oxides, something more is necessary.

50. It requires one third more than the weight of a P glass in sulphur to give it the cupreous blue appearance referred to in paragraph 47; that is to say, a 50 mgrs. glass of P requires 75 mgrs. of flour sulphur added by degrees for that purpose. But it is found that by treatment in H. P. the flour sulphur, when it assumes the metallic appearance referred to in paragraph 17, is reduced to one fifth of its bulk; so that 75 mgrs. of flour sulphur only represent 15 mgrs. of the allotropic sulphur, and 15, therefore, is taken as the standard of sulphur. It has also been ascertained that 16 mgrs. per cent. of oxide of copper, when added to this sulphurous P bead, cause it to remain *green* even after a P. P., probably on account of the disposal by the sulphur of the superfluous oxygen; 16, therefore, is taken as the equivalent standard of copper to sulphur. If we now add oxide of lead to the *green* cupreo-sulphurous P bead thus produced, we shall find that, on the addition of 24 mgrs. per cent. the glass will again appear blue; 24, therefore, is taken as the equivalent standard of PbO to sulphur.

51. Copper pyrites dissolved in a P glass has a dirty green appearance, without any shade of blue in it, after a P. P.

* This is not the case with borax or microcosmic salt.

As an example, it took 57·1 mgrs. of PbO to azurize a green Cu pyrites P bead of 100 mgrs.

$$\text{Then } \overset{\text{PbO}}{24} : \overset{\text{S}}{15} :: 57{\cdot}1 = 35{\cdot}6 \text{ Sulphur. per cent.}$$

$$24 : \overset{\text{Cu}}{16} :: 57{\cdot}1 = 38{\cdot}0 \text{ Copper.}$$

Therefore the oxide of iron = 26·4 Iron.

This is not very wide of the actual composition of Cu pyrites as decided by chemical analysis, which is sulphur 34·9, copper 34·6, iron 30·5. The specimen treated was a rich one from Freiberg in Saxony *.

52. With rich ores, as the red oxide, the method (paragraph 49) is so delicate, that an assay roasted *through* platinum foil gave 4 mgrs. more in the hundred than it did unroasted. The best and safest plan is to have an azure P glass coloured with 5 per cent of CuO as a pattern, and place the assay alongside of it on a sheet of white paper.

Titanium and Tin in P.

53. A diaphanous P glass having either of these oxides dissolved in it will, after being held a considerable time as at 3, fig. 1, show (apparently) crystals, yellow with Ti, and white with Sn, produced in it. This result cannot be effected with the mouth, but only with a table pyrogene †.

Alumina and Silica in P.

54. Berzelius proposes (page 86) to estimate silica pyrognostically in a mineral thus:—"Every substance of an earthy or mineral nature, which melts with *soda* with effervescence into a transparent glass which remains transparent on cooling, is either silica or a silicate in which the oxygen of the silica is at least double the quantity of that of the base." This ingenious discovery, which is strictly correct in cases where it is applicable, and in such cases, therefore, most useful, is unfortunately inapplicable to those silicates in which the estimation of the silica is of the most importance. The so-called "alkaline earths," especially *Lime*, will not permit silica, though combined in any proportion, to yield a bead with soda transparent on cooling, and they seem also to prevent or cancel effervescence in the same.

55. After many comparatively futile attempts to separate and estimate

* Another mode of procedure with sulphides is to carefully add the roasted ore (which, by a method of roasting to be hereafter described, *invariably* loses *just* 17 per cent.) atom by atom to the P bead in a P.P. (when the CuO blue reaction will *first* appear) until the FeS *begins* to interfere with it; then deduct from the large amount of copper thus indicated the sulphur and iron as determined by roasting and protoxide of lead.

† Such as are sold at Freiberg by "Herr Bergmechanikus Lingke," manufacturer to the Royal University of that place: *vide* Plattner's 'Probirkunst mit dem Löthrohre,' vierte Auflage (Leipzig, 1865), p. 32, note.

alumina and silica in various ways, one of which is referred to in paragragh 18, which occupied the writer some years, the following plan, which ought from its simplicity to have suggested itself at first, has been followed with apparent success.

Nearly every oxide or substance is more soluble pyrologically in P than alumina and silica, while alumina is far more soluble than silica is. The "alkaline earths" are rapidly dissolved; and lime especially is not only dissolved, but forms a salt, referred to in paragraph 39, which will dissolve almost any thing but silica *.

56. It has been ascertained that alumina will dissolve to the extent of 20 per cent., and silica to that of only 6 per cent. in a P bead; and this result is not materially modified by lime. After those amounts respectively have been added, the undissolved alumina appears as white roundish fragments, like pieces of fat, the silica as a semitransparent mass like melting snow, so that they are thus distinguished without difficulty even in presence of lime or the alkaline earths.

Six per cent. is therefore taken as the standard of silica in quantitative calculations; but as 20, that of alumina, is inconveniently large, it is better to employ as the flux a P bead half saturated with 10 per cent. of pure alumina, and to make 10 the standard of that "earth."

57. A P glass saturated with silicic acid still dissolves a little alumina, but the converse is not the case; it is best, therefore, to test *qualitatively* for either earth with a P glass saturated with *alumina.*

Boric Acid (Symbol B).

58. Plattner and succeeding writers on the "blowpipe" made use of this substance for the purpose of separating metallic lead from copper in an alloy of the two by oxidizing the former, and therefore as a means of *affinage* of the latter metal †. In this operation, B appears to absorb litharge precisely as bone-ash does in cupellation; but it acts at the same time as a kind of shield to protect the copper from oxidation.

59. Berzelius employed B as a test of phosphoric acid in phosphates by the insertion through the mass of a piece of pure iron wire, which is corroded and fused if the phosphoric acid exists over 5 per cent.; but this reaction, to be effectual, presupposes the perfect solution of the substance containing the phosphate in B, which, as will be seen, can be effected with very few oxides indeed.

60. In fact it is precisely the insolubility of almost all substances but the alkalies in boric acid which gives it the extraordinary value it undoubtedly possesses as an agent of *separation*; and the following few examples will not only clearly demonstrate this fact to the impartial chemist, but show

* Borax dissolves silica pyrologically more completely than any known flux. The writer found that phosphoric and nitric acids, combined in about equal proportion, attacked and broke up the Berlin saucers in which they were boiled.

† Vide Plattner's "Probirkunst" &c., edition of 1865.

him that the fact itself has been utterly passed over by writers on the subject hitherto, and that the real analytical value of B, therefore, has remained unknown.

Oxides of Cerium, Didymium, and Lanthanum.

61. If a good specimen of the mineral "cerite" be powdered and applied to a bead of B in an O. P., the following phenomenon results. The substance appears to separate into three distinct parts:—(1) red-brown and resinous-looking round spots appear near, but not *on* the surface; (2) other round spots, more bulky or puffed out, nearer the middle of the bead, of a pale buff colour; (3) a slight milky turbidity through the bead, as though a finely divided precipitate were suspended throughout the whole mass. If what is sold or made by the chemists as "pure oxide of cerium" (generally a nut-brown powder) be applied to a B bead in like manner, precisely the same triplicate separation occurs, only the turbidity is very much less.

62. These round spots are observed to be round through a lens when viewed in *every* direction; they are therefore sphericles or globules *.

After considerably long treatment with O. P. the smaller buff-coloured globules will be dissipated throughout the bead, causing more turbidity with a slight shade of buff in it; but the red resinous globules remain unchanged, appearing white-hot in the red-hot bead (from which it appears that their fusing-point is higher than that of B), and may be collected, from their superior specific gravity, at the bottom of the bead, by careful manipulation with the point of the blue pyrocone, into one large globule.

63. Professor Stokes has found that, by treating this B bead with distilled water, the general mass is dissolved while the contained globules remain intact, the most minute of which may be thus perfectly extracted. They may be also extracted more quickly, but with less precision, by wrapping the bead when *quite* cold in paper, and tapping it gently upon an anvil with a light hammer, when the matrix breaks away from the globules. Added to a bead of P under O. P., the red globule first fuses to an intensely lemon-coloured mass on the surface, and then dissolves in the glass with effervescence, giving the reactions of ceric oxide. These red ceric globules have been discovered by Professor Stokes to contain also oxide of *didymium*, the absorption-bands of which, when spectroscopically examined, they show very distinctly.

64. It is therefore assumed that the red balls are composed of ceric + didymic oxides, and the buff-coloured ones of lanthanic oxide; while the turbidity is proved beyond reasonable doubt to be caused by lime, which with baryta are the only substances, except phosphoric acid, capable of causing this opaline turbidity on *first* application to the B bead †. At any

* This was written before the writer knew how to extract these globules: *vide* next paragraph.

† A trace of phosphoric acid applied to a bead of boric acid and heated in O. P. affords a glass when cool having an almost perfect resemblance to an opal.

rate, the so-called "pure oxide of cerium" is thus shown, by one simple operation, to consist in reality of no less than *four* substances.

Lime, Strontia, Baryta, Alumina, Silica, and Magnesia.

65. Of the alkaline earths, lime, as above mentioned, causes immediate turbidity over the whole bead, and when added in greater proportion, produces round spots suspended in the turbid mass. These spots are perfectly clear and colourless like drops of limpid oil in milk. When added in very large proportion, the clear spots collect in one large one, which, if still further addition of lime is made, absorbs the remainder of the turbid part, leaving the whole bead beautifully clear and colourless. The turbid part, therefore, would appear to be an attempted solution of the lime by the boric acid; the clear part, a complete solution of the boric acid in the borate of lime.

66. Strontia forms large beautiful vitreous-looking globules, quite transparent and even refractive; they have great specific gravity, and can be easily aggregated at the bottom of the bead. Baryta affords sphericles like fish-eyes, transparent at first, but soon becoming opaque. Magnesia is not at first acted on by B, but after a short O. P. resolves itself into opaque, white, and compact sphericles like miniature snow-balls; these, after long O. P., and consequent exhaustion of B, are clarified and become transparent, but are again rendered opaque by the addition of fresh B. It is concluded from this fact that these contained balls have a fixed relative proportion as regards quantity to the containing B. Alumina and silica remain as amorphous fragments, and do not congest into globules; those of the former are white and opaque like pieces of fat; of the latter semi-transparent.

The Alkalies.

67. Soda, potash, and lithia appear to be the only substances which dissolve completely in boric acid in any proportion, and hence the value of the latter as a detective agent for them, and also as an alkalimeter; for if a very small trace of soda or potash contained in a mineral or salt be applied to a B bead having globules of cobalt (for instance) suspended in it, those nearest the side where the alkali is applied are dispersed and spread over that side as a pink suffusion. If 5 per cent. be added, the sphericles of cobalt disappear, the whole bead is clarified, and assumes a blue colour while hot, but remains pink on cooling. If 17 per cent. be added, the bead remains *blue* on cooling, and (in the case of soda) borax has been formed; but this method would probably be considered incomplete if it did not afford a means of distinguishing between, as well as of measuring these two alkalies; and this it does as follows:—

Vesiculation.

68. If the B bead, on the addition to it of the substance to be examined, shows, by the reaction above described, that an alkali is contained in the

latter, a fresh bead should be impregnated with a weighed portion of the substance, which should be dissolved as far as possible by an O. P. Then, the bead being rapidly removed from the point of the pyrocone, it should be blown into while red-hot with the jet of the pyrogene, which for that purpose should be advanced so as just to touch the ring of the platinum wire. If the bead be not too cold, the result will be that the whole mass is blown out into a thin clear bubble or vesicle about seventy times its first size, thus presenting a very large surface of the dissolved substance to be operated on. This vesicle should then be held in the operator's open mouth and breathed upon for some time, when, if the alkali has contained even a trace of potash (1 per cent. of KO affords the reaction), the vesicle will immediately become clouded over with a light blue film of the colour of a solution of quinine when held against the light. If only soda be present, the vesicle will remain clear but will begin to deliquesce. Lithia affords a tarnish like that of breath on a pane of glass, and the vesicle does *not* deliquesce.

69. This test for potash in presence of soda is so delicate that if two beads, even of borax, one containing a trace of potash, be vesiculated and allowed to remain in a moderately dry room for some time, the one containing the potash will in the course of a few hours cloud over, while the other will remain quite clear.

Determination of Gases by Vesiculation.

70. The B vesicle by itself will cloud over after a few minutes, but the appearance then is more white than blue; and if the clouded vesicle be approached to the flame of a spirit-lamp, this white coating is not removed until the flame almost touches it and shrivels the substance of the vesicle, whereas in the case of the addition of a trace of potash, the cloud flies as if by magic when the vesicle is even a considerable distance off.

71. The addition of *chlorine* to the B bead, made by dipping the bead in strong hydrochloric acid and treating as in paragraph 48, apparently causes the vesicle made therefrom to be still more sensitive to the action of gases upon its surface. It clouds over in common air almost the instant it is made. If held over a solution of *ammonia,* this nubilescence is prevented, and the vesicle remains clear. If held in a noxious or putrescent gas, creamy or brownish-white streaks are formed over the surface, which is otherwise clear. But the most characteristic reaction is that afforded by sulphuretted hydrogen, in an atmosphere of which this vesicle becomes rapidly pitted with circular spots as though it had suddenly taken smallpox. These spots through a lens are observed to be round radiated crystals, with a yellowish tinge near the circumference. After a short time, they rot into holes.

72. Another curious result of vesiculation is the crystallization of the surface in a moderately damp atmosphere in the course of ten or twelve hours. Borax-vesicles show these best; but the complicated nature of the salts formed renders conclusions from their crystallization very uncertain.

Microscopically viewed, however, these crystals are very beautiful, especially in polarized light; and it seems at any rate certain that silica and the silicates thus treated invariably crystallize in most elegant and beautiful arborescent appearances, the form taken by other salts being usually that of a disk, often of a leaf.

Cobalt, Copper, and Metallic Oxides.

73. The behaviour of these in B has not yet been fully examined; but such as have, promise results quite as interesting and important as those which may be derived from that of the earths. For instance, when an ore containing the oxides of cobalt and nickel, previously manipulated as in paragraph 90, is boracically treated as described in paragraph 61, the cobalt immediately congests into globules, which after O. P. appear blue-black, after H. P. red-purple through a lens; the nickel oxide, on the contrary, remains in amorphous fragments, which are bright green after O. P., and have a metallic lustre after H. P. Cupric oxide forms in O. P. balls of an indigo-blue colour, not easily distinguishable at first from those of cobalt; but in H. P. the cupreous globules immediately give out streaks of the red suboxide, which cannot be mistaken, though, of course, were further proof necessary, the addition of 5 per cent. of soda would form a pink glass from the first, and a blue one from the second. Iron oxide remains in amorphous fragments of a black-brown colour with a rusty halo or tinge round them, and is thus easily distinguished. Oxide of Uranium forms a stringy black mass with a yellowish opacity round, which the addition of soda dissolves to a bright pea-green bead. Molybdic acid affords many curious and beautiful changes, for a description of which there is not space.

Silver, Lead, and the Volatilisable Oxides.

74. None of these form either balls or fragments in boric acid, but spread over the whole bead as a milky precipitate, that of silver having a slight pinkish tinge. Nothing, therefore, can be easier than their separation thus from those metallic oxides which form balls or fragments, as the former of these can not only be collected or aggregated into one sphericle, but extracted from the cold bead with the greatest ease, as described in paragraph 63. It is obvious that this process would probably form an important method of extracting *silver* from its ores with very little loss; for the boric acid protects oxides contained in it from the direct effect of an O. P., which dissipates pure silver unprotected to the extent of 10 per cent. in a very short time.

75. Altogether, as a detective reagent, boric acid seems scarcely inferior even to phosphoric acid, while as a separating one it is quite unsurpassed.

Many hypotheses of the formation of these sphericles or balls have suggested themselves to the writer, but none with a sufficient weight of evidence in their favour to be stated here. They may be due (*a*) to capillary phenomena, (*b*) to the retention of a certain amount of carbonic acid (for of

the "earths" it is evidently only those forming carbonates which produce them), or (*c*) to some law or principle as yet not fully known.

76. A bead of pure boric acid is evidently more fluid in H. P. than in O. P., and the hydrated appearance of cobaltine balls in the former (paragraph 73) would seem to suggest the setting free of some constitutional water (?)

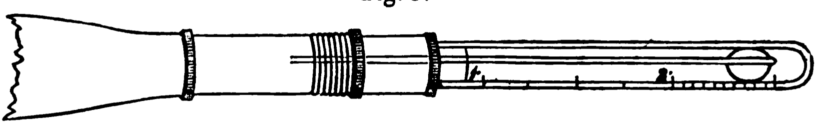
Phosphate of Lime.

77. This is a useful flux for purposes of mere solution, as referred to in paragraph 39. A curious phenomenon results from the application to the hot glass, containing a metallic oxide in solution, of carbonate of soda, which, instead of fusing in the glass under O. P., does so by itself at first, apparently drawing or precipitating the metallic oxide to itself. If a hot glass of phosphoric acid be applied to warm calcined lime, the mixture takes fire and burns with a very pretty pale yellow light, phosphate of lime being formed. This flux has been little investigated for pyrological purposes, having been thus first used by the writer.

Quantitative Analysis by "Glasses" or "Beads."

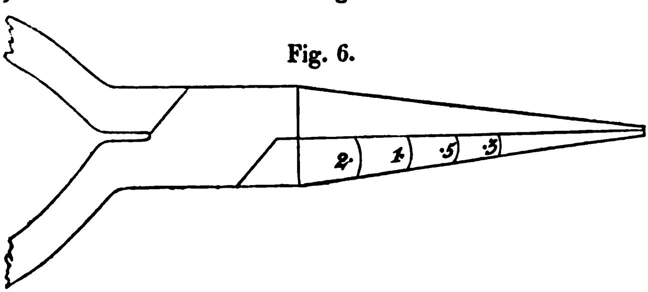
78. For this purpose it is evident that the glass must not only be weighed but measured; for, as both phosphoric and boric acids lose weight and substance in direct proportion after a strong O. P., some means of measuring the diameter of the bead, and of thus keeping it up to the mark, is required. Sufficient accuracy for this purpose seems to be afforded by a simple instru-

Fig. 5.



ment as that shown in fig. 5, representing a common glass tube which exactly covers a 50-mgr. bead of phosphoric acid. The graduations on the outside of the tube are for the purpose of showing the length of the platinum wire (in tenths of an inch), which of course is proportional to its weight, the thickness or diameter being the same.

Fig. 6.



79. Fig. 6 represents the instrument with which the platinum wire is

twisted into a loop, which must be perfectly circular, and of a diameter corresponding to the size of glass required. It is a pair of common cage-maker's pliers with round but tapering legs; only the right (or left) leg should be graduated and figured, say, in tenths of an inch, to show the diameter of the loop made on the other one. Neither of these instruments, however, is to be understood as at all dispensing with the use of the assay balance, of which a beautifully portable description is now made cheaply at Freiberg; it indeed is indispensable, and to be referred to when any doubt is entertained.

Roasting through *Platinum foil.*

80. The cautions and directions against using the platinum which is supplied as part of all pyrological apparatus, to be found in most chemical works, are manifold and almost amusing. It would be better, if these directions are to be followed, to have no platinum foil at all than to have it and not use it; but in practice it will be found that there are comparatively few substances which injure platinum foil when heated *through* it, to such an extent that it cannot be advantageously used for months. The ore called "stibnite" is one of these, but with care even galena may be thus innocuously roasted.

81. The foil, which should be thicker than the usual English kind, can

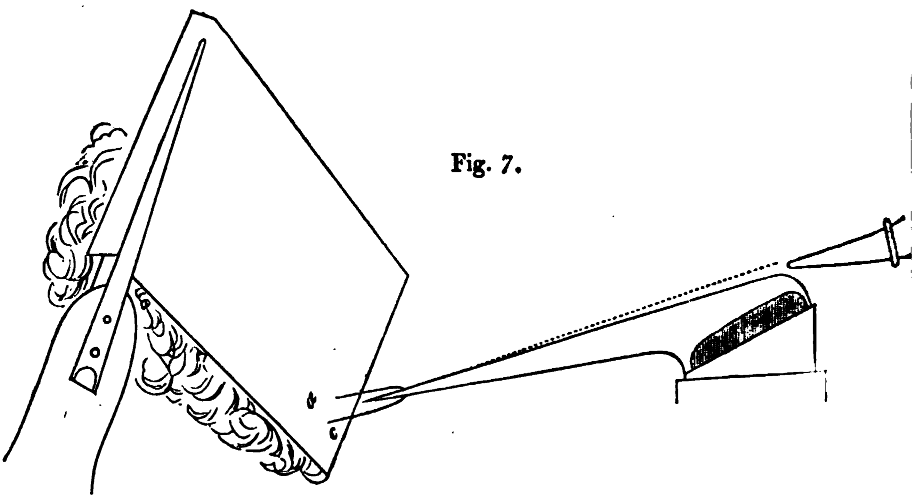


Fig. 7.

be conveniently made into a small tray about 1·5 × 1 inch, and held, as in fig. 7, with a pair of brass pliers having steel legs, the subject of examination being deposited as a paste (made on a slab with distilled water) on its lower lip. The point of the pyrocone must then be applied to the *back* of the tray opposite the substance, and on no account is it to be directed upon its surface.

82. It will be found that only a certain and normal, not an uncertain and irregular degree of heat *can* thus be applied to substances which under pyrological conditions combine with oxygen or are reduced to the metallic state; and therefore that oxidation on the one hand is as exactly regulated as though it had been controlled by a balance, and that reduction on the other hand need not be feared, except in the case of the very fusible metals, as antimony or lead. For instance, copper pyrites roasted in this way will be found to lose exactly 17 per cent. and no more, however long or strongly the pyrocone has been applied. The same amount of sulphur is thus driven off from "copper glance;" and there can therefore be little reasonable doubt that 17 per cent. is the extent to which sulphur may be dissipated from copper ores without fusing both together.

Sublimation.

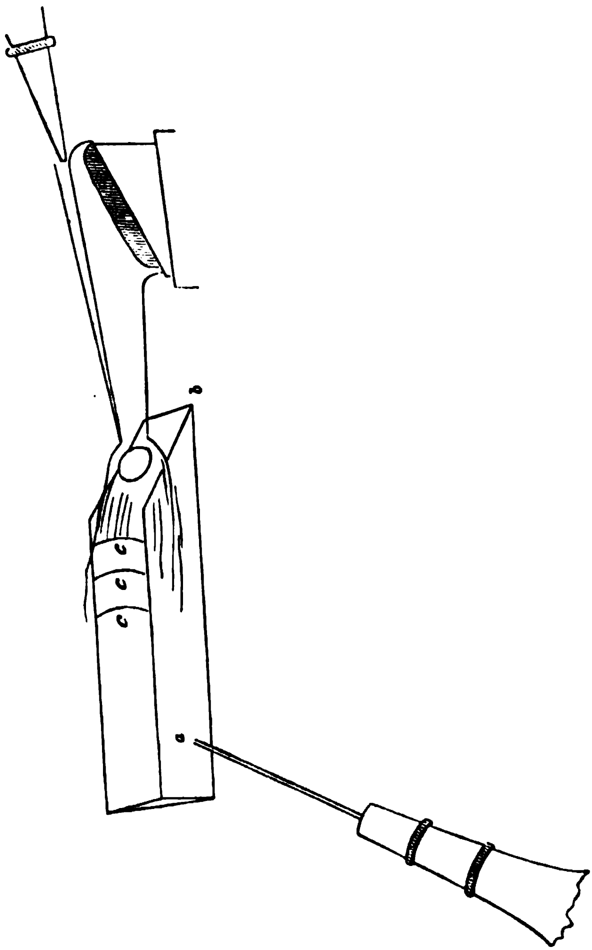
83. This is better performed on such a platinum tray held by steel-legged forceps than in the ordinary manner on charcoal or in a glass tube. By mixing a little common rust or lime with arsenic or antimony, the most timid operator need not fear injury to his platinum, which, however, it is far better to spoil than to lose a single valuable reaction. The addition of iron sesquioxide has another advantage; for it will be observed that the antimonial oxide mixed with it is deposited on the upper steel leg of the forceps unchanged as a *white* sublimate, but the arsenic oxide as an *orange* one, in consequence, it is presumed, of the ability of the latter to carry up a portion of the red iron oxide with it. This would appear to afford a valuable distinction between these two metals in toxicological cases, which even Marsh's test does not give.

84. If flour sulphur be treated in this way, and the upper leg of the forceps be sufficiently high to be out of its blue flame, for of course it ignites, the steel leg will not be found to have changed further than by being covered with a yellow varnish, which is apparently distilled sulphur; but if the leg be plunged warm into water, it will appear white from the number of bubbles caused by some chemical action upon it. If instead of sulphur only, a mixture of sulphur and any inorganic substance containing nitrogen, as gunpowder (only of course, for this purpose, that must be well watered and ground into a paste), be used, we shall find the forceps apparently unchanged; but after being plunged warm into water, the upper leg will come out perfectly *black*. No bubbles will be observed through the lens, but the leg, on drying, will be found covered with rust.

85. This curious reaction is also produced when sulphur is thus sublimated in company with such minerals as emit an empyreumatic or nauseous odour when heated in a matrass alone, as, *e. g.*, stinkstone. Such minerals, dissolved in a P glass, give also the nitrogenical reaction referred to in paragraph 48; and one of such (a black mineral found at Mussoorie in India, as hard as topaz, though consisting apparently only of silica, and

emitting a smell of burnt fat when heated in a matrass) produced, when dissolved to a supersaturated extent in borax, a fine cerulean-blue bead of extreme hardness*. Both this mineral and gunpowder (the latter alone, the

Fig. 8.



former combined with sulphur) were found, when ground with water in agate mortars, to give them a deep violet tint, best seen by transmitted light, which is quite ineradicable even by the strongest acids†.

86. If sulphur and clear drinking water be heated as at *a c*, fig. 7, a white sublimate is deposited on the polished leg of the forceps, similar in appearance to that afforded by fusing chloride of sodium. It was thus extracted, though in extremely minute quantity, from even distilled water. To observe

* Can the blue colour of the *sapphire* be due to this fact?

† *Nitrate* of silver gives the agate a purplish-black tint.

slight sublimates, the steel legs of the forceps should be polished bright before use, and then pointed downwards near a window, when the slightest deposit will affect the appearance of the shining surface through a lens.

87. The distinction made by writers on the "blowpipe" between sublimates of metals by means of the different distances at which they are deposited from the assay on charcoal, is apparently based on an error, as these seem to be due chiefly to the violence or weakness, as the case may be, of the superposed blast (paragraph 7). This may be proved by causing the sublimate of the *same* metal, as antimony, to be deposited at different distances, as shown in fig. 8 at *c c c*, through modified blowing.

88. The same figure shows the manner in which it is proposed to utilize the whole effects of the hydrocarbonous pyrocone for substances which cannot be conveniently supported on platinum wire, as metals or alloys, by which the defects of large pieces of charcoal in breaking up and spreading out the pyrocone, and in absorbing and wasting so large an amount of heat, may be avoided. This is by sawing charcoal paste (made of C powder, flour, and water according to the directions given in Plattner's work) into parallelograms about 1½ inch long by a ¼ inch deep, and bevelling or slanting off one end as at *b*. This is called a "charcoal mortar," and is supported by a common sewing-needle stuck into one side, as at *a*, figs. 8 and 10. A cavity is scooped at first in the slanting face of the mortar with the point of a penknife, or, better, an implement like fig. 9, which is the representation of a broken drift. After some use the mortar burns away as shown in fig. 10; but no fresh cavity

Fig. 9.

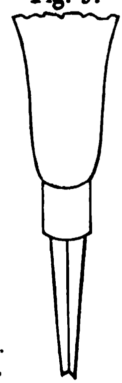
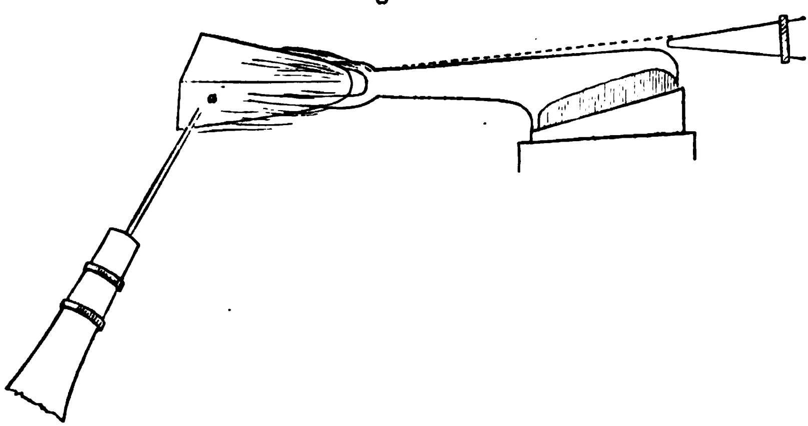


Fig. 10.



requires to be scooped, as the assay being hotter than the surrounding paste, burns a place for itself, while the great advantage is obtained by

the operator of being able to instantaneously cool and examine the assay at any time, by dipping the whole in a cup of water.

Aluminium foil as a support.

89. It has been found that pieces or strips of aluminium foil not *under* three inches long, withstand the strongest heat of the pyrogene without fusing as well as platinum does, over which the former metal possesses this great advantage, viz. that many metals, as gold, silver, lead, &c., or their alloys, may be fused *upon* it without the least fear of combination.

It is thus possible to use this beautiful metal as a support upon which to fuse most other metals, alloys, or metalliferous ores wrapped in a piece of soda paper, instead of upon charcoal—the advantages being cleanliness, portability, and even economy, for one strip will last out any number of pieces of charcoal. It is rapidly attacked, however, by chlorides and phosphates.

90. [Minerals heated in P. P. upon Al. foil afford extremely valuable indications of some oxides, which on charcoal would be fused and reduced; as, e. g., *Kupfer nickel* and *Speisscobalt*, which *thus* immediately yield a fine green oxide—"emerald nickel." (6th August, 1872.)]

XV. "On the Action of Electricity on Gases." By Sir B. C. Brodie, Bart., F.R.S., Hon. D.C.L. Oxon., late Wayneflete Professor of Chemistry in the University of Oxford. Received June 6, 1872.

(Abstract.)

This memoir, which is intended to be the first of three communications as to the action of electricity on gases, is devoted to the consideration of the changes produced by the action of electricity on oxygen gas as estimated by the changes thus effected in its chemical properties.

The memoir is divided into four sections.

Section I. contains an account of the methods employed for generating, collecting, and preserving the electrized gas, and also of the measuring-apparatus employed for estimating the changes in the volume of the electrized gas effected in the various experiments subsequently described.

The gas, carefully dried, was submitted to the action of electricity by causing a current of the gas to pass through the induction-tube of Siemens, the interior of which was filled with water or (where a low temperature was desired) with a saline solution. The tube was placed in a glass cylinder containing water or a refrigerating-mixture. The interior and exterior of the tube were respectively connected with the terminals of a powerful Ruhmkorff's coil. The electrized gas, after its passage through the induction-tube, was collected, in a gas-holder of peculiar construction, over concentrated sulphuric acid. It may be thus preserved for several hours without sensible variation in its properties.

The principle employed for the measurement of the gas in which it was

desired to estimate the changes in volume produced by the experiment, was the principle of pipette-measurement which has been so successfully employed by chemists for the measurement of liquids. In this way a considerable volume (say from 250 to 300 cub. centims.) may be measured with facility and precision. A definite volume of gas was thus always operated upon.

The gas having been measured in the pipette was drawn over by means of a mercurial aspirator, an instrument which served the double purpose of an aspirator and measurimg-apparatus. The principle of this aspirator was that originally employed in the apparatus of Regnault for measuring the volumes of gases, namely, the estimation of the pressure and temperature at which the gas occupied a known space, from which the volume of the gas at standard temperature and pressure was calculated. By means of this apparatus a change in the volume of the electrized gas, to the extent of about 1 part in 1000, could be accurately estimated; that is to say, after the calibration of the apparatus, 1000 volumes of gas as measured in the pipette were found to measure 1000·7 volumes in the aspirator. These numbers represent the errors of the experiment, and any differences in the volume of the gas beyond this limit must be considered to be due to the experiment to which the gas was submitted. The pipette and the aspirator were placed on a table, separated by an interval of about 8 or 10 inches.

In Section II. the results are given of passing the electrized gas through a solution of neutral iodide of potassium, also of heating the gas, of passing the gas over metallic silver, copper, gold, aluminium, and binoxide of manganese, and of the decomposition of a solution of binoxide of sodium effected by the passage of the gas, a quantitative estimation of the changes in the volume of the gas and the oxidation effected being in all cases made.

The precision attained in such experiments may be estimated by the results of the measurement of the gas before and after its passage through a solution of neutral iodide of potassiun. As the mean of eight concordant experiments, 100 cub. centims. of gas, as measured in the pipette, were found after the experiment to measure 99·93 cub. centims. in the aspirator. An oxidation was effected in the solution of neutral iodide of potassium equivalent to 3·77 cub. centims. of oxygen; that is to say, 3·77 cub. centims. of oxygen were thus removed from the gas without an appreciable variation in its volume. The volume of the gas thus absorbed by neutral iodide of potassium was in subsequent experiments assumed as the unit to which other analogous variations were referred.

When the electrized gas is passed through a solution of binoxide of sodium, an increment occurs in the volume of the gas. Thus in two experiments the increment in the volume of the gas, as estimated from the difference of the volumes in the pipette and the aspirator, was 1·93, 1·99, the "titre" of the gas (as just explained) being taken as 1; and the ratio of the sum of the oxygen lost by the binoxide of sodium (as estimated by titration of the solution of binoxide of sodium before and after the experiment with permanganic acid) and the titre of the gas to the titre of the gas

was in the same experiments 2·06, 2·17. In two other experiments this same ratio was 2·00, 2·08; the reaction being analogous to the decomposition of binoxide of sodium by ferrocyanide of potassium, and of binoxide of hydrogen by permanganic acid, previously investigated by the author.

Section III. comprises an account of the action of the electrized gas upon a solution of hydriodic acid, strongly alkaline hyposulphite of soda, polysulphide of sodium, and other substances.

In the case of the passage of the gas through a solution of hydriodic acid, the oxidation effected (after a certain degree of concentration of the acid has been attained) is exactly twice the oxidation effected by the same gas in a solution of the neutral iodide of potassium. The mean of 33 such experiments gave 1·99 as the amount of oxygen employed in the oxidation of the hydriodic acid as compared with the "titre" of the gas. The individual experiments exhibit no inconsiderable differences; but the probable error of the result, as estimated by the method of least squares, is 0·02; that is to say, from these experiments alone, without introducing any hypothetical considerations whatever, it is an equal chance that the true value of the ratio sought lies between the values 2·01 and 1·97. The value indicated by chemical theory is 2, with which theory, therefore, the experiments agree.

The action of the gas upon a strongly alkaline solution of hyposulphite of soda is precisely of the same character. The volume of gas was measured before and after the experiment, and a contraction was found to occur equal in amount to the "titre" of the gas. The mean of twenty-two experiments gave 1·03 as the amount of this contraction. The peculiar oxidizing properties of the ozone are entirely destroyed by its passage through the solution; and it is to be inferred that while the diminution in volume is equal to the "titre" of the gas, the oxidation effected in this, as in the previous case, is the same as that which would be effected by a volume of oxygen equal to twice the "titre" of the gas.

Experiments made with a solution of polysulphide of barium gave a similar result.

Section IV. comprises various experiments made with solutions of neutral and slightly alkaline hyposulphite of soda, with oil of turpentine, and with protochloride of tin.

The experiments with neutral and slightly alkaline hyposulphite of soda were conducted precisely in the same manner as the experiments described in Section III., with the strongly alkaline hyposulphite. The result, however, is very different, the contraction in this case being equal in amount to twice the "titre" of the gas. The mean of 17 experiments made with the neutral hyposulphite gave 2·02 as the value of this contraction, and the mean of 10 experiments made with the slightly alkaline hyposulphite gave for it the same value.

It is hence to be inferred that the oxidation effected in these cases is equal in amount to three times the oxidation effected by the same gas in neutral iodide of potassium.

Similar experiments made with oil of turpentine entirely confirmed the view of Soret as to the amount of contraction which the gas undergoes when acted upon by this substance, the mean of eight experiments giving 2·02 as the value of the contraction.

The investigation of the effect due to the action of the electrized gas upon protochloride of tin is attended with considerable difficulty, from the circumstance that a solution of protochloride of tin is readily oxidized by the action of pure oxygen. The difficulty was met in two ways, both of which led to the same conclusion,—namely, by applying a correction for the oxidation effected by the oxygen with which the ozone was associated, and by using very dilute solutions of the protochloride of tin in which this oxidation is reduced to a minimum. In two experiments conducted on the latter principle, and in which the oxidation, as well as the contraction, was experimentally determined, the value of the contraction was found to be 2·19 and 2·33, while the oxidation in the two experiments respectively was 3·12 and 3·07.

Using the notation employed by the author in a previous communication * to the Royal Society, and putting ξ^2 as the symbol of the unit (that is of 1·000 cub. centim. at 0° and 760 millims.) of oxygen, and putting $[\xi]$ as the symbol of that simple weight ξ transferred to the oxidized substance in the various oxidations effected by the ozone, and further assuming that ozone is to be regarded as some denser form of oxygen, to the unit of which the symbol ξ^{2+n} (where n is a positive integer) is to be assigned, the result of the total system of experiments of which the account is given in this memoir may be expressed, so far as regards the distribution of the matter of the unit of the ozone, in the various reactions by the general equation

$$(p+q)\,\xi^{2+n}=q\xi^2+(p(2+n)+qn)\,[\xi],$$

...e p, q, n are positive integers.

...investigation of the various hypotheses originating in this equation ...he conclusion that the hypothesis that the unit of ozone is com- ...ree simple weights, ξ, and is to be symbolized as ξ^3, is both ne- ...ufficient for the explanation of the total system of phenomena, ...her hypothesis of the order referred to is tenable.

...on the Atomic Weight of Thallium." By WILLIAM ... F.R.S. &c. Received June 18, 1872.

(Abstract.)

...ruary 1863, I had the honour to lay before the ... on the subject of the then newly discovered ... an account of its occurrence, distribution, ...m the ore, together with its physical

...tions," by Sir B. C. Brodie, Bart., F.R.S,.

characteristics and chemical properties; also I discussed the position of thallium among elementary bodies, and gave a series of analytical notes.

In the pages of the 'Journal of the Chemical Society' for April 1, 1864, I collated all the information then extant, both from my own researches and from those of others, introducing qualitative descriptions of an extended series of the salts of the metal. I propose in the present paper to lay before the Royal Society the details and results of experiments which have engrossed much of my spare time during the last eight years, and which consist of very laborious researches on the atomic weight of thallium.

SECTION I.

On the Determination of Atomic Weights.

In determining accurately the atomic weight of a metal that stands so high in the scale as thallium, difficulties and sources of error which are comparatively small with elements of low atomic weight are magnified to serious proportions, and require more than ordinary care for their elimination. When so large a proportion of the compound under analysis or synthesis consists of the body itself whose atomic weight is the one unknown quantity, it is evident that the almost unavoidable errors occasioned by impurity in the materials employed, the losses incident to imperfect manipulation, or the inaccuracies arising during the weighing from the omission of the corrections required by temperature, pressure, &c., will all find their way into the number which is finally considered to represent the atomic weight of the metal.

I have attempted two entirely different methods of arriving at the atomic weight of thallium. Had the results of these determinations differed materially, I should have extended the research to other methods; but as they so nearly agree, it appeared unnecessary to incur so great an additional expenditure of time and material with no reasonable prospect of getting any but confirmatory results. The first method, and that which I shall describe, consists in taking a known quantity of metallic thallium, dissolving it in nitric acid, and weighing the nitrate of thallium produced.

The second method consists in dissolving known quantities of sulphate of thallium in water, and ascertaining how much nitrate of barium is necessary to precipitate the sulphuric acid as sulphate of barium.

SECTION II.

Apparatus employed.

The absolute weight of any substance may be found from its apparent weight in an atmosphere of 30 inches of mercury, and from its apparent weight under, say, 25 inches of mercury. But the best weighings are undoubtedly one in air at ordinary pressure and temperature, and one in a

highly rarefied atmosphere,—it cannot be said *in vacuo*, owing to the difficulty of working under such a difference of pressure between the atmosphere of the balance and that surrounding it.

The Balances.

Two balances were used. That which I shall call the *air*-balance was made by Messrs. Keissler and Neu expressly for this work, and will clearly indicate a difference of 0·0001 of a grain when loaded with 1000 grains in each pan.

The second balance, which I shall call the *vacuum*-balance, is almost a duplicate of the first, of 14-inch beam, with agate knife-edges and planes, made by Œrtling. It is enclosed in a cast-iron case connected with an air-pump, and so arranged that I can readily weigh any substance in air of any desired density, the rarefaction being measured by a barometer-gauge.

At first it was attempted to put nearly the correct weight into the pan, and then make the final adjustment by means of the rider. It was, however, soon found that a nearer approach to accuracy is to introduce a certain weight, and then to alter the pressure of the air until the balance shows equilibrium. Two weighings at different degrees of atmospheric pressure, varying by a considerable interval, give data upon which to calculate what the weight would be in a perfect vacuum.

The Weights.

A set of weights as ordinarily supplied by even the best instrument-makers is never absolutely exact; however carefully they may be adjusted, the pieces of metal which respectively represent 1000 grains, 100 grains, 10 grains, &c. are only more or less approximations to the true weights. In most chemical analyses, the error arising from such inaccuracies in the weights used is so small in comparison to errors of manipulation, or to imperfections inherent in the chemical processes adopted, that it may generally be disregarded; but when the chemist has for his object the determination of an atomic weight, or is engaged in other researches demanding the highest refinement of accuracy which chemistry and physics can supply, then he is bound to neglect no correction which will increase the precision of the results.

The weights I employed were of platinum, made expressly for these investigations by Messrs. Johnson and Matthey. The platinum was quite pure; it was fused, cast, and then well hammered. The weights were adjusted by myself during May, June, July, and August 1864: they were first roughly adjusted, and then the specific gravity of each weight was taken. The weights were heated to redness in a bath of magnesia previous to ascertaining their specific gravity. The density of the larger weights was ascertained to the second place of decimals, and that of the smaller ones to the first place. The following are the results of the final adjustment, the weight *in vacuo* being calculated by the formula:—

W=weight in air,
w=weight in water,
a=sp. gr. of air as compared with water;

then

$$x=\text{weight } in\ vacuo=\frac{W-aw}{1-a},$$

where $a=0{\cdot}001225$, and
$1-a=0{\cdot}998775$.

Results of the Adjustment of Standard Grain-weights (Platinum set).

Weights.	True value in air 30 in. 62° F.	Weight of air displaced.	Volume in water of max. density.
grs.	grs.	grs.	grs.
1000·	1000·000000	0·058271	47·51
600·	599·998340	0·035533	28·97
300·	300·000240	0·017501	14·27
200·	199·998910	0·011664	9·51
100·	99·991420	0·005887	4·80
60·	59·993232	0·000483	2·84
30·	29·999991	0·001668	1·36
20·	19·999984	0·001104	0·90
10·	9·998477	0·000490	0·40
6·	5·998268	0·000355	0·29
3·	3·000469	0·000171	0·14
2·	1·999839	0·000113	0·10
1·	0·998980	0·000055	0·04
·6	0·602350	0·000035	0·03
·3	0·303600	0·000017	0·02
·2	0·203240	0·000011	0·01
·1	0·098110	0·000005	0·004
·06	0·061472	0·000003	0·003
·03	0·030561	0·000002	0·002
·02	0·022884	0·000001	0·001
·01	0·014097	0·000001	0·0004
*·01′	0·009997	0·000001	0·0004
*·01″	0·009967	0·000001	0·0004

The value of each weight in air, plus the weight of air displaced, is, of course, the weight *in vacuo*.

The Glass.

The flasks and vessels used were of the hardest Bohemian glass, and as thin as they could be employed. When practicable, vessels of old green German glass were used; neither this nor Bohemian glass is practically affected by reagents.

No cork or luting was employed in the distillations &c.; in most cases the apparatus was blown in one piece, and the operations performed in a vacuum. The apparatus, which was weighed, was entirely composed of glass suspended with platinum-wire loops: fingers were never allowed to touch it after the first weighing.

The weight of tubes, bulbs, and flasks, even of hard Bohemian glass, constantly diminishes when the glass is long heated in a spirit- or gas-

* Riders.

flame; this loss may amount to several thousandths of a grain in the space of two hours when a bulb of Bohemian glass 3 inches in diameter is exposed to a decided red heat in a gas-flame. Following the suggestion of Professor Stas, I have obviated this source of error by employing a bath of pure magnesia; and I find that the weight remains constant even at a nearly white heat. I have likewise employed baths of lime with similar satisfactory results.

The special apparatus that I have used are described in the processes in which they were required; I need scarcely say that in no case were materials of untried purity employed.

Improved Sprengel Vacuum-pump.

Before detailing the processes of the determination, it will be requisite to describe the means of producing a vacuum in the flasks and bulbs employed. In proceeding with the determinations, several additions and improvements have been made to the Sprengel pump as generally found in the laboratory. The apparatus, as thus arranged, is readily manageable, with certainty of obtaining a Torricellian vacuum.

SECTION III.

The Chemicals.

The detail of the processes of preparing the thallium and the reagents in a chemically pure state is too necessary to admit of useful abstraction.

SECTION IV.

Processes and Results.

The processes and manipulation necessary to the determination of an atomic weight are at all times difficult and delicate, but especially in the case of a metal such as thallium, so readily oxidizable. This strong tendency to combine with oxygen, renders the ordinarily exact processes of weighing out pure metals inapplicable to the present purpose. The chances of contact with the oxygen of the atmosphere must be reduced to a minimum, and to this end it was found desirable to work *in vacuo*. For this purpose the series of bulbs shown in fig. 11 have been blown. Pure thallium is introduced into *a*, the upper end of which is then sealed. The end *c* is also sealed up, and the horizontal tube *e* is connected to the Sprengel pump, and a vacuum obtained, the tube being then sealed at *f*. The thallium in *a* is now heated on a magnesia-bath to its point of fusion (561° F.), and when molten caused, by gently tilting the vessel, to flow by the narrow channel *d* into the lower bulb *b*, all oxide remaining in *a*. The channel *d* is now contracted before the blowpipe, and *a* removed. The bulbs then appear as in fig. 12, and after cooling are carefully weighed in air and *in vacuo*. The fine point of a blowpipe-flame, caused to impinge upon the end of the tube at *g*, softens the glass, and the air, endeavouring to force

its way into the bulbs, forms a capillary orifice. By heating the bulb, and immersing the orifice in the pure nitric acid, the acid is introduced into the bulbs *h* and the globe *b* until all the thallium has been dissolved. The most tedious part of the process then commences, the evaporation of the excess of free acid. For this purpose an apparatus is used of the form represented in fig. 13: *a* is the apparatus, connected by a wide tube (*b*) and a narrower glass tube with a Woulfe's bottle (*c*); this is in connexion with a Bunsen's water-pump (*d*), having 15-feet fall of water, and capable of producing an exhaustion equal to 10 inches of mercury. In the course of time the nitrate of thallium is left in the form of dry white crystals. The pump is then stopped, and air allowed to enter the apparatus by opening the pinch-cock, *e*, connected with the chloride-of-calcium tubes, *f*. The nitrate of thallium is then treated with a solution of oxalic acid to reduce any pernitrate that may be formed, the crystals dried, fused, dissolved in water, again allowed to crystallize, the evaporation of the water being repeated under diminished pressure. When there is no longer a loss of weight, the nitrate of thallium is finally weighed, the air being exhausted from the apparatus. The apparatus is now of the form shown in fig. 14, and is weighed at two different atmospheric pressures. The nitrate of thallium having been afterwards removed, the apparatus is alone weighed at two different pressures. There have thus been obtained:—

α. The weight of the glass + thallium.

β. The weight of the glass + nitrate of thallium.

γ. The weight of the glass alone.

Particulars of these weights are given in the next section.

SECTION V.

Calculation of the Results.

The formulæ by which I have calculated the weights from weighings at two different atmospheric pressures are given at length in the full description of the processes that I have the honour to submit to the Royal Society.

Collecting the data, we have:—

	grs.
True weight of thallium *in vacuo*	=183·790232
True weight of nitrate of thallium *in vacuo*	=239·646066
True weight of glass	=766·133831
(*a*) Weight of thallium according to true value of weights in air...........	=183·783921
(*b*) Weight of nitrate of thallium in air (1005·425937 − 765·814578)....	=239·611359
(*c*) Weight of glass &c. in air	=765·814578
Weights employed to balance (*a*)........	=183·8099
Weights employed to balance (*b*) (1005·4364 − 765·8081)........	=239·6283
Weights employed to balance (*c*)........	=765·8081

The reduction of the atomic weight from these data becomes a case of simple proportion; but the values found are absolute in so far only as the atomic weights of nitrogen and oxygen are correct. The atomic weights of nitrogen and oxygen have been usually represented by the numbers 14 and 16; but Professor Stas found these elements represented, according to observation, by

Oxygen (Θ_3)........................... $=47{\cdot}880$

Nitrogen .... $=14{\cdot}009$

or nitric acid $N\Theta_3=61{\cdot}889$. According to the old equivalents, $NO_6=62$.

Taking as data the series of weighings *in vacuo*, the quantity of nitric acid required to convert the thallium into nitrate is

$$(239{\cdot}646066-183{\cdot}790232=)\ 55{\cdot}855834 \text{ grs.}$$

We have, then, with Professor Stas's determination of the atomic weights of nitrogen and oxygen, the following proportion:—

Weight of nitric acid.		Weight of thallium.		Atomic weight of nitric acid.		Atomic weight of thallium.
55·855834	:	183·790232	::	61·889	:	x;

$$\therefore x=203{\cdot}642.$$

Let us see what would be the atomic weight of thallium if one or other of the corrections introduced into the above determinations had been omitted. The use of the old equivalent (=62) for nitric acid, with the data derived from the weighings *in vacuo*, gives

$$55{\cdot}855834 \ : \ 183{\cdot}790232 \ :: \ 62 \ : \ 204{\cdot}007$$

as the atomic weight; but I cannot admit this number to be so nearly correct as 203·642.

If we take the corrected weighings in air of ordinary density, we have, with $N\Theta_3=61{\cdot}889$,

$$203{\cdot}738.$$

With $NO_6=62$,

$$204{\cdot}103.$$

Accepting the uncorrected weights, observed in air, we have, with $N\Theta_3=61{\cdot}889$,

$$203{\cdot}162.$$

With $NO_6=62$,

$$204{\cdot}165.$$

The error of the last deduction; $+{\cdot}523$, is sufficiently large to show the necessity of neglecting no precaution in chemical manipulation, especially in a determination of this character. The largeness of these errors have an immediate bearing upon quantitative analysis; for they show that from data ordinarily given, very varying results may be obtained. Chemists have to deal with much smaller quantities than a quarter per cent., particularly in organic analysis, where such a difference from the truth may lead to very erroneous reasoning.

RESULTS.

Ten results of the most trustworthy weighings (with $N\Theta_3=61\cdot889$) are* :—

Determination.	True weights *in vacuo.* Weight of thallium taken.	Weight of nitrate of thallium + glass.	Weight of glass.	Calculated atomic weight from these data.
	grs.	grs.	grs.	grs.
A.	497·972995	1121·851852	472·557319	203·666
B.	293·193507	1111·387014	729·082713	203·628
C.	288·562777	971·214142	594·949719	203·632
D.	324·963740	1142·569408	718·849078	203·649
†E.	183·790232	1005·366796	766·133831	203·642
F.	190·842532	997·334615	748·491271	203·636
G.	195·544324	1022·176679	767·203451	203·639
H.	201·856345	1013·480135	750·332401	203·650
I.	295·683523	1153·947672	768·403621	203·644
K.	299·203036	1159·870052	769·734201	203·638

I wish it to be noted that I have made determinations with various weights of thallium. In ordinary analysis chemists are satisfied to take 5 or 10 grains of the substance under investigation: here I have gone to the very highest weight that can be entrusted, with safety, to the balance. The lowest weight of thallium taken is 183·790232 grains, the heaviest 497·972995 grains, the remaining determinations varying between these limits. It is hardly necessary to say that the purpose has been to eliminate the error arising from manipulation with small quantities, and to produce such variety in the results as renders the chances of coincidence of very small value.

Let me now tabulate the results of the determinations, with the view to ascertain severally their degree of approximation to the arithmetic mean :—

A.	203·666	+·024
B.	203·628	—·014
C.	203·632	—·010
D.	203·649	+·007
E.	203·642	+·000
F.	203·636	—·006
G.	203·639	—·003
H.	203·650	+·008
I.	203·644	+·002
K.	203·638	—·004

The arithmetic mean of the ten observations is

$$a=\frac{2036\cdot424}{10}=203\cdot642.$$

* It should be noted that the arithmetic mean of *all* the readings, including the highest as well as the lowest result, in which doubt might arise as to success in manipulation, is 203·6.

† Fully illustrated in the paper.

The probable error is 0·0022; and the probability that the true value lies between 203·632 and 203·652 is 0·99808, certainty being represented by unity.

I may therefore conclude that the atomic weight of thallium is, within the limits of error (as small as possible) of observation,—

203·642.

Professor Stas has shown the hypothesis of Prout—that the atomic weights of the elements are severally multiples of the atomic weight of hydrogen—to be without the corroboration of experimental result. This view of the hypothesis is further borne out in the present investigation; for the number 203·642 cannot, within the limit of what has been shown to be the probable error, by any liberty be made to follow the hypothesis. Without doubt, when the atomic weights of all the metals are redetermined according to the standard of recent scientific method, it will be found that there are more exceptions to the hypothesis than commonly considered. Marignac gives, in his confirmatory discussion of Stas's experiments, and in his own results with calcium (40·21), lanthanum (94·13), strontium (87·25), analogous opposed evidence, as in the case of the weight found for thallium.

I have thus striven to eliminate all erroneous influence in the number I submit to the Royal Society as the atomic weight of thallium; and I shall be amply rewarded for my long labour if I can know that the determination has secured to researches of this character a nearer approach to the standard of truth.

The drawings appended to the complete paper comprise copies of:—

Fig. 1. The case of the vacuum-balance.
2. The improved Sprengel vacuum-pump.
3. Apparatus for preparing pure water under diminished pressure.
4. Apparatus for preparing nitric acid under diminished pressure.
5. Apparatus for preparing oxalic acid under diminished pressure.
6. Apparatus for preparing sulphuric acid under diminished pressure.
7. Stoppered tube in which thallium was weighed in the early determinations.
8. A series of bulbs*.
9. Another series of bulbs*.
10. Apparatus for sealing up thallium in hydrogen.
11. The first stage of selected glass apparatus*.
12. The second stage of selected glass apparatus*.
13. The Bunsen water-pump and drying-apparatus.
14. The third stage of selected glass apparatus*.

* For the conversion of thallium into nitrate of thallium.

XVII. "On the Spectrum of Nitrogen." By ARTHUR SCHUSTER, Student at the Physical Laboratory of Owens College. Communicated by BALFOUR STEWART, F.R.S. Received June 13, 1872.

1. *Introductory.*—The formation of the different spectra which one gas is said to exhibit, when examined under different conditions, still remains one of the most obscure points of spectrum analysis. In 1864, when Plücker and Hittorf published their researches "On the Spectra of Ignited Gases and Vapours, with especial regard to the different Spectra of the same elementary gaseous substance"*, they drew attention to the close resemblance in character of the band-spectra which certain metals yield at a comparatively low temperature to the band-spectrum of nitrogen and sulphur. Roscoe and Clifton, n their paper "On the effect of increased Temperature upon the nature of the Light emitted by the Vapour of certain Metals or Metallic Compounds" †, rendered it probable that the band-spectra of the metals belonged really to the oxides. The two spectra of nitrogen were not, however, examined from that point of view, but, on the contrary, they were made the starting-point of new investigations by Wüllner, who came to the conclusion that certain gases may give even more than two different spectra. Ångström ‡, expressing his doubts about the trustworthiness of Wüllner's experiments, says in a note: "As regards the spectra which are usually attributed to nitrogen, I mention here, as a general fact, that it is my conviction that the fluted bands which are so characteristic of the oxides of metals are never found in spectra of elementary gases."

I propose to show, in the present communication, (1) that pure nitrogen gives only one spectrum; (2) that this is the line-spectrum; (3) that the fluted spectrum of the first order is due to oxides of nitrogen, formed under the influence of the electric spark.

2. *First experiment.*—The first experiment which I made with respect to the spectrum of nitrogen, was a repetition of an experiment of Secchi, who found that in different sections of the same tube three different spectra of nitrogen might be obtained. A vacuum-tube was made exactly according to Secchi's description, filled with nitrogen and exhausted. To my astonishmemt the tube showed, even in its widest parts, only a spectrum of lines. No accurate measurements were taken at the time, but the spectrum was no doubt that of the second order described by Plücker. Suddenly, and while I was looking through the spectroscope, the spectrum changed, and the well-known fluted bands appeared. The first spectrum could now easily be obtained by introducing a Leyden jar in the circuit. The spark very soon ceased to pass, and it was then found that the tube was leaking.

3. The behaviour of this tube at once suggested the idea that the presence of air was necessary for the formation of the fluted spectrum. It is

* Philosophical Transactions, vol. clv. p. 1. † Chemical News, vol. v. p. 233.

‡ Comptes Rendus, Aug. 1871.

well known that the oxides of nitrogen are formed on passing the electric spark through air, and the resemblance which this spectrum bears to the spectra of the oxides of metals rendered this view probable. In order to test it, a series of experiments were made, showing that,—

(*a*) Whenever the fluted spectrum appeared, it could be shown that traces of oxygen were present;

(*b*) Whenever there was a certainty of no oxygen being present, the spectrum of the second order appeared under all pressures and in all temperatures.

In order to free the nitrogen from every trace of oxygen, I adopted, at Dr. Stewart's suggestion, the plan of heating a small piece of sodium placed in the vacuum-tube. This proved in each case perfectly satisfactory; for when every trace of oxygen had thus been absorbed, the line-spectrum alone was invariably obtained*.

4. *Wave-length of the two spectra.*—There is no possibility of confounding the two spectra. The fluted spectrum is well known by its beautifully shaded violet bands; but in order to exclude any possibility of error, their position was read off on the reflecting scale of the spectroscope; the measurements were reduced to wave-lengths, and the following numbers obtained for the least refrangible end of the bands in tenth metres†:—

Fluted Spectrum.	
5129	4436
4981	4390
4649	4318
4556	4237

As the measurements were taken merely for the sake of reference, they do not lay claim to great accuracy.

The true spectrum of nitrogen is easily recognized by a very bright green line followed at a small distance towards the more refrangible parts by a green band; it also contains some violet bands, which are not shaded. The position of the principal lines was read off; their wave-lengths, as determined by Dr. Marshall Watts from the measurements made by Plücker, are as follows:—

Line-spectrum.				
6243	5767		4214	band
6176	5666		4199	
6087	5164	(the green line)	4184	band
6051	4894		4170	
5908	4644			

* The formation of the fluted spectrum does not imply that all the nitrogen in the tube has been oxidized; it has been remarked by different observers, and especially noticed by Plücker, that when the spark passes through a mixture of two gases, the spectrum of one only is often seen.

† A tenth metre, according to Ångström, means a metre divided by 10^{10}.

5. *Description of apparatus.*—The tubes generally used had two pockets, A and B, into which small pieces of metallic sodium were introduced by means of the tubes C and D. The tube C was connected with the receiver

containing the nitrogen, whilst the tube D was connected with the air-pump. The nitrogen was generally prepared by the combustion of phosphorus in air. After a few hours' standing, when all the phosphoric acid formed had been absorbed, the gas became quite clear and was ready for use. This mode of preparation, it is true, does not give the nitrogen very pure; but as my object was to get the nitrogen free from oxygen, and this was easily obtained by means of the absorption by sodium, the method was found sufficient. Other modes of preparing the nitrogen were tried, such as passing air over red-hot copper or the decomposition of ammonia by chlorine, but the same results were invariably obtained. The air-pump used was that of Carré's freezing-machine, with which pressures down to 2 millims. could be easily obtained. When the pressure was measured, a T-shaped tube was employed, one side of which was connected with the Geissler's tube, the other with the pump, while the mercury was drawn up in the longer part of the tube; its height was read off and compared with a barometer. I now pass to the description of the experiments.

6. *Method of experimenting.*—When the air in the vacuum-tube had been exhausted, the communication with the receiver containing the nitrogen was opened, and the gas was allowed to pass through it for some time while the pump was being worked. The tubing connecting the tube with the receiver was then clamped air-tight, and the tube was exhausted.

The electric spark in passing through it exhibited a violet colour, and gave the spectrum of fluted bands:

5129	4436
4981	4390
4649	4318
4556	4237

The sodium was next heated until it presented a clean metallic surface. The light which the tube now emitted was bluish white, and much fainter than before; and the whole appearance of the spectrum had changed to that of the second order with its characteristic green line. It was, however, found that the pressure in the tube had slightly increased, owing most likely to the vapour of the sodium present; and on bringing the mercury to its former level, the spectrum became brighter, but remained the same in character. New nitrogen was then led into the tube, and after exhaustion the old fluted spectrum again appeared; this was, however, at once changed into that of lines by heating the sodium. This process was

repeated several times in succession, but invariably with the same result. I have in my possession two tubes sealed off under 2 millims. pressure, one without sodium, showing the fluted bands, the other containing sodium, showing the spectrum of lines. Two other tubes, sealed off under 15 millims. pressure, show the same thing. I have repeatedly convinced myself that, from the highest pressure under which the spark of the induction-coil passes to the lowest pressure which I could obtain with an ordinary air-pump, pure nitrogen invariably gave one and the same line-spectrum. Once, when I intended to seal a tube off under higher pressures, it was found that the sodium was not sufficient to absorb all the oxygen present, so that a sort of mixture of the two spectra was seen. Such a mixture was often observed by Plücker and Wüllner at the point where one spectrum changed into the other; it is characterized by the green line of nitrogen and the fluted violet bands at the same time.

The tube showing the mixture at 15 millims. pressure was gradually exhausted, but the spectrum remained exactly the same. If the formation of the two spectra depends merely upon the pressure or temperature to which the gas is subjected, how can a mixture of the two spectra, indicating a state of transition, exist under so entirely different pressures and different temperature?

In order to ascertain whether nitrogen even carefully prepared contains oxygen, a drop of a solution of iodide of potassium and starch was introduced into the tube; after the spark had passed for a few seconds only, the liquid was coloured blue—showing either the formation of oxides of nitrogen or of ozone, but at any rate the presence of oxygen.

7. *Spectrum of oxides of nitrogen.*—I tried to obtain the spectra of the different oxides of nitrogen; they all give the same fluted spectrum, and I could get no information as to which particular oxide the fluted spectrum is due: this is, however, easily understood if we remember that it is just as difficult to prepare the oxides of nitrogen free from oxygen as pure nitrogen itself; so that the oxide giving the spectrum in question will always be formed. I have, however, convinced myself that the absorption-bands of nitrous acid gas are not coincident with the bright bands of the spectrum; and it is probable that the spectrum is due to nitric oxide, this being the most stable of all the oxides of nitrogen.

I may add that one of the tubes containing the sodium and showing the lines one day cracked, and then at once showed the violet bands. This fact will not be easily explained by the assumption that the fluted spectrum belongs to a lower pressure and lower temperature than the spectrum of lines.

I propose to subject the different spectra of the remaining gases to a careful examination.

The above experiments were made in the Physical Laboratory of Owens College, Manchester; and I have to thank Professors Balfour Stewart and Roscoe for many valuable suggestions.

XVIII. "On the Combined Action of Heat and Pressure upon the Paraffins." By T. E. Thorpe and John Young, of the Andersonian University, Glasgow. Communicated by Prof. Roscoe, F.R.S. Received June 5, 1872.

(Abstract.)

The authors first refer to their preliminary communication read before the Royal Society on March 9, 1871, in which they described the combined effect of heat and pressure upon the solid paraffins. They showed that when these substances are exposed to a high temperature in a closed vessel, they are almost completely resolved, with the evolution of but little gas, into hydrocarbons which remain liquid at the ordinary temperature. In the present communication they describe how this transformation may be easily effected on the small scale. A few grams of ordinary paraffin are sealed up in a piece of strong combustion-tubing, bent in the form of the letter V; the tube is securely surrounded by strong wire gauze, and the limb containing the paraffin is gently heated along its entire length in a gas combustion-furnace. If the heat is properly regulated, the paraffin rapidly distils over and solidifies in the cold portion of the tube. The gas-flames are then turned down, the tube reversed, and the paraffin again distilled. After a very few repetitions of this process the paraffin acquires the consistency of butter, and the warmth of the hand is sufficient to liquefy it; and after about a dozen distillations, the greater portion of the substance remains permanently liquid. It seems to be absolutely necessary that the paraffin should thus be distilled over and condensed; by merely heating it in the tube in such a manner that the condensed vapours flow back again upon the heated portion, the liquefaction is never accomplished. It appears that only paraffins boiling at an extremely high temperature, and those usually solid under ordinary conditions, are thus susceptible of decomposition. The readiness with which they yield liquid hydrocarbons appears to depend upon the complexity of their constitution.

The authors have not determined with certainty the limits of the decomposition, but they find that the mixed paraffin and olefine, boiling at about 255°, may be repeatedly distilled backwards and forwards in a sealed V-tube without suffering the slightest change.

The authors have repeated the process of conversion of solid paraffin into liquid products upon a large scale, in the hope of obtaining an insight into the constitution of the higher members of the $C_nH_{2n}+2$ series of hydrocarbons. The paraffin employed was obtained from shale; it melted at 46°, and had a specific gravity (when solidified under pressure) of 0·906 at 13°. Its composition was:—

Carbon	85·14
Hydrogen	14·81
	99·95

The apparatus employed is fully described in the original memoir. $3\frac{1}{2}$ kilograms of paraffin yielded about 4 litres of liquid made up of hydrocarbons boiling

	litres.
Below 100°	0·3
From 100–200°	1·0
From 200–300	2·7
	4·0

A considerable quantity of substance which could not be distilled within the range of the mercurial thermometer remained in the retort, and solidified on cooling. By repeated recrystallization from ether, this substance was obtained of a constant melting-point (41°·5). Its composition was:—

Carbon	85·19
Hydrogen	15·34
	100·53

The action of bromine upon this body showed that it was a member of the $C_nH_{2n}+2$ series. Heated in a sealed tube in the manner above described, it was readily split up into liquid hydrocarbons, which were shown to be mixtures of hydrides and olefines.

The four litres of liquid were submitted to a systematic fractional distillation over sodium; the greater portion of the operation was effected in an apparatus after Warren's design. The following fractions were thus isolated (boiling-points uncorrected):—

(1) at	35°– 37°	(7) at	193°–195°
(2)	65– 70	(8)	212–215
(3)	94– 97	(9)	230–235
(4)	122–125	(10)	252–255
(5)	145–148	(11)	273–276
(6)	170–172	(12)	290–295

By means of bromine all these fractions were shown to be mixtures of hydrides and olefines; and an attempt was made in each to determine, by means of this reagent, the relative quantities of the two series of hydrocarbons. The method by which this was accomplished is detailed in the original memoir. It was shown that in the lower fractions (up to 200°) the amount of hydride was sensibly equal to that of the olefine. Thus the fraction boiling at 65–70° was shown to be a mixture of hexane and hexylene in equal proportions; the fraction at 94–97° was also made up of heptane and heptylene in equivalent amounts. In the higher fractions, however, there is a decided increase in the amount of the hydride present.

After treatment with bromine the hydrides could be isolated in the pure state. The authors obtained:—

	Boiling-point.	Sp. gr.
Pentane	37°	
Hexane	67°– 68°	0·6631 at 18° C.
Heptane	97– 99	0·6913 „ 18·5
Octane	122–125	0·7165 „ 15·6
Nonane	147–148	0·7279 „ 3·5

From their specific gravities and boiling-points these hydrides in all probability belong to the series of normal paraffins.

$$CH_3—CH_2 \ldots\ldots\ldots\ldots\ldots\ldots (n\,CH_2)—CH_3.$$

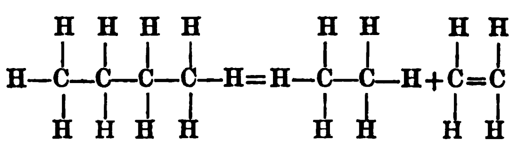
The authors have established the existence of the olefines, not only by the action of bromine, but also by the preparation of a number of brominated derivatives. They have obtained :—

	Boiling-point.	Sp. gr.
$C_5H_{10}Br_2$	184°–188°	
$C_6H_{12}Br_2$	195–200	1·5967 at 20°
$C_7H_{14}Br_2$	decomposes	1·5146 „ 18·5
$C_8H_{15}Br$	185–190	
$C_8H_{16}Cl_2$	about 235° with decomposition.	
$C_9H_{17}Br$	208–212	

They have also prepared a few derivatives from the hydrides, and studied the action of nitrogen tetroxide upon the mixture boiling at 122–125°.

The mode of decomposition of the paraffins under the influence of heat and pressure appears to be general for the higher terms of the series of normal hydrocarbons.

If the paraffins be represented constitutionally, by linking together the carbon atoms in a single chain, the simultaneous formation of hydride and olefine obviously arises from the loosening of the affinities of the CH_2 groups. Under the influence of heat, these groups become disassociated, and recombine to form saturated hydrocarbons. Assuming, for the sake of simplicity, that this decomposition may occur so low down in the series as in the case of butane, it might be thus represented :—



The authors have but little direct evidence to offer as to the exact manner of this decomposition—whether it is attended by the gradual elimination of ethylene, a hydride containing a greater number of carbon atoms being left behind, or whether the paraffin is at once split up into

hydride and olefine containing an equal number of carbon atoms as in the above equation. Neither supposition is exactly substantiated by experiment. If the action of heat gave rise to the former mode of decomposition, we ought to obtain a larger quantity of ethylene after prolonged heating, especially when the liquefied portion is rich in hydrocarbons of low molecular weight; but it has already been pointed out that the process of liquefaction is accompanied with the production of comparatively little gas. On the other hand, an examination of the amounts of bromine required to render the hydrocarbons boiling above 200° permanently red, shows that the proportion of hydride to olefine in the several mixtures becomes gradually larger as the molecular weight increases.

It would doubtless have been interesting to have determined the relative amounts of the twelve fractions isolated from the decomposed paraffin; but when it is considered that their separation was only effected after several thousand distillations, it will be evident that the quantities obtained after such prolonged treatment can afford no real indication of the amounts present in the original liquid. It appears, however, that the amounts of liquid boiling at 94–97° and 122–125° were but slightly, if at all, less than the quantities boiling at 252–255° and 273–276°.

XIX. "On the Echinidea of the 'Porcupine' Deep-sea Dredging-Expeditions." By Prof. Wyville Thomson, LL.D., D.Sc., F.R.S. Received June 15, 1872.

(Abstract.)

The deep-sea dredging-cruises of H.M. Ships 'Lightning' and 'Porcupine' during the summers of 1868, 1869, and 1870 in the North Atlantic, were comprehended within a belt 1500 miles in length by from 100 to 150 miles in width, extending from the Færöe Islands along the northern and western coasts of Scotland and Ireland and the coasts of Portugal and Spain to the Strait of Gibraltar. In this area fifty-seven successful hauls of the dredge were made during the three summers in water exceeding 500 fathoms in depth, sixteen beyond 1000 fathoms, and two beyond 2000 fathoms.

Even at the latter extreme depth Echinodermata appeared to be abundant. At 2435 and at 2090 fathoms all the Echinoderm orders were represented—the Echinidea by a small variety of *Echinus norvegicus*, D. & K., and a young example of *Brissopsis lyrifera*, Forbes; the Asteridea by a species of the genus *Archaster*; the Ophiuridea by *Ophiocten sericeum*, Forbes, and *Ophiacantha spinulosa*, M. & T.; the Holothuridea by *Echinocucumis typica*, Sars; and the Crinoidea by a very remarkable new form of the Apiocrinidæ, which has been described under the name of *Bathycrinus gracilis*, Wy. T. From 2000 fathoms upwards the number of Echinoderms seems to increase rapidly; but this apparent increase may possibly be

due to our wider knowledge of the fauna of the shallower water; from 300 to 800 fathoms along the coast of Britain many species of all the orders are enormously abundant, so much so as to give a very marked character to the fauna of that special zone. Several of these species, such as *Cidaris papillata*, Leske, *Toxopneustes drobachiensis*, Müller, *Echinus norvegicus*, D. & K., *Astropecten tenuispinus*, D. & K., *Archaster Parellii*, D. & K., *A. Andromeda*, M. & T., and *Euryale Linkii*, M. & T., have been long known to inhabit the deep water of the British area, and form part of a fauna which will be probably found to have a very wide lateral extension at temperatures whose minimum ranges from 0° C. to +2° C., a fauna which crops up, as it were, within the ordinary limits of observation in the seas of Scandinavia, and which has consequently been carefully studied by the Scandinavian naturalists.

Another group of species, including *Tripylus fragilis*, D. & K., *Ctenodiscus crispatus*, Retzius, *Pteraster militaris*, M. & T., *Amphiura abyssicola*, Sars, *Antedon Eschrichtii*, O. F. Müller, and several others, are members of the same fauna described from localities in the seas of Scandinavia and Greenland, but not hitherto known as British. A third section, consisting of a number of undescribed Echinideans, Asterideans, and Ophiurideans, may probably also belong to this fauna; while a fourth group, likewise undescribed, and including such forms as *Porocidaris*, *Phormosoma*, *Calveria*, *Pourtalesia*, *Neolampas*, *Zoroaster*, *Ophiomusium*, *Pentacrinus*, *Rhizocrinus*, and *Bathycrinus*, would rather appear to be referable to a special deep-sea fauna of which we as yet know only a few examples, and with whose conditions and extension we are unacquainted. This abyssal fauna is of great interest, inasmuch as nearly all the hitherto discovered forms referred to it show close relations to family types of Cretaceous or early Tertiary age, and hitherto supposed to be extinct.

Twenty-seven species of Echinidea were procured during the cruises of 1868, 1869, and 1870, off the coasts of Britain and Portugal, at depths varying from 100 to 2435 fathoms.

Cidaridæ.

Cidaris, Lamarck.

1. *C. papillata*, Leske.

Occurs in enormous numbers on gravel at depths from 100 to 400 fathoms, from Færöe to Gibraltar, and small-sized examples are frequent down to 1000 fathoms. This is a variable species, and every possible link may be shown between the typical *C. papillata*, Leske, and *C. hystrix*, Lam. I have no hesitation, after examining many hundreds of specimens, in fusing the two forms into one species.

2. *C. affinis*, Stokes.

This is a pretty little species, and apparently distinct, although it is sometimes not easy to draw the line between it and small forms of *C. papillata*.

It occurs abundantly in the Mediterranean, and locally off the coast of Portugal.

Porocidaris, Desor.

This genus was established by Desor chiefly on a character which I cannot regard as of great importance, and which is absent in the present species, a row of small holes surrounding the tubercles of the primary spines in the scrobicular areæ. From the description these holes seem to be nothing more than complete perforations, owing to imperfect calcification, in the position of the depressions which frequently occur in the scrobiculæ of the Cidaridæ for the insertion of the muscles of the spines. Along with this character, however, there were some others of greater value, a very remarkable paddle-like form of the spines surrounding the mouth, and a tendency to coalescence in the scrobicular areæ. These characters are well marked in the species described. This genus has hitherto only been found fossil—a few detached plates and some of the characteristic spines in the Nummulitic bedsof Verona and Biarritz, and some spines referred to the genus, on account of their having the same singular form, in the Lower Oolite of Frick.

1. *P. purpurata*, n. sp.

Four examples from depths from 500 to 600 fathoms off the Butt of the Lews.

ECHINOTHURIDÆ.

I think it due to the memory of the late Dr. S. P. Woodward to adopt as the type of this very distinct and remarkable family the genus *Echinothuria*, which he described with singular sagacity from one or two imperfect specimens from the White Chalk. The Echinothuridæ are regular urchins with depressed tests, rendered perfectly flexible by the whole of the plates, both ambulacral and interambulacral, being arranged in imbricating rows, the interambulacral plates overlapping one another from the apex to the mouth, and the ambulacral plates in the opposite direction. The margin of the peristome is entire, and the peristomial membrane is covered with imbricated scales, through which the ranges of double pores and ambulacral tubes are continued up to the edge of the mouth as in *Cidaris*. The ambulacral plates are strap-shaped, and the pores trigeminal; the two inner pairs of each arc pass through small accessory plates intercalated between the ambulacral plates, and the third pair, remote from the others, pass through the end of the ambulacral plate. The dental pyramid is broad and low, and the teeth are simply grooved as in *Cidaris*. The two divisions of the tooth-socket are not united by a closed arch; the ambulacral tube-feet on the oral surface are provided with suckers, while those on the apical surface are simple and conical.

Phormosoma, n. g.

Plates overlapping slightly and forming a continuous shell, the corona coming to a sharp edge at the periphery, and the upper surface of body differing greatly in character from the lower.

1. *P. placenta*, n. sp.

One example from 500 fathoms off the Butt of the Lews; several fragments from deep water in the Rockall Channel.

Calveria, n. g.

Plates overlapping greatly in the middle line of the ambulacral and interambulacral areæ. Plates narrow, and leaving fenestræ between them which are filled up with membrane. Character of the peristome with regard to the distribution of spines, the structure of the pore-areæ, &c. nearly uniform from the apex to the edge of the peristome.

1. *C. hystrix*, n. sp.

Fenestræ between the plates small. Colour a nearly uniform rich claret. One specimen in deep water off the Butt of the Lews.

2. *C. fenestrata*, n. sp.

Plates narrower than in the last species, and fenestræ wider. Of a pale grey colour, with bands of chocolate radiating from the apical pole. Two specimens from the coast of Portugal, and fragments in deep water off the south and west of Ireland.

ECHINIDÆ.

Echinus, Link.

1. *E. melo*, Lam.

One or two small specimens off the coast of Portugal.

2. *E. Flemingii*, Ball.

The large typical form of this species was met with in deep water off the Shetlands, but not abundantly.

3. *E. rarispina*, G. O. Sars.

4. *E. elegans*, D. & K.

5. *E. norvegicus*, D. & K.

The last three are critical species; and although the extreme forms are very dissimilar, in a large series there are so many intermediate links, that it is difficult to tell where the one begins and the other ends. It is possible that they ought to be regarded as varieties, and lumped together under Lamarck's name, *E. acutus*.

6. *E. microstoma*, n. sp.

Although I have great hesitation at present in proposing an addition to the genus *Echinus*, I feel compelled in the meantime to separate this very distinct form with a thin depressed test, a remarkably large periproct, and a small peristome with the edge markedly curved inwards and a uniform vivid red colour. *E. microstoma* is very abundant from 150 to 400 fathoms off the west coasts of Scotland and Ireland.

Sphærechinus, Desor.

1. *S. esculentus*, L., sp.

A marked variety, with a tall narrow test and white spines, in deep water.

Toxopneustes, Agassiz.

1. *T. drobachiensis*, Müller.

Of this species it seems to me that *T. pictus*, Norman, and *T. pallidus*, G. O. Sars, can only be regarded as varieties. It is generally distributed at depths beyond 100 fathoms.

2. *T. brevispinosus*, Risso, sp.

Shallow water on the coast of Spain.

Psammechinus, Agassiz.

1. *P. miliaris*, Lam., sp.
2. *P. microtuberculatus*, Ag.

CASSIDULIDÆ.

Neolampas, A. Agassiz.

This genus, with a nearly central pentagonal mouth and a tolerably distinct floscelle, with the anal opening at the bottom of a deep posterior groove excavated in a kind of projecting rostellum, with narrow ambulacral areæ and a small compact group of apical plates, must be referred to the Cassidulidæ; but it differs from all known genera of the family, living or extinct, in having no trace of a petaloid arrangement of the ambulacra, which are reduced on the apical surface of the test to a single pore penetrating each ambulacral plate, and thus forming a double row of alternating simple pores for each ambulacral area.

1. *N. rostellatus*, A. Ag.

I believe I am correct in referring to this species a single specimen dredged at the mouth of the English Channel. It is upwards of an inch in length, and therefore nearly double the size of the examples procured by Count Pourtales in depths from 100 to 150 fathoms in the Strait of Florida.

CLYPEASTRIDÆ.

Echinocyamus, Van Phelsum.

1. *E. angulatus*, Leske.

Generally distributed, but not found living beyond 150 fathoms.

ANANCHYTIDÆ.

Pourtalesia, A. Agassiz.

According to the classification of Desor, which makes the "disjunct" arrangement of the ambulacra at the apex the test character of the Dysasteridæ, this genus should be referred to that group; for the apical disk is truly decomposed as in *Dysaster* and *Collyrites*, and not merely drawn out as in *Ananchytes*. From the arrangement and form of the pore-plates, however, and from the general appearance and habit of the animal, I am inclined to think with A. Agassiz that its affinities are more with such forms as *Infulaster*. *Pourtalesia* must be an aberrant form, in whatever

group it may be placed. The mouth is at the bottom of a deep anterior groove, occupying the anterior ambulacral area. The arrangement of the trivium is nearly normal; but the bivial region is enormously prolonged backward into a long rostrum, on the upper surface of which, near its posterior extremity, the anus is situated in a pit partially covered by a projecting boss. The ambulacral pores are simple, one pore on each plate.

1. *P. Jeffreysi*, n. sp.

A single specimen of this very remarkable form was dredged in 640 fathoms to the north of the Shetlands. It is nearly allied to *P. miranda*, Pourtales, from the Strait of Florida, but differs in several details.

2. *P. phyale*, n. sp.

Two or three small specimens were dredged by Mr. Gwyn Jeffreys in the Rockall Channel. All the specimens are immature; but from the marked difference in form, and from some other characters, I believe them to be the young of a second species.

Spatangidæ.

Brissopsis, Agassiz.

1. *B. lyrifera*, Forbes, sp.

Large specimens of this species are abundant from 50 to 250 fathoms. Beyond the latter depth the specimens decrease in size, and at extreme depths only examples which have all the appearance of being very young are met with. These small delicate specimens were found at all depths, even down to 2090 fathoms.

Tripylus, Philippi.

1. *T. fragilis*, D. & K.

From 400 to 500 fathoms between Scotland and Færöe. Hitherto known as Scandinavian.

Schizaster, Agassiz.

1. *S. canaliferus*, Val.

A single small specimen from the coast of Spain.

Amphidetus, Agassiz.

1. *A. ovatus*, Leske, sp.

Abundant at moderate depths.

Spatangus.

1. *S. purpureus*, O. F. Müller.
2. *S. Raschi*, Lovén.

This species is apparently gregarious, and is enormously abundant in patches here and there from the Færöes to the Strait of Gibraltar at depths from 100 to 300 fathoms.

Of the twenty-seven species observed, six (namely *Echinus Flemingii*, *Sphærechinus esculentus*, *Psammechinus miliaris*, *Echinocyamus angulatus*, *Amphidetus ovatus*, and *Spatangus purpureus*) may be regarded as denizens

of moderate depths in the "Celtic province," recent observations having merely shown that they have a somewhat greater range in depth than was previously supposed. Probably *Spatangus Raschi* may simply be an essentially deep-water form having its headquarters in the same region. Eight species (*Cidaris papillata, Echinus elegans, E. norvegicus, E. rarispina, E. microstoma, Toxopneustes dröbachiensis, Brissopsis lyrifera*, and *Tripylus fragilis*) are members of a fauna of intermediate depth; and all, with the doubtful exception of *Echinus microstoma*, have been observed in comparatively shallow water off the coasts of Scandinavia. Five species (*Cidaris affinis, Echinus melo, Toxopneustes brevispinosus, Psammechinus microtuberculatus*, and *Schizaster canaliferus*) are recognized members of the Lusitanian and Mediterranean faunæ, and seven (*Porocidaris purpurata, Phormosoma placenta, Calveria hystrix, C. fenestrata, Neolampas rostellatus, Pourtalesia Jeffreysi*, and *P. phyale*) are forms which have for the first time been brought to light during the late deep-sea dredging operations, whether on this or on the other side of the Atlantic: there seems little doubt that these must be referred to the abyssal fauna, upon whose confines we are now only beginning to encroach. Three of the most remarkable generic forms, *Calveria, Neolampas*, and *Pourtalesia*, have been found by Alexander Agassiz among the results of the deep-dredging operations of Count Pourtales in the Strait of Florida, showing a wide lateral distribution; while even a deeper interest attaches to the fact that while one family type, the Echinothuridæ, has been hitherto known only in a fossil state, the entire group find nearer allies in the extinct faunas of the Chalk or of the earlier Tertiaries than in that of the present period.

XX. "On Supersaturated Saline Solutions." By Archibald Liversidge, Assoc. R.S. Mines, and Scholar of Christ's College, Cambridge. Communicated by Prof. W. H. Miller, For. Sec. R.S. Received June 13, 1872.

There is, perhaps, no necessity to describe in detail the ordinary phenomena presented by supersaturated saline solutions, since they must now be well known to all.

The following series of experiments have chiefly been made upon sodic sulphate; but before citing them, it may, however, not be out of place to briefly allude, *en passant*, to the conclusions drawn by the numerous writers and experimenters upon this subject, since the results of my own experiments are supported by the authority of some of these observers and run counter to that of others.

The theories which have been put forth are, in the main, as follows:—

a. That the crystallization of supersaturated solutions is caused by purely mechanical agencies, such as agitation &c. The principal supporter of this view was Gay-Lussac, who wrote in 1819. It has since been shown to be utterly untenable.

β. That the sudden crystallization is due to some unknown catalytic force. Advocated by Lowell in 1850, but since disproved.

γ. That it is due to the entrance of a particle of the same salt. This explanation is favoured by the majority of the writers upon the question, such as Ziz in 1809, Gernez in 1851, Violette in 1860, Dubrunfaut in 1869, by Lecoy de Boisdandran, and others.

δ. That crystallization is due to the presence of fatty, oily, greasy, or other matters in the form of thin films. This theory was propounded by Mr. Tomlinson in two papers* read before the Royal Society, in which also it is stated that certain liquids, such as absolute alcohol, act as nuclei in determining the solidification of such solutions by separating water from the solution, whereas the thin film, on the contrary, owes its activity to the greater attraction which it has for the salt held in solution.

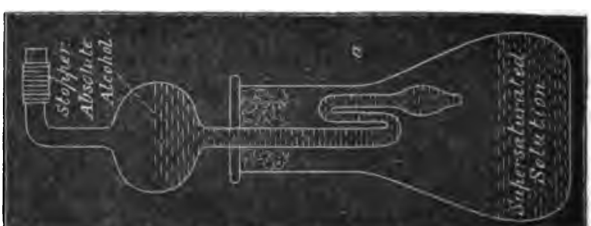
Preparation of the Supersaturated Saline Solution.

A little water is placed in the flask, boiled, and sodic sulphate added to the boiling liquid until it ceases to dissolve any more and a deposit of the anhydrous salt begins to take place; the solution is then filtered and transferred to smaller flasks, usually of about 2 oz. capacity; these are then again boiled up after being covered with a small beaker, watch-glass, or plugged with cotton-wool. By this method any nuclei adhering to the watch-glass, beaker, or wool are rendered inactive, even should they fall into the solution.

The solutions are always used of such a degree of supersaturation that crystals of the anhydrous salt are deposited during the boiling.

Do some liquids, such as alcohol, act as nuclei by combining with a portion of the water of the solution and liberating a little salt which acts as a nucleus?

Exp. Supersaturated solutions of sodic sulphate were prepared, in the manner described, in 2-oz. flasks, which were closed with a plug of cotton-



wool through which a bulb-tube was passed, of the form figured†, containing absolute alcohol.

* Phil. Trans. vol. clviii. pt. ii. and vol. clxi. pt. i.

† The loop was made in the tube at *a* so as to prevent any fluid from escaping until required.

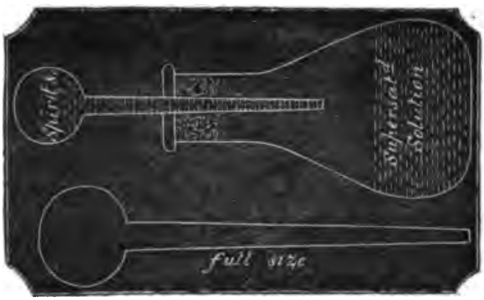
After waiting some time to be certain that nuclei had not gained admittance, some of the alcohol was run out on to the surface of the solution by momentarily loosening the stopper.

This experiment was repeated many times, at different temperatures and with alcohol of various strengths, but never did the alcohol act as a nucleus.

Previous to the experiment the alcohol had been boiled to destroy nuclei.

Exp. Concentrated sulphuric acid was substituted for the alcohol, but likewise with no result. The smallest quantity of acid was added, so as to prevent any undue rise in temperature, which would of course vitiate the result. The flask was likewise kept cold by a stream of water.

In a later form of these experiments, a small glass bulb with a long neck blown from glass tubing, such as is used in the elementary analysis of a fluid by combustion, was made use of.



The bulb was first well heated in a Bunsen burner, so as to destroy any nuclei which might adhere to it; then, while still hot, the open end was dipped into the alcohol or acid under trial, when, of course, as the air in the bulb cooled some of the liquid was forced up into it; its liquid contents were then boiled and the open end again dipped into the fluid, and as the vapour condensed more fluid was forced up into it.

The tube was then surrounded by cotton-wool and inserted into the neck of the flask, and the supersaturated solution boiled up for a moment, so as to render the whole apparatus, cotton-wool included, inactive, the steam escaping through the interstices of the cotton and not affecting the spirit. When cold a drop of the spirit or acid was delivered by merely heating the glass bulb.

Exp. Trial was next made of several solid dehydrating substances, such as calcic chloride, anhydrous chromic acid, phosphoric anhydride, freshly ignited quicklime, &c.

These bodies were placed in sealed thin glass bulbs and heated nearly to redness and then dropped into the supersaturated solution; the flasks were plugged with cotton-wool, through which a glass rod passed, and boiled up, after which they were allowed to cool for some hours; when quite cold the bulb was broken by means of the glass rod and its contents set free, but, as in the case of the liquid, with no result.

It should perhaps here be mentioned that each flask was always proved to be thoroughly supersaturated by dropping in a crystal of the salt or touching the solution with a dirty rod, after the substance made trial of was found to be wanting in nuclear power.

From the foregoing it appears that the crystallization of supersaturated saline solutions is not determined by the removal of water by chemical agency; neither do porous bodies, like wood, charcoal, sponge, spongy platinum, earthenware, &c., determine the solidification of solutions by mechanical absorption of the water.

Concerning the action of thin films.

In the same paper it is stated that while oils, fats, and greasy bodies generally do not act as nuclei when chemically clean and in the bulk, *i. e.* in the form of a solid mass, lens, or drop, yet these identical bodies when in the form of thin films do act as nuclei, and that any substance which possesses a nuclear action has derived such power from having become contaminated with a thin film of greasy matter, which it acquires by handling, wiping with a dirty cloth, or by mere exposure to the air containing the products of respiration and other excretions, &c.

Thus in the series of experiments detailed it was found that such bodies as ether, absolute alcohol, naphtha, turpentine, herring-oil, sperm-oil, castor-oil, and many others, while in the form of a lens or globule, did not act upon a supersaturated solution, but did immediately when spread out into a thin film.

It should be noticed that the oil was added to the solution by removing the cover of the flask, delivering the drop, and then replacing the cover; or a glass tube was used provided with a shield covering the mouth of the flask: both methods have the great objection of exposing the solution to the air, and so allowing nuclei to gain access.

It is stated that if the finger be cleaned by washing it in alcohol or caustic potash, or by passing it through the flame of a spirit-lamp, it may be held in a supersaturated solution for some time without causing crystallization; but that if it be rubbed against the sides of the flask, a greasy smear is produced which at once acts.

The writer has repeated this form of experiment several times, but never with the above result when sufficient care had been taken to free the finger from nuclei.

Exp. The finger was made greasy by dipping it into oil and imperfectly wiping it with a cloth; it was then passed many times through the flame of a spirit-lamp, and finally, while still far above its normal temperature, inserted into a flask of supersaturated solution: the flask was chosen with a neck such that it could be entirely closed by the thicker part of the finger. The flask was then transferred to a vessel of water, lowered artificially to 38° F., and there kept, with the finger still in it, for several minutes, varying in different experiments from 10, 15, 20, 25, 30, and 35

minutes; and although the finger was strongly pressed against the sides of the flask, which was seen to be smeared all over, yet crystallization was not set up when the solution was made to flow over the finger-marks, which were plainly visible. That the solutions were not warmed by the heat of the finger, and so rendered inactive, is proved by their immediately solidifying on the insertion of a dirty glass rod.

Exp. By means of the two modifications of bulb-tube, as already described for the experiments with absolute alcohol, thin films of various oils and other bodies were formed upon the surface of supersaturated solutions without inducing crystallization. That is, a small glass bulb was filled with the oil or other body and boiled, then supported in the neck of the flask by a plug of cotton; the supersaturated solution was then boiled and allowed to cool; when quite cold a drop of the liquid was forced out of the bulb on to the solution, then by a sudden jerk the lens or small globule thus obtained was flattened out into a thin film, often iridescent, but without causing solidification.

In numerous instances the temperature of the solution was lowered by means of ice-cold water, so as to increase its sensitiveness, but with no different result.

In many cases the oil or fatty body, such as olive-oil, Russian tallow, citronella-oil, castor-oil, &c., was dissolved in ether and then used; this device was used for two reasons: first, so that the greasy matter might be much diluted and so spread over a large surface, and then be left as a thin film on the evaporation of the ether; and second, so that a much smaller quantity of the oil might be delivered at a time. Usually the oil collected into globules shortly after the evaporation of the ether, but could generally be spread out into a film again by imparting a sharp twist to the flask.

Supersaturated solutions of sodic sulphate having films of oil, benzol, turpentine, citronella-oil, &c. upon their surface have been kept by the writer for several months together, and some even as long as eighteen months; it is true that the oil &c. soon lost the form of an iridescent film, but could be made to assume it at any moment; and the above lot of flasks were seldom allowed to stand for a day without being made to do so, *i.e.* for the first three months after their preparation and at greater intervals afterwards. Every now and then a flask was caused to crystallize in order to ascertain that the solutions had in no way lost their sensitiveness to a dirty rod; and when the last flask of all was proved, it had stood for rather more than eighteen months.

One explanation accounting for the activity of the thin film as prepared by the eminent author of the paper referred to may be this:—That in order to place the oil upon the solution, the flask was opened and exposed to the air, thus affording an opportunity for nuclei to gain entrance; and also they may have been carried in by the greasy rod itself, for there would be plenty of time in its passage for it to pick nuclei up: such nuclear bodies would

probably float upon the surface of the disk or globules of oil, and would not come into contact with the solution itself; neither might they touch its surface even when the disk was broken up into small globules, for these globules would be immensely large in comparison with the dimensions of the nucleus itself; but, on the other hand, when the disk was flattened out into an iridescent film, and therefore one of excessive tenuity, the nuclei might then easily fall through it, come in contact with the supersaturated solution, and start its crystallization; as it is probable that several nuclei would enter at the same time, they would naturally become dispersed by the jerk, and hence crystallization would be set up at various points.

That nuclei will pass through the substance of a thin film is shown by the solidification which almost immediately takes place on exposing to air the solution covered merely by a film of oil, turpentine, &c.; a thick coating of oil is, of course, one of the best means we have of protecting a supersaturated solution from nuclei.

The principal substances made use of by the writer for the formation of thin films were as follows:—Citronella-oil, olive-oil, Russian tallow, castor-oil, camphor in alcohol, creosote, turpentine, benzol, chloroform, ether, &c.

Concerning the action of a crystal of the normal sodic sulphate upon a supersaturated solution of the same.

It is well known that there are three modifications of sodic sulphate crystals:—

1. *The anhydrous salt* (Na_2SO_4), crystallizing in octahedra, and deposited from a supersaturated solution on further concentration; these crystals are inactive to a supersaturated solution.

2. *The modified salt* ($Na_2SO_4, 7H_2O$), containing $7H_2O$, formed in a supersaturated solution by reduction of temperature and other causes; these also are inactive, and admitted so by all.

3. *The normal salt* (Na_2SO_4, $10H_2O$), crystallizing in prisms with dihedral summits, and containing $10H_2O$. Usually regarded as the best nucleus. Experiments relating to its behaviour as such will be detailed.

It is always the normal salt (Na_2SO_4, $10H_2O$) which is formed when a solution is caused to crystallize by touching it with a dirty rod or by exposing it to the air, &c.

Experiments were made with recently generated crystals of the normal salt.

Exp. Two beakers, containing fully supersaturated solutions, were covered with watch-glasses, and allowed to cool; in one of the beakers a small glass bucket, attached to a thread, had been placed and boiled up with the solution. Next, both beakers were arranged under a large bell-jar, and the silk thread from the bucket passed up between the stopper and the neck of the jar. The solutions were then uncovered, after waiting ten minutes for any nuclei which might have been disturbed to fall; a fine wire was passed down into the beaker containing the bucket, and as far as possible

from the part of the solution through which it would pass on being drawn up.

The bucket, now full of the crystallized normal sodic sulphate (Na_2SO_4, $10H_2O$), was raised, and lowered into the second beaker of still fluid solution; immediately that the point of one of the crystals hanging from the under surface of the bucket touched the solution, crystallization was set up instantaneously throughout the mass.

This experiment was performed many times, and with every possible care to prevent the entrance of nuclei other than those purposely borne by the wire.

A modification of the above plan was tried and with similar results.

Exp. A tubulated glass bell was fitted with a cork bearing two glass tubes, open below and closed above with cotton-wool; they were bent so as to permit both of them being placed in one and the same beaker, or into either separately.

In the first place, the ends of the tubes inside the bell were freed from nuclei by passing them through a flame; two beakers of cold supersaturated solution were then placed in position under the bell-jar, and their covers removed. After waiting five minutes or so for any dust to settle, both tubes were next lowered into one of the beakers, on opposite sides, so as to be as far apart as possible. A dirty wire was now passed down one of the tubes, when, of course, crystallization immediately took place, and was propagated across the beaker. The second tube, with its adhering crystals, was then raised and lowered into the second beaker, when, the moment the extreme point of the longest crystal touched the surface of the solution, crystallization immediately started from that point, and the whole contents became solid.

A third variation was then made in this experiment. One of the two beakers was replaced by a U-tube of thin, hard glass, one of the beforementioned tubes being inserted into either limb. Crystallization, when set up in one limb, travelled round the bend and up into the other, from which crystals were transferred, as before, to a beaker or flask of solution also under the bell-jar.

The three modifications of this form of experiment were tried time after time, and always with the same unvarying result. Solutions which were supersaturated although not perfectly, and therefore less sensitive, were operated upon in this way; but, even with such less favourable circumstances, the normal crystals always started crystallization in the solution to which they were added.

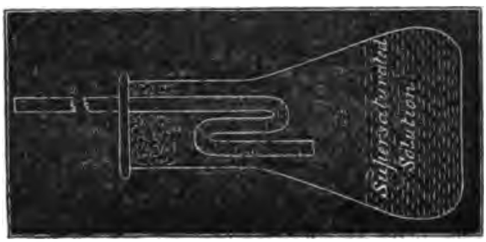
To ascertain, if possible, whether nuclei, other than crystals of the normal salt, were carried by the tube or its adhering crystals, a capsule of sulphuric acid was placed under the bell. The crust of crystals was by this means dried, and became effloresced to a greater or less extent. Now, on lowering them into a supersaturated solution of alum or of magnesic sul-

phate, they were proved to be inactive, having been changed to the inactive anhydrous salt.

But such dried normal crystals were active to a solution of sodic sulphate, even after three days' exposure to the sulphuric acid *. It seems as if the normal crystals become covered with a coating of effloresced anhydrous salt which acts as a protection to the underneath portions, in the same way as oxide of lead does to metallic lead; hence it takes a long time to convert a crystal of the normal salt into the anhydrous by simple exposure to dry air, although it is an exceedingly short operation to perform at temperatures superior to 34° C.

Yet another form of this experiment was tried again and again, and always with the same result.

A glass tube bent into the form of an elongated letter S was suspended by a plug of cotton-wool in the neck of a flask containing a supersaturated solution; the solution was boiled, and the tube was also boiled in it, so as to get all nuclear particles adhering to it thoroughly destroyed.



The solution was then allowed to cool, with the tube still in it; the tube was then raised out of the solution and a dirty wire passed down it; crystallization was, of course, set up in the portion of supersaturated solution contained within the tube; the crystals gradually grew down the tube, then through the first bend, travelled up the upright portion, then travelled round the second bend, and finally down the third and last straight portion. Now, on lowering the extreme tip of the crystals formed at the end of the tube into the solution, crystallization was immediately set up from it as a centre, and thence throughout the mass.

By this arrangement access of extraneous nuclei was entirely prevented. The upper end of the tube was plugged with cotton-wool until the dirty wire was passed down.

That the normal crystals thus formed did not act by any transient molecular movements, which recently formed crystals might be supposed to have, is proved doubtless by the fact that such crystals were found to act just as readily even when they had been kept over the solution for 2½, 5, 10, 24, and 48 hours, and then lowered into the solution, and when any molecular agitation may with fairness be supposed to have ceased.

* At a future day I hope to have the results of more experiments upon this point.

Exp. Supersaturated solutions of common potash alum were treated in the same way and with the like results; alum, perhaps, affords a prettier example even than sodic sulphate, since the crystals formed in the tube are of an opaque white, and can therefore be more readily observed during their growth.

Exp. Supersaturated solutions of magnesic sulphate were also operated upon and with the same success; but the experiment is not so striking, owing to the much longer time required by magnesic sulphate to crystallize.

Although pure clean crystals of the normal sodic sulphate are active to a supersaturated solution of sodic sulphate, yet, as might be expected, they are not active to a similar solution of alum or magnesic sulphate, and *vice versâ.*

For example, let us take a supersaturated solution of alum, and one of sodic sulphate, and also crystals of both their salts, which crystals have just formed and are taken from their still warm mother-liquors.

Exp. A crystal of alum from its mother-liquor was added to a supersaturated solution of alum. Crystallization immediately took place.

Exp. A like crystal of alum was then added to a supersaturated solution of sodic sulphate. No effect.

Exp. A crystal of the normal salt was taken from its mother-liquor and added to a solution of sodic sulphate. The solution instantly crystallized, although another crystal was inactive to a solution of alum.

Exp. A crystal of magnesic sulphate was added to solutions of alum and of sodic sulphate respectively. No effect on either, but active in a solution of magnesic sulphate.

Concerning the composition of the crystals of sodic sulphate formed by spontaneous evaporation of a supersaturated solution of the same.

When a supersaturated solution of sodic sulphate is allowed to evaporate spontaneously, a crust or ring of crystals forms on the surface of the solution, or a ring in the upper part of the vessel; these crystals are perfectly inactive, as has long been known; and this has been accounted for by regarding them as crystals of the modified salt ($Na_2SO_4, 7H_2O$), which is non-nuclear: but recently they have been regarded as crystals of the normal salt ($Na_2SO_4, 10H_2O$), and their want of action upon the supersaturated solution has been explained by saying that, unlike crystals which have been exposed to the air, they are chemically clean, and therefore free from any film of greasy or other matter; for this writer views the activity shown by the normal salt as being entirely due to impurity of this kind, and not as due to any property inherent in it.

Lōwel made analyses of this salt, formed by spontaneous evaporation, and found it to consist of the modified salt containing $Na_2SO_4, 7H_2O$.

Faraday also examined it and came to much the same conclusion, only that he gave it $8H_2O$, instead of $7H_2O$. There is no doubt that Faraday

obtained this salt and not the normal with $10H_2O$, although he made what has since been proved to be a mistake in assigning $8H_2O$ to the modified salt.

The writer allowed some supersaturated solutions of sodic sulphate to evaporate spontaneously, and after several vain attempts at last succeeded in obtaining good crops of such crystals, without admixture of the normal salt, which, of course, is liable to crystallize out also on opening the receiver. The ring of crystals at the top of the solution only were taken.

Results of determinations of water of crystallization in crystals of sodic sulphate formed by spontaneous evaporation.

No. 1. ·365 grm. of salt, on drying in water-oven at 100° C., after first well drying the powdered salt with blotting-paper, lost ·170 grm.=46·57 per cent. OH_2, $=Na_2SO_4 7OH_2$.

No. 2. ·172 grm. lost ·081 grm.=47·09 per cent., $=Na_2SO_4, 7OH_2$.
No. 3. 2·708 grms. lost 1·273 grm.=47·00 per cent., $=Na_2SO_4, 7OH_2$.
No. 4. 1·260 grm. lost ·605 grm.=47·00 per cent., $=Na_2SO_4, 7OH_2$.
No. 5. 3·936 grms. lost 1·812 grm.=46·69 per cent., $=Na_2SO_4, 7OH_2$.
No. 6. 3·275 grms. lost 1·520 grm.=46·41 per cent., $=Na_2SO_4, 7OH_2$.
No. 7. 3·326 grms. lost 1·570 grm.=47·11 per cent., $=Na_2SO_4, 7OH_2$.

No.	Weight. grms.	Loss. grm.		OH_2. per cent.
1.	·365	·170	=	46·57
2.	·172	·081	=	47·09
3.	2·708	1·273	=	47·00
4.	1·260	·605	=	47·00
5.	3·936	1·812	=	46·69
6.	3·275	1·520	=	46·41
7.	3·326	1·570	=	47·11

I trust that by the above-mentioned results I have clearly proved the following facts with respect to supersaturated solutions of sodic sulphate:—

1. That liquids and solids, such as alcohol, quicklime, &c., do not determine crystallization by removing water.

2. That thin films, when sufficient precautions are taken to guard against the entrance of nuclei, do not act as nuclei.

3. That chemically clean crystals of the normal salt (Na_2SO_4, $10H_2O$) *do act* as nuclei and are most powerful.

4. That crystals of the normal salt are not produced in supersaturated solutions of sodic sulphate on allowing it to evaporate spontaneously, but that crystals of the modified (and known inactive) salt are.

In conclusion I may perhaps be permitted to state that the above series of experiments have extended over a period of three years, less a few months, and that most of them have been repeated a countless number of

times, and with every conceivable modification and check. Some few of them have already been published in the 'Chemical News,' but are here referred to again for the sake of comprehensiveness.

At present the writer does not venture to put forth any definite theory respecting the presence and nature of the nuclei which are so universally diffused throughout the atmosphere; but when it is considered how much sodic chloride is constantly present in the air, and what quantities of sulphurous acid are evolved daily, which becomes partly converted into sulphuric acid, the presence of particles of sodic sulphate in the air would not be surprising; and that it does exist is proved by drawing air through water and finding comparatively large quantities in the solid matter arrested by water.

Sodic sulphate solutions, too, crystallize on exposure much more readily than those of any other salt. The other salts which form supersaturated solutions are certainly less diffused than sodic sulphate.

XXI. "Note relating to the Attraction of Spheroids." By I. Todhunter, M.A., F.R.S. Received May 16, 1872.

In a memoir on the Attraction of Spheroids, published in the 'Connaissance des Tems' for 1829, Poisson showed that certain important formulæ were true up to the *third* order inclusive of the standard small quantity. The object of this note is to establish the truth of the formulæ for *all* orders of the small quantity.

1. Suppose we require the value of the potential of a homogeneous body at any assigned point. Take a fixed origin inside the body; let r', θ', ψ' denote the polar coordinates of any point of the body; and let r, θ, ψ be the polar coordinates of the assigned point; and, as usual, put μ' for $\cos\theta'$, and μ for $\cos\theta$. The density may be denoted by unity.

Then the potential V is given by the equation

$$V=\int\int\int\frac{r'^2\,dr'\,d\mu'\,d\psi'}{\sqrt{(r^2+r'^2-2rr'\lambda)}},$$

where

$$\lambda=\mu\mu'+\sqrt{(1-\mu^2)}\sqrt{(1-\mu'^2)}\cos(\psi'-\psi).$$

The integration must extend over the whole body.

2. Suppose that r is greater than the greatest value of r'; then $(r^2+r'^2-2rr'\lambda)^{-\frac{1}{2}}$ can be expanded in a convergent series of powers of $\frac{r'}{r}$. Thus, for example, let the body be an ellipsoid, and take the centre as the origin; let a, b, c denote the semiaxes in descending order of magnitude. Then, if r is greater than a, the expansion may be effected in the manner just stated; and so a convenient expression may be obtained for the potential of an ellipsoid on an external particle. This expression, however, is not demonstrated to hold for every external particle, but only for

those which make r greater than a. It is obvious that there may be external particles for which r is less than a; and for these the process cannot be considered satisfactory, since it involves the use of a divergent series.

3. Still it has been usual with writers on the Attraction of Spheroids and the Figure of the Earth to leave this point unexamined. They, in fact, assume that formulæ which are demonstrated on a certain condition are true, even when that condition does not hold; so that, for example, an expression obtained strictly for the potential of an ellipsoid on an external particle when r is greater than a, is assumed to be true for any external particle.

4. Poisson, however, has drawn attention to the difficulty; his discussion of it is the main part of his elaborate memoir "Sur l'Attraction des Sphéroides," which was published in the 'Connaissance des Tems' for 1829. He shows that the ordinary formulæ, although obtained in an inadequate manner, are really true *as far as the terms of the order* α^3 *inclusive,* where α is the well-known standard small quantity of such investigations. I propose to extend his process so as to show that the result is true for all powers of α.

It will be necessary to give some preliminary transformations; this I shall do with brevity, referring to Poisson's memoir for detail.

5. It is convenient to separate V into two parts, one being the potential of a sphere of radius r, and the other the potential of the excess of the spheroid above the sphere; the word *excess* is here used in an algebraical sense, for the surface of the spheroid is not necessarily all external to that of the sphere. Thus we obtain

$$V=\frac{4\pi r^2}{3}+\int\int\int_r^u \frac{r'^2\,d\mu'\,d\psi'\,dr'}{\sqrt{(r^2+r'^2-2\lambda r r')}}, \quad . \quad . \quad . \quad (1)$$

where u denotes the radius vector of the surface of the spheroid corresponding to the angles θ' and ψ'; so that the integration with respect to r' is to be taken between the limits r and u. The integration for μ' and ψ' may be considered to be taken over the surface of a sphere of radius unity; and we may denote an element of this surface by $d\omega'$, and use the symbol $\int d\omega'$ instead of $\int\int d\mu'\,d\psi'$.

6. Now, for those elements in the integral in (1) which have r' less than r, the radical must be expanded in powers of $\frac{r'}{r}$; and for those elements which have r' greater than r, the radical must be expanded in powers of $\frac{r}{r'}$. Thus we obtain

$$V=\frac{4\pi r^2}{3}+\Sigma\frac{1}{r^{n+1}}\int\left(\int_r^u r'^{n+2}dr'\right)P'_n d\omega'+\Sigma\, r^n\int\left(\int_r^u \frac{dr'}{r'^{n-1}}\right)P'_n d\omega', \quad (2)$$

where P'_n denotes Laplace's coefficient of the nth order. In the second

term on the right-hand side of (2) the integration with respect to ω' is to extend over so much of the surface of a sphere of radius unity as corresponds to *negative* values of $u-r$; and in the third term the integration with respect to ω' is to extend over so much of the surface of the sphere as corresponds to *positive* values of $u-r$. By Σ is denoted a summation with respect to the integer n for all values from zero to infinity.

7. By adding a certain quantity to the second term on the right-hand side of (2), and subtracting the same quantity from the third term, we obtain, finally,

$$V=\frac{4\pi r^2}{3}+\Sigma\frac{1}{r^{n+1}}\int_0^{4\pi}\left(\int_r^u r'^{n+2}dr'\right)P'_n\,d\omega'-\Sigma\int U\,P'_n\,d\omega', \quad . \quad . \quad (3)$$

where U stands for. . .

$$\frac{1}{r^{n+1}}\int_r^u r'^{n+2}dr'-r^n\int_r^u\frac{dr'}{r'^{n-1}}.$$

In the second term on the right-hand side of (3) the integration for ω' extends over the *whole surface* of the sphere of radius unity; and this I denote by explicitly putting the limits 0 and 4π. But in the third term the integration for ω' extends only over that portion of the surface which corresponds to positive values of $u-r$; and this I denote by leaving the limits unspecified.

8. For the rest of this paper the notation just explained will be strictly preserved. If the integration with respect to ω' extends over the whole surface of the sphere, the limits 0 and 4π will be expressed; if the integration extends only over that portion of the surface which corresponds to positive values of $u-r$, the limits will not be expressed.

9. The value of V obtained in (3) is quite general, but it is specially convenient for the case of an external particle. Poisson gives also another form which is specially convenient for the case of an internal particle.

It will be sufficient for us to confine ourselves to the case of an external particle, as the same process is readily applicable to the case of an internal particle.

10. For an external particle which is sufficiently remote, the third term on the right-hand side of (3) vanishes, because in this case $u-r$ is never positive; so that we have then simply

$$V=\frac{4\pi r^2}{3}+\Sigma\frac{1}{r^{n+1}}\int_0^{4\pi}\left(\int_r^u r'^{n+2}\,dr'\right)P'_n\,d\omega'.$$

Now what we have to show is that this formula will also hold for every external particle. In other words, it must be shown that for *any* external particle

$$\Sigma\int U\,P'_n\,d\omega'=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

11. Put z' for $u-r$. Then

$$U=\frac{1}{(n+3)r^{n+1}}\{(r+z')^{n+3}-r^{n+3}\}+\frac{r^n}{n-2}\{(r+z')^{-n+2}-r^{-n+2}\}$$

$$=\frac{n+2+n-1}{2}z'^2+\frac{(n+2)(n+1)-(n-1)n}{\lfloor 3}z'^3$$

$$+\frac{(n+2)(n+1)n+(n-1)n(n+1)}{\lfloor 4}z'^4$$

$$+\frac{(n+2)(n+1)n(n-1)-(n-1)n(n+1)(n+2)}{\lfloor 5}z'^5$$

$$+\ldots\ldots$$

$$=\frac{2n+1}{2}z'^2+\frac{2n+1}{3}z'^3+\frac{(2n+1)(n^2+n)}{\lfloor 4}z'^4+\ldots\ldots$$

12. Let ζ' be a discontinuous function of μ' and ψ', such that ζ' is always equal to z' when z' is positive, and always zero when z' is negative. Then, for all values of m, we have

$$\int z'^m P'_n d\omega'=\int_0^{4\pi}\zeta'^m P'_n d\omega'.$$

This is a very important step in Poisson's process; and he explains it with adequate care. We may suppose that ζ' is expressed by means of a series of Laplace's functions.

13. As we may also suppose ζ'^2 expanded in a series of Laplace's functions, it will follow, from the well-known properties of such functions, that

$$\Sigma(2n+1)\int_0^{4\pi}\zeta'^2 P'_n d\omega'=4\pi\zeta^2, \quad \ldots\ldots \quad (5)$$

where ζ is the value of ζ' when $\theta'=\theta$ and $\psi'=\psi$. But, by supposition, ζ is zero. Hence

$$\Sigma(2n+1)\int_0^{4\pi}\zeta'^2 P'_n d\omega'=0,$$

and therefore

$$\Sigma(2n+1)\int z'^2 P'_n d\omega'=0.$$

In precisely the same manner we have

$$\Sigma(2n+1)\int z'^3 P'_n d\omega'=0.$$

14. Thus far Poisson carries his process. His words are, on his page 368:—"Pour simplifier la question, on néglige ici les puissances de ζ' supérieures à la troisième, ou autrement dit, on borne l'approximation aux quantités de l'ordre α^3 inclusivement."

I am not certain whether Poisson himself had carried his investigation beyond this point. In the later part of his memoir he certainly implies

that results which partly depend on the present investigations are true for all powers of $u-r$. It seems, therefore, curious that he did not here explain how the terms which involve powers of z' above the third vanish, so as to make (4) absolutely true. To this we now proceed.

15. In Art. 11 we see that the coefficient of z'^4 is

$$\frac{(2n+1)(n^2+n)}{\underline{|4}}.$$

Hence we have to show that

$$\Sigma(2n+1)(n^2+n)\int_0^{4\pi}\zeta'^4 P'_n\,d\omega'=0.$$

Now, by the nature of Laplace's coefficients, we have

$$(n^2+n)P'_n=-\frac{d}{d\mu'}\left\{(1-\mu'^2)\frac{dP'}{d\mu'}\right\}-\frac{1}{1-\mu'^2}\frac{d^2P'_n}{d\psi'^2}. \quad . \quad . \quad (6)$$

Hence, by two integrations by parts, we find that

$$\Sigma(2n+1)\;(n^2+n)\int_0^{4\pi}\zeta'^4 P'_n\,d\omega'=-\Sigma(2n+1)\int_0^{4\pi}P'_n\nabla\zeta'^4 d\omega',$$

where, for abbreviation, ∇ is used to denote the operation which, as performed on P'_n, is expressed on the right-hand side of (6).

Then, in the same way as (5) is obtained, we have

$$\Sigma(2n+1)\int_0^{4\pi}P'_n\nabla\zeta'^4\,d\omega'=4\pi\nabla\zeta^4\,;$$

and $\nabla\zeta^4$ is zero, for every term involves ζ^2 as a factor.

Hence, finally,

$$\Sigma(2n+1)\,(n^2+n)\int_0^{4\pi}\zeta'^4 P'_n d\omega'=0.$$

16. In Art. 11 it will be found that the coefficient of z'^5 is zero.

The coefficient of z'^6 is

$$\frac{(n+2)(n+1)n(n-1)\{n-2+n+3\}}{\underline{|6}},$$

that is,

$$\frac{(n+2)(n-1)(n^2+n)(2n+1)}{\underline{|6}},$$

that is,

$$\frac{(n^2+n-2)(n^2+n)(2n+1)}{\underline{|6}},$$

that is,

$$\frac{(n^2+n)^2-2(n^2+n)}{\underline{|6}}(2n+1).$$

Hence we have to show that

$$\Sigma(n^2+n)^2(2n+1)\int_0^{4\pi}\zeta'^6\,P'_n\,d\omega'-2\Sigma(n^2+n)(2n+1)\int_0^{4\pi}\zeta'^6\,P'_n d\omega'=0.$$

The second term we see vanishes by the process of Art. 15. As to the first term, we must apply that process twice; and we shall then transform this term into

$$\Sigma(2n+1)\int_0^{4\pi}P'_n\nabla(\nabla\zeta'^6)\,d\omega';$$

and, as before, this is equal to $4\pi\nabla(\nabla\zeta^6)$, which vanishes, because every term will have ζ^2 as a factor.

17. In Art. 11 it will be found that the coefficient of z'^7 is

$$-\frac{6}{\lfloor 7}\{(n^2+n)^2-2(n^2+n)\}(2n+1),$$

and hence this term may be treated as the term was in the preceding Article.

18. Generally the coefficient of z'^r in U will be found to be

$$\frac{(n+2)(n+1)\ldots(n-r+4)}{\lfloor r}+(-1)^r\frac{(n-1)n\ldots(n+r-3)}{\lfloor r};$$

and hence, in order to carry on the process like that in Art. 16, we must show that this coefficient will take the form

$$\frac{2n+1}{\lfloor r}\,\mathrm{N},$$

where N is some rational integral function of n^2+n.

This may be etsablished inductively.

Assume that the required theorem holds for a certain value of r, and also for the value $r+1$; then it will hold for the value $r+2$.

For let it be assumed that

$$(n+2)(n+1)\ldots(n-r+4)+(-1)^r(n-1)n\ldots(n+r-3)=(2n+1)\mathrm{N}_1,$$

and also that

$$(n+2)(n+1)\ldots(n-r+3)-(-1)^r(n-1)n\ldots(n+r-2)=(2n+1)\mathrm{N}_2,$$

where N_1 and N_2 are rational integral functions of n^2+n; then we require to show that

$$(n+2)(n+1)\ldots(n-r+2)+(-1)^r(n-1)n\ldots(n+r-1)$$

will take a similar form.

We may denote our two assumed results thus:—

$$\mathrm{P}+(-1)^r\mathrm{Q}=(2n+1)\mathrm{N}_1,$$
$$\mathrm{P}(n-r+3)-(-1)^r\mathrm{Q}(n+r-2)=(2n+1)\mathrm{N}_2;$$

and then we have to investigate the form of

$$P(n-r+3)(n-r+2)+(-1)^r Q(n+r-2)(n+r-1).$$

Now the two following identities may be verified:—

$$(n-r+3)(n-r+2)=n^2+n-(r-3)(r-2)-2(n-r+3)(r-2),$$
$$(n+r-2)(n+r-1)=n^2+n-(r-3)(r-2)+2(n+r-2)(r-2).$$

Hence

$$\begin{aligned} P(n-r+3)(n-r+2)+(-1)^r Q(n+r-2)(n+r-1) & \\ &= \{n^2+n-(r-3)(r-2)\}(2n+1)N_1-2(r-2)(2n+1)N_2 \\ &= (2n+1)N, \end{aligned}$$

where N is a rational integral function of n^2+n. Hence, as we have seen by actual inspection that for integral values of r up to 7 inclusive the required form is obtained, it follows that this form will be obtained for all positive integral values of r.

19. We may collect our results into two propositions, one of elementary algebra and one of the theory of Laplace's functions.

Let $f(z')$ stand for

$$\frac{1}{(n+3)r^{n+1}}\{(r+z')^{n+3}-r^{n+3}\}+\frac{r^n}{n-2}\{(r+z')^{-n+2}-r^{-n+2}\},$$

and suppose r greater than z', so as to ensure convergent series when the binomials are expanded in powers of z'; then the coefficient of every power of z' is the product of $2n+1$ into some rational integral function of n^2+n.

Let ζ' be a Laplace's function of the usual variables μ' and ψ', and ζ the same function of μ and ψ; and suppose r greater than ζ'; then

$$\Sigma\int_0^{4\pi} f(\zeta')\,P'_n\,d\omega'$$

is a function of ζ and its differential coefficients, which involves ζ^2 as a factor, and so vanishes when ζ vanishes.

20. I have not proposed to examine any difficulties which a reader may find in Poisson's process, but only to show that it can be made to furnish a general result instead of the result merely to the third order. Poisson's memoir has been much used by Bowditch in his translation of the 'Mécanique Céleste,' with a commentary (see vol. ii. p. 185); but Bowditch confines himself to the same order of approximation in the theorem as Poisson.

May 3, 1872.

XXII. "Report on the Exploration of Brixham Cave, conducted by a Committee of the Geological Society, and under the immediate superintendence and record of WM. PENGELLY, Esq., F.R.S., aided by a local Committee; with descriptions of the Organic Remains by G. BUSK, Esq., F.R.S., and of the Flint Implements by JOHN EVANS, Esq., F.R.S." By J. PRESTWICH, F.R.S., F.G.S., &c., Reporter. Received May 16, 1872.

(Abstract.)

This Cave, or rather series of enlarged fissures in the Devonian Limestone, was discovered in January 1858 whilst quarrying the rock on the slope of the hill which rises above the small fishing town of Brixham, near Torquay. The owner of the quarry had the excavation carried sufficiently far to show that the cave had several branches, and contained bones both on the surface of the stalagmite and in the red loam beneath it. Mr. Pengelly visited the cave soon after its discovery, and, believing it likely to prove of much interest, opened negotiations with the proprietor, with a view to secure the right of exploration. There were, however, obstacles which then prevented this object being carried into execution. Shortly afterwards the late Dr. Falconer, while on a visit to Torquay, was informed of the discovery, and, after a careful inspection of the cave, he was so impressed with the opportunity here afforded of working out completely a new and untouched bone-cavern, that on his return to London he addressed a letter to the Council of the Geological Society, in which, after referring to the early researches on ossiferous caves by Dr. Buckland, and the little progress the subject had made since that period, notwithstanding its great interest on so many grounds, he urged the importance of a thorough exploration of such a cave, and suggested that this was a case "deserving of a combined effort among geologists to organize operations for having it satisfactorily explored before mischief was done by untutored zeal and desultory work."

Dr. Falconer further stated that, "from what he had already seen of the cave, he was strongly of the conviction that, with our present advanced knowledge, the thorough investigation of a well-filled virgin cave in England would materially aid in clearing up the mystery, either of the contemporaneity of the Pliocene Mammalian Fauna with the commencement of the Postpliocene Fauna, or of the conditions and associations under which the former was replaced by the latter."

The Council of the Geological Society, not having at their disposal funds for undertaking such a work, addressed a letter to the President and Council of the Royal Society, by whom a grant of £100 was promptly made from the Donation Fund, on the understanding that any specimens obtained should be eventually deposited in the British Museum. This sum was afterwards increased by the liberal donation of £50 from Lady

Burdett Coutts, of £5 from Sir James Kay Shuttleworth, of £5 from R. Arthlington, Esq., and by a further grant of £100 from the Donation Fund of the Royal Society.

A Committee of the Geological Society was thereupon appointed to direct, and a local committee named to carry out the work of exploration. It is, however, to Mr. Pengelly that the Committee are indebted for the constant, close superintendence of the work, and for the record of each day's proceedings,—assistance without which it would not have been possible to have carried through this investigation.

The work was commenced in July 1858, and was sufficiently advanced by the following September to enable Dr. Falconer, Professor Ramsay, and Mr. Pengelly to report highly satisfactory progress, and to state that "one result of great interest had been brought out, namely, the superposition of undoubted remains of the Reindeer above the so-called 'Flint Knives,' from which the inference arose that the 'Reindeer' continued to be an inhabitant of Britain after the appearance of man in this island.'"

In November 1858, by permission of the late Sir R. I. Murchison, a plan and sections of the cave were made by Mr. Bristow, of the Geological Survey, and these were completed by Mr. Bovey, of Brixham, at the conclusion of the exploration.

Brixham stands at the entrance of a narrow valley which runs about three miles inland. The hills on either side consist of Devonian Limestone dipping northward, and rise to the height of from 170 to 190 feet, while higher up the valley traverses Devonian slates and grits. On the slope of Windmill Hill, on the south side of the valley, and at a height of 94 feet above high-water mark, is the entrance to the cave, the galleries of which follow the direction of the two sets of joints traversing these strata—the one running nearly magnetic north and south, and the other being at nearly right angles to it. The several galleries of the cave were found to extend 135 feet from north to south, and 100 feet from east to west; and although the passages became so contracted at the end that further progress was stayed, it is a question whether the ramifications of the cavern do not extend deeper into the hill.

The work of exploration was completed in the summer of 1859, and it was hoped, but for his untimely death, that the late Dr. Falconer would have furnished the Society both with an account of the organic remains and with the general report. As it is, Mr. Busk kindly undertook the former, and I, as Treasurer of the Committee, was deputed to furnish the latter, while Mr. Evans examined and reported on the flint implements found in the cave—the whole being based on Mr. Pengelly's observations and collections.

The object of this investigation is necessarily to put on record, in a form available for future examination, information of that special and exact character which, from the costly nature of the work and the variety of subjects connected with it, places it generally beyond individual research.

In dealing with the theoretical questions connected with the subject, they will be restricted to those alone which are suggested by the local nature of the phenomena.

The main gallery of the cavern was that which extended southwards from the entrance for a distance of 135 feet, and was termed the Reindeer Gallery. From the left of this, and at a short distance from the entrance, is a small inclined passage 40 feet long, and called the "Steep Slide Hole." From the Reindeer Gallery further on branches on the right the Flint-knife Gallery, which runs westward, and terminates in the West Chamber, from which other short galleries, known as the Pen, Keeping's, and Mundy's galleries, proceed, and connected with them is the South Chamber. These chambers were found to have three other external openings, which, like the original entrance from the road, had all become blocked up by fallen débris.

When first opened, all the galleries and chambers were found to be more or less filled with the following deposits, in descending order:—

1st. A layer of stalagmite, varying from a few inches to upwards of a foot in thickness.

2nd. Reddish cave-earth, with angular fragments and blocks of limestone in places, generally averaging from 2 to 4 feet.

3rd. Water-worn shingle, 2 to 6 feet.

In addition to these, a thin layer of peaty or carbonaceous matter extended on the cave-earth from near the entrance to a distance of 40 feet, and was overlain part of the distance by a limestone breccia.

The stalagmite was not of constant occurrence; the Flint-knife Gallery was almost free of it.

In places the galleries were completely choked up by the cave-earth rising to the ceiling, as in the West Chamber and part of the Flint-knife Gallery. A few pebbles, the same as those composing the underlying shingle bed, were occasionally found in the cave-earth, together with fragments of stalagmite,—portions, apparently, of an old destroyed stalagmite floor. These latter were extremely numerous in the West Chamber and adjacent part of the Flint-knife Gallery, and also in the Steep Slide Hole, where no stalagmite floor existed.

The basement, or shingle bed, consisted of pebbles of limestone, quartz, greenstone, grit, and shale, all derived from rocks of the Brixham district. In one part of the Flint-knife Gallery the gravel rested on a continuous limestone floor, but elsewhere its base was not reached, the fissures contracting to a wedge too narrow to allow of working them out to the bottom.

The section of the Reindeer Gallery may be likened to the irregular figure of a skate, with double pectoral fins—the shingle bed filling the space represented by the tail, or a little more, and the cave-earth extending usually about halfway up the body. The pectoral fins represent the position of two sets of longitudinal grooves, which Mr. Pengelly states to extend the whole length of the cave, with a general slight dip in a given

direction from the West Chamber, and also from the main entrance to the Steep Slide Hole. Below this the limestone walls curve inwards, "but instead of meeting to form a continuous floor, they remain separated by a central longitudinal fissure, which varies from 3 to 18 inches in width, tapering till the walls meet," and forming, as it were, the tail of the skate. The dimensions and exact shape of all these parts vary considerably; 6 to 8 feet in width by 8 to 10 in height may, however, be taken as the average dimensions of the galleries above the basal fissure.

In addition to the existing stalagmite floor, remains of an older and higher floor were found throughout part of the Reindeer Gallery, extending in places horizontally from wall to wall, and varying from 6 inches to a foot in thickness. Above the considerable openings which occurred in it, there was an open space of from 1 to 5 feet high. In places fragments of limestone, of quartz, and of older stalagmite, both angular and rounded, were attached to the underside of this stalagmite ceiling.

In that part of the Reindeer Gallery called the Crystal Gorge, there was a series of six or seven thin stained layers of stalagmite extending from wall to wall, and separated by layers of the cave-earth.

A few stalactites depended from the roof of the Reindeer and Pen Galleries and the South Chamber.

Mr. Pengelly found that the "dips of both the second and third beds corresponded, in amount and direction, to that of the grooves in the same branches of the cavern. But between the eastern end of the Flint-knife Gallery and the northern end of the Crystal Gorge, the inclination of these beds was southwards, that is from the former to the latter point." Mr. Pengelly also describes some variations in the deposits of the less important part of the cavern, which do not, however, affect the general persistence of the characters and structure above named.

Organic Remains.—No shells were found in any of the beds; but a considerable number of existing land-shells, and one limpet-shell, were found on the surface, and a few in the stalagmite. They were most numerous near the external entrances.

Mammalian remains were found sparingly in the stalagmite, in abundance in the cave-earth, and rarely in the shingle. Mr. Pengelly gives a complete list of every find, and tabulates the result in a series of valuable tables, from which it appears that in the stalagmite 25 bones were found, in the cave-earth and on its surface 1589, and in the shingle 7, making a total of 1621 bones. Of these, 835 occurred in the Reindeer Gallery, 515 in the Flint-knife Gallery, 244 in the West Chamber, and only 27 in the other parts of the cave. A great part of these were found at various depths of from 1 to 10 feet beneath the top of the cave-earth; but a considerable number were also found on the surface of the cave-earth, generally where there was no stalagmite, especially in the southern extremity of the Reindeer Gallery, and were the remains of

small rodents, no doubt of comparatively recent introduction. Omitting these 519 specimens, the number of bones in the cave-earth is reduced to 1102.

No coprolites were found in any part of the cave.

The bones belong to 20 or 21 animals, referred by Dr. Falconer and Mr. Busk to the following species:—

1. *Elephas primigenius.*	11. *Ursus spelæus.*
2. *Rhinoceros tichorhinus.*	12. —— *ferox* (*priscus*).
3. *Equus caballus.*	13. —— *arctos.*
4. *Bos primigenius*?	14. *Canis vulpes.*
5. —— *taurus*?	15. *Lepus timidus.*
6. *Cervus elaphus.*	16. —— *cuniculus.*
7. —— *tarandus.*	17. *Lagomys spelæus.*
8. *Capreolus capreolus.*	18. *Arvicola amphibius.*
9. *Felis leo* (var. *spelæa*).	19. —— ——?
10. *Hyæna spelæa.*	20. *Sorex vulgaris.*

On these Mr. Busk makes the following remarks:—

1. Remains of the Mammoth occurred in five situations, viz. the Reindeer Gallery, the Flint-knife Gallery, the West Chamber, South Chamber, and Steep Slide Hole. For the most part they appear to have lain at a considerable depth, and, with one or two exceptions, they were the only remains met with in the shingle bed. On the whole, it would appear that these remains occupied the deepest levels in the cavern.

The most remarkable specimens among these bones are *an astragalus* and the lower end of the corresponding tibia, which were found in close contiguity, and doubtless belonged to the same individual. It is tolerably certain that they had been introduced into the cavern whilst still connected by the soft parts. The astragalus, which is nearly entire, is much gnawed on the inner, anterior, and hinder aspects, and the portion of tibia is also gnawed on the same three sides, that upon which they lay presenting no marks of the Hyæna's teeth. It is to be remarked that no fragments of the tusks or teeth of the mammoth were met with, whence it may perhaps be concluded that the remains discovered were merely the relics of parts brought in by Carnivora for prey.

2. The remains belonging to the Woolly Rhinoceros were rather numerous, amounting to between sixty and seventy specimens: of these no less than forty-nine occurred in the "Reindeer" and "Flint-knife galleries;" they were usually met with at a considerable depth in the cave-earth. In the South Chamber, however, one specimen, most probably rhinocerine, though possibly elephantine, was found "in the stalagmite floor," whilst other undoubted fragments occurred "immediately beneath it." A curious circumstance with respect to the rhinocerine remains consists in the numerous instances of the middle portion of the femur, or that part which includes the base of the third trochanter, which have been gnawed

by Hyænas in almost exactly the same manner—a condition in which Mr. Boyd Dawkins informs us numerous remains of the thigh-bone of Rhinoceros were found in Wokey Hole. Many of the rhinocerine bones, however, presented no marks of gnawing or of being water-rolled.

3. The equine remains include about thirty well-marked specimens, indicating a species varying from 13 to 15 hands high, and indistinguishable from the existing Horse. They varied a good deal in external characters, some appearing as ancient as those of the Hyæna, whilst others presented, like some of those of the Bear, a far more modern aspect.

4, 5. The bovine remains are not very numerous, and, with the exception of the teeth, most of them are very imperfect. They occurred chiefly in the Flint-knife Gallery and West Chamber. From the size of the bones and certain other characters, these bovine remains appear to have belonged to more than one species, which may be provisionally regarded as *Bos primigenius* and *Bos taurus* (var. *longifrons*).

6. Not more than ten or eleven specimens certainly referable to *Cervus elaphus* appear to have been met with, and these, with three exceptions, lay at an average depth of between three and four feet in the cave-earth in the Reindeer and Flint-knife Galleries. The majority of the specimens consisted of either the basal portion of shed horns or fragments (2) of round antlers.

7. Next to those of the Bear, the remains of the Reindeer are by far the most abundant of all others in the collection. About seventy well-characterized specimens have been determined. They occurred in about fifty different localities in the Reindeer and Flint-knife Galleries and in the West Chamber; they occurred at a rather less depth in the Reindeer than in the Flint-knife Gallery, but in both localities generally at a depth of little more than two or three feet in the cave-earth. Several (five or six out of thirty-five) were found lying on the surface or immediately beneath it, some below and others either on or protruding through the stalagmite floor. Some are incrusted with a thin, crystalline stalagmitic deposit. But it is a curious circumstance that several among them, though met with in different parts of the cavern and at considerable distances apart, appear to be parts of the skeleton of one and the same young animal. Most of the Reindeer bones, but not all, present marks of gnawing.

8. The specimens of remains distinctly belonging to the Roebuck are not more than ten or twelve in number. Some among them are gnawed, but others not; but they exhibit, perhaps more distinctly than any of the other bones, indications of having lain long on the surface of the ground, and exposed to atmospheric influence, before their introduction into the cavern.

9. The remains of the Cave Lion are scanty in number, but amply sufficient to show that carnivora formed part of the most ancient fauna of the cavern.

10. Next below those of the Reindeer in frequency are the remains belonging to *Hyæna spelæa*, amounting to about sixty in number; and they occurred in nearly equal proportions in three divisions of the cavern, their average depth in the cave-earth exceeding 3 feet. No specimen seems to have occurred above the stalagmite floor, nor more than four or five on or very near the surface of the cave-earth. Their general condition betokens great antiquity, and very few among them exhibit any indication of gnawing, weathering, or rolling. Many among them are quite perfect, and in all probability belong to one and the same animal. The bones and teeth clearly indicate several individuals of all ages, from that at which the epiphyses of the femur and tibia were still ununited, up to one at which the canine teeth were almost worn away; but no certain trace of a fœtal or very young Hyæna is perceptible. All the teeth belong to the permanent series.

11, 12, 13. Of all the animal remains discovered in the cavern, those belonging to the Bear are by far the most numerous, and in some respects, more especially with regard to their distribution, perhaps of the greatest interest.

In the present, as in almost every instance of the occurrence of ursine remains in caverns, the extreme variation in size and other characters of the different bones and teeth is so great, as naturally to lead to the belief that they must have belonged to more than one species.

The number of specimens clearly determined is about 350 or 360, and to these, from among the less easily determined fragments, probably forty or fifty more might be added. But, in part explanation of this large number, it should be remarked that it includes several collections, each composed of numerous bones of the skeletons of individuals of various ages found lying together at the same spot.

Of the specimens enumerated above, about 116 occurred in the Reindeer Gallery, 214 in the Flint-knife Gallery, 26 in the West, and only 1 in the South Chamber.

Taking all the levels at which these remains were found, the mean horizon of the genus *Ursus* appears to be about the same as that of the Reindeer, and a few inches above that of the Hyæna. It must also be remarked that, as compared with the latter species, a much larger proportion of the ursine remains were found lying on or near the surface of the second bed, or sometimes even imbedded in the stalagmite itself. Another remarkable circumstance connected with the ursine remains is the frequency with which bones, obviously belonging to one and the same skeleton, were found collected together in one spot; and to this remark it may be added that the great number of very young, or even fœtal bones, affords the strongest possible evidence that the Bear actually inhabited the cavern. The close investigation and comparison of the ursine bones and teeth leaves little doubt that they represent the remains of three distinct

forms or species, viz. *U. spelæus*, *U. ferox fossilis** (*U. priscus*, Goldf.), and *U. arctos*, or the existing European and Asiatic Brown Bear: but the evidence tending to show this would demand more space than can be given to this abstract.

14. Strange to say, the genus *Canis* is represented only by the Fox, no trace of the Wolf having occurred in the entire collection; and of the Fox not more than seven specimens were met with, five of which occurred in the Reindeer Gallery, and all but one on or near the surface: but in no other respect is there the slightest difference in the characters of the bones and teeth.

15–20. Innumerable bones of the Hare and Rabbit, of different sizes and of all ages, occurred in the Reindeer and Flint-knife Galleries, and for the most part on or near the surface of the third bed, or in the stalagmite floor. The only marked exceptions to this are in "find" No. 92, which affords the tibia of a young Hare, found at a depth of three feet in the Reindeer Gallery.

The most interesting of all the rodent remains, however, is a fragment of the skull, including, fortunately, the entire maxilla, with all the teeth but one of *Lagomys spelæus*. It was found, together with numerous bones of the Polecat, Hare, Rabbit, Water-Rat, Shrew, &c., in the Reindeer Gallery, 110 feet from the Dyer's Entrance, lying on the surface of the second bed. It differs in no respect, as regards condition or appearance, from the other bones with which it was associated, and, like most of them, is very slightly dendritic.

Traces of Man.—Not a single human bone has been found in Brixham Cave; but thirty-six rude flint implements and clips, referable in great part, or wholly, to man's workmanship, were met with in different parts of the cave; of these, sixteen were found in the bed of shingle, at depths in it of from 6 inches to 12 feet, or, including the overlying beds, of from 6 to 18 feet from the surface of the cave-floor. Seventeen were found in the Reindeer Gallery, five in the Flint-knife Gallery, eleven in the West Chamber, and three in other parts. In fourteen instances their infraposition to bones of the Mammoth, Rhinoceros, Hyæna, Tiger, Bear, Reindeer, Red Deer, Horse, and Ox is perfectly well proved, as many as 120 of such bones having been discovered higher in the cave-earth over where these flints were found. One specimen in particular deserves notice; it is a roughly shaped flint hatchet, broken in two, but the two halves not worn, and their jagged edges fitting perfectly. These two parts were found buried in the cave-earth in distant parts of the cave, and it was not until some time after their discovery that Dr. Falconer detected their relation.

The report then proceeds to discuss the origin of the cave, and the mode in which its contents were accumulated, considering this to be a typical case for a large number of ossiferous caves.

* It was the specimens from this cave that first led Mr. Busk to recognize this northern American species amongst the ossiferous cave-fauna of Europe.

The cave follows the course of two lines, or planes of joint, traversing the limestone, along which the galleries have been excavated by the long-continued action of water. Mr. Pengelly attributes the excavation "to a stream of fresh water not subject to great floods, and flowing constantly from the West Chamber through the Flint-knife and Reindeer Galleries to the Steep Slide Hole." Mr. Bristow, on the other hand, is inclined to attribute some portion of the formation of the cave to marine action at a time when the land was lower. The same causes are supposed by these gentlemen to have led respectively in each case to the accumulation of the bed of shingle; and as no shells are found in it, this question has to be settled on other than palæontological evidence. Both, however, refer the cave-earth to subaërial action, Mr. Pengelly considering it to have been chiefly carried on by running water, whereas Mr. Bristow views it as mainly due to the erosion of the limestone, whereby the calcareous portion has been dissolved and the insoluble portion left behind as a red loam. In the same way Mr. Pengelly is of opinion that the bones were likewise carried in from the exterior by the action of running water; while Mr. Bristow, like Mr. Busk, thinks that they are for the most part those of animals which were carried into the cave to be devoured.

It is difficult to reconcile the conditions of the bones and their numbers with their introduction by water; for a large number are not at all worn, and a great proportion of them show sharply graven marks of gnawing. At the same time the local origin of the cave-earth cannot be admitted, for the mass of limestone removed is quite insufficient to have formed so large a quantity. Your reporter is of opinion that the bones were brought into the cave by predatory animals, and that they are, in some cases, the bones of animals which died there; but he considers the cave-earth to have been introduced by water, not by a constant, slow running stream, but by occasional floods; for all the circumstances of the case seem to show that at certain periods the cave was dry and at other periods flooded by fresh water, that during the former intervals the cave was frequented by Hyænas which brought in their prey to devour, and that with each successive inundation successive collections of bones were covered up and imbedded in the sediment with which the flood-waters were charged. The weathering of some of the bones noticed by Mr. Busk is a weathering which may have occurred in the cave; for it is evident that all the bones were not at once imbedded in the cave-earth, as there are some which are incrusted with a thin coating of stalagmite that must have been produced by their lying for a time exposed to the dripping from the roof before they were covered up. Others, on the other hand, in places free from the dripping, would weather to a certain degree, as they would weather on the surface of the ground, according to the length of exposure. That the formation of stalagmite, under certain favourable conditions, did proceed during the whole cave-period is apparent from the section given by Mr. Pengelly, where at

one spot, favourably situated, a series of thin seams of stalagmite, contemporaneous with the cave-earth, was met with.

Although the evidence proves the contemporaneity of Man with the cave animals, it is doubtful whether Brixham Cave was at that period ever inhabited by man. Caves have constantly been a place of resort for uncivilized man, either for shelter or for security. When resorted to permanently for these objects, traces of his habitation, in the form of refuse (whether of bones cast away at meals, of broken and lost tools of daily use, and, after the discovery of fire, of hearths and their surroundings), necessarily occur in quantities more or less abundant, according to the length of man's habitation; but no such evidence of his presence exists. It may rather be supposed that the worked flints were lost or left behind by man during occasional visits to the cave, either for the sake of temporary refuge, or in following prey which may have sought shelter there.

Looking at all the phenomena of Brixham Cave, the conclusion your reporter has arrived at is that the formation of the cave commenced and was carried on simultaneously with the excavation of the valley—that the small streams flowing down the upper tributary branches of the valley entered the western openings of the cave and, traversing the fissures in the limestone, escaped by lower openings in the chief valley, just as the Grotte d'Arcy was formed by an overflow from the Cure taking a short cut through the limestone hills, round which the river winds. These tributary streams brought in the shingle bed No. 3 which fills the bottom of the fissure. It was only during occasional droughts, when the streams were dry, that the cave seems to have been frequented by animals, their remains being very scarce in that bed, while indications of man are comparatively numerous. As the excavation of the valley proceeded, the level of the stream was lowered and became more restricted to the valley-channel. The cave consequently became drier, and was more resorted to by predatory animals, who carried in their prey to devour, and was less frequented by man. At the same time, with the periodical floods which there is every reason to believe, from other investigations, were so great during the quaternary period, the cave would long continue to be subject to inundations, the muddy waters of which deposited the silt forming the cave-earth, burying progressively the bones left from season to season by succeeding generations of beasts of prey. By the repetition at distant intervals of these inundations, and by the accumulation during the intervening periods of fresh crops of bones, the bone-bearing cave-earth was gradually formed. During this time the occasional visits of man are indicated by the rare occurrence of a flint implement lost probably as he groped his way through the dark passages of the cave. As the valley became deeper, and as, with the change of climate at the close of the quaternary period, the floods became less, so did the cave become drier and more resorted to by animals. At last it seems to have become a place of permanent resort for bears: their remains in all stages of growth, including even sucking cubs, were met with in the upper part of the cave-earth,

in greater numbers than were the bones of any other animal. These animals resorted especially to the darker and more secluded Flint-knife Gallery, where 221 out of 366 of their determinable bones were found, whereas only twenty-six were met with in the Reindeer Gallery.

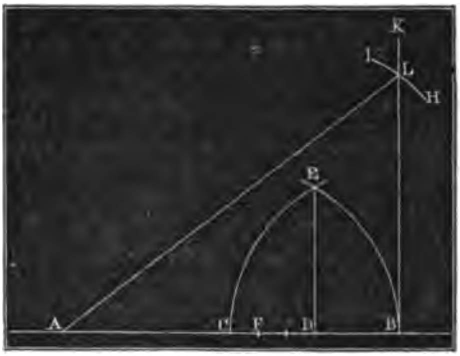
Finally, as the cave became out of the reach of the flood waters, the drippings from the roof, which up to this period had, with the single exception before mentioned, been lost in the accumulating cave-earth or deposited in thin calcareous incrustations on the exposed bones, now commenced that deposit of stalagmite which sealed up and preserved undisturbed the shingle and cave-earth deposited under former and different conditions. The cave, however, still continued to be the occasional resort of beasts of prey; for sparse remains of the Reindeer, together with those of the Bear and Rhinoceros, were found in the stalagmite floor. After a time, the falling in of the roof at places (and any earthquake movement may have detached blocks from it) and the external surface-weathering stopped up some parts of the cave, and closed its entrances with an accumulation of débris. From that time it ceased to be accessible, except to the smaller rodents and burrowing animals, and so remained unused and untrodden until its recent discovery and exploration.

At this time it is not necessary to contend for the correctness of many of the early observations, so long contested, in evidence of the antiquity of man; they are too numerous and too well attested to admit of doubt, and are now generally accepted by geologists. At the same time it is to be observed that the discovery and exploration of Brixham Cave have had a very important influence in bringing about such a result. The discoveries of Schmerling and others had dropped into oblivion, the assertions of M. Boucher de Perthes were ignored, until the certainty of the facts early established in the exploration of Brixham Cave showed the strong *primâ facie* evidence in proof of the contemporaneity of man and of the great extinct mammalia, and at once led to the conclusive investigation of the Somme valley. The evidence of Brixham further has its own special points of value,—in the completeness of its record, in the certainty of its data, and in the fact of its having been the first ossiferous cavern worked out in a systematic and complete manner, of which the record, plans, and sections are complete, and of which every specimen is preserved, duly marked and registered, so that it can at any time be assigned to its exact original place in the cave. This work, in fact, is not only important as the first systematic attempt to solve an obscure natural-history question, but it may further be considered as having inaugurated the great question, since so well established on other additional evidence, of the Antiquity of Man.

XXIII. "Approximate Geometrical Solutions of the Problems of the Duplication of the Cube and of the Quadrature of the Circle." By William Hayden. Communicated by Prof. G. G. Stokes, Sec. R.S. Received November 30, 1871.

I. *Duplication of the Cube.*

Let A B be the given cube root. Erect the perpendicular B K; bisect A B in C, and with radius C B describe the arcs intersecting at E; let fall the perpendicular E D and trisect C D; then, with radius B F from E as a centre, describe the arc H I cutting B K in L; join A L, which will be nearly equal to the cube root of double the cube of A B, the amount of error being very small, which is proved as under.



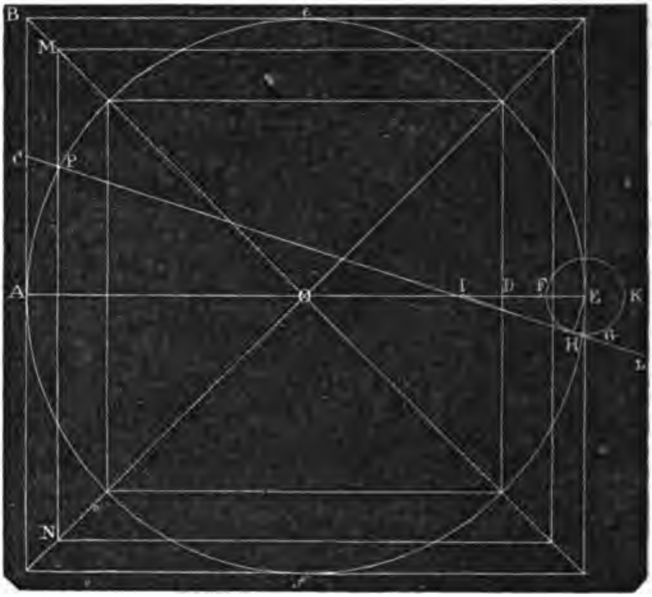
Let AB=3. Then

$$CB=1{\cdot}5,$$
$$BF=1{\cdot}25,$$
$$BD=\ {\cdot}75,$$
$$DE=\sin 60^\circ\times CB,$$
$$\sqrt[2]{(BF^2-BD^2)}=1,$$
$$(\sin 60^\circ\times CB)+1=BL,$$
$$\sqrt[2]{(AB^2+BL^2)}=AL=3{\cdot}7796264\ \ldots$$
$$\sqrt[3]{2}=1{\cdot}2599210$$
$$\tfrac{1}{3}AL=\underline{1{\cdot}2598754}$$
$$\text{Error}\ldots\ldots\ {\cdot}0000456$$

II. *Quadrature of the Circle.*

In and about a given circle, as A *f* E *e*, draw the inscribed and tangent squares in the manner indicated by the figure, with their diagonals. Draw the diameter A E bisecting the opposite sides of the said squares; bisect A B in C, and D E in F; set off from E and D respectively E G and D I equal to D F, E F and join G I; from E let fall E H perpendicular to G I, and with radius E H describe the circle H K; draw the line C L touching

the circle H K, and cutting the circle A f E e in P; then through the point P, parallel to A B, draw M N terminating in its intersections with the



diagonals, as shown by the diagram, which will be one side of a square very nearly equal to the circle A f E e.

[The author has appended a mathematical calculation proving, in a somewhat indirect manner, that his construction gives an exceedingly close approximation. The construction leads directly to the following result:—

Let the radius of the circle be taken as unity, and let s be a side of the square given by the construction; then

$$s=\frac{\sqrt{3+4m}+2m^2-m}{1+m^2},$$

where

$$m=\frac{2+r\sqrt{17-4r^2}}{8-2r^2}, \quad r=\frac{3}{20}(\sqrt{10}-\sqrt{5})$$

(r is the radius of the circle H K, and m the tangent of the inclination of C L to A E).

Mr. Hayden has reduced these expressions to numbers, with the following result:—

$$r= \cdot 13893145240028844\ldots,$$
$$m= \cdot 322999477624\ldots,$$
$$s=1\cdot 7724538677\ldots,$$
$$s^2=3\cdot 141592713\ldots,$$

instead of $3\cdot 141592653\ldots$

—September. 1872 G. G. S.]

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Appendix.

INTRODUCTION.

As it was understood that the requirements of the Public Service would not permit the employment, during any part of the last year (1871), of either the 'Porcupine' or the 'Lightning,' for the continuation of the Deep-sea Researches carried on by my Colleagues and myself in the three preceding summers, it was not deemed advisable by us that the Council of the Royal Society should be moved to make any application to the Admiralty in furtherance of this object. Early in June, however, I was informed by the Hydrographer that as the Surveying-ship 'Shearwater,' under the command of Capt. Nares, was about to proceed to the Mediterranean, and would not be required to enter on her work until the end of October, an opportunity would present itself for making further researches on the Gibraltar Current, in which, if so disposed, I should have the advantage of being associated with Capt. Nares; whilst, if inclined to proceed as far as Egypt, I might have an opportunity of prosecuting in the Eastern basin of the Mediterranean some of the Physical and Biological researches which I had carried out last year in the Western.

Although the results of the conjoint inquiries which had been made by Capt. Calver and myself during the 'Porcupine' Expedition of 1870 seemed to us to leave no reasonable doubt as to the existence of an outward Under-current in the Strait of Gibraltar, yet we both felt extremely desirous that the matter should be more thoroughly examined; for we were fully conscious that the proof could not be regarded as complete, until direct *mechanical* evidence should be obtained by the "current-drag" of the passage of Mediterranean water over the "ridge" or "marine watershed" between Capes Trafalgar and Spartel, which forms the proper boundary of the Mediterranean basin, the evidence we had obtained of such passage being *inferential*, and therefore open to objection. And as we saw, in addition, that the rate, perhaps even the direction, of this under-current was subject to variation under the influence of Winds and Tides, we felt that the

subject could not be fully elucidated without a far more prolonged and systematic study of its phenomena than it was in our power to carry out.

But, further, I had endeavoured to correlate the phenomena of the Gibraltar Current with those of the Bosphorus and Dardanelles Currents, and of the Baltic-Sound Current, under one general Physical Theory; and also to show that this Theory also applies to the doctrine of a General Oceanic *vertical* Circulation sustained by difference of Temperature. For whilst, in the case of Inland Seas, a disturbance alike of level and of equilibrium between their water and that of the outside Ocean will be produced either by excess of evaporation over the inflow of fresh water, or by an excess of the inflow of fresh water over evaporation, the like disturbance will be produced at the two extremes of each quadrant of the Earth along which there is a free communication between the Polar and Equatorial seas, by the opposite effects of Polar Cold and Equatorial Heat.

This view has been strongly opposed, however, on the one hand by Captain Spratt, and on the other by Mr. James Croll. The former has not merely affirmed* that this Physical Theory is altogether disproved, as regards the Bosphorus and Dardanelles Currents, by his own observations, but has called in question the validity of the evidence obtained by the inquiries of Capt. Calver and myself in regard to the Gibraltar Undercurrent, and has expressed his disbelief in the Under-current Theory altogether.—Mr. Croll, on the other hand, taking his stand upon what he regards as a well-established *datum* in regard to the gradient necessary to produce movement in water, considers that he has demonstrated the complete inadequacy of Thermal agency to maintain the General Circulation I advocate†.

The objections of a Surveying Officer of Capt. Spratt's ability and experience could not but be entitled to great respect; and I had therefore much satisfaction in availing myself of the opportunity afforded me by the Hydrographer, for cooperating with Capt. Nares in the re-investigation of the Gibraltar Currents, and for prosecuting such other scientific inquiries during the voyage to Alexandria as circumstances might admit.

The 'Shearwater' being specially fitted for the Surveying Service, only such arrangements could be made for Scientific research as were consistent with its requirements. And since it was not considered expedient to include *deep* Dredging in our *programme*, the following were the objects which I proposed to myself, in addition to the investigation of the Gibraltar Current:—

I. To ascertain whether *Serial* Temperature-soundings off the Coast of Portugal would justify the inference I had drawn, from the correlation of the *Bottom*-soundings taken in the previous year (Report for 1870, §§ 79, 80: Proc. Roy. Soc. vol. xix. p. 188), as to the existence of a

* Proceedings of the Royal Society, June 15, 1871 (vol. xix.).

† See his papers "On the Physical Cause of Ocean-currents" in the Philosophical Magazine for Oct. 1870 and Oct. 1871.

very distinct division between the *upper warm* and the *lower cold* stratum, in that portion of the Eastern Atlantic.

II. To ascertain whether any evidence would be furnished, by the relative Specific Gravities of the *surface-* and *bottom-*waters on the Atlantic slope of the "marine watershed" between Capes Spartel and Trafalgar, of the passage of Mediterranean water over that ridge.

III. To make a further Dredging-exploration of the comparatively shallow area between Sicily and the Coast of Africa,—part of it constituting the Skerki and Adventure Banks,—from which several specimens were obtained last year that proved of great interest.

IV. To make a sufficient number of deep Soundings in the Eastern Basin of the Mediterranean, to ascertain whether its water corresponds in Temperature and Density with that of the Western basin; and also, whether the deposit on its bottom is of the same character, and whether the water immediately above it is copiously charged, as in the Western basin, with very finely divided particles in suspension.

V. To ascertain, by analysis of the Gases of this bottom-water, whether the proportions of Oxygen, Nitrogen, and Carbonic acid correspond with those which had been found in the Northern Atlantic to be compatible with the presence of an abundant Fauna; or whether, as I suggested in my last year's Report (§ 103), the stagnation of the deeper part of the Mediterranean basin (in consequence of the want of Thermal Circulation) is attended with such an excess of Carbonic Acid and diminution of Oxygen, as is incompatible with the existence of Animal Life.

VI. To carry a series of Dredgings along the Northern Coast of Africa, eastward from Malta, in moderate depths, to ascertain how far its Fauna presents the same characters, and the same limitation to depths of between 150 and 200 fathoms, that it does in the corresponding part of the Western Basin.

With the view of enabling me to obtain the requisite assistance in carrying out the Zoological portion of the inquiry, and of meeting other scientific expenses, the Council of the Royal Society assigned a sum of £100 from the Government Grant; and this enabled me to secure, as on former occasions, the services of Mr. Laughrin as Dredger and Sifter.—A further sum of £50 was assigned to me from the Donation Fund, as a provision for the expenses of our return from Alexandria, the point at which we were to quit the 'Shearwater.'

The Admiralty having charged itself, as on former occasions, with the provision of Thermometers and Water-bottles, it was only requisite for me to provide the Apparatus needed for Gas-Analysis and for the accurate determination of Specific Gravities. As this last point was of special importance, and as I had found the use of a delicate Hydrometer on board ship, when there was even a trifling roll, to be attended with uncertainty, I thought it worth while to make trial of the method of taking Specific

Gravities by a graduated series of *bulbs*, adjusted to indicate differences of 0·1. On consulting Mr. Casella, I found him willing to undertake the construction of such a series, which should range from 1025·0 to 1030·0. These I have found to answer admirably, as the motion of a ship interferes much less with their performance than it does with that of a delicate Hydrometer.—I should strongly recommend that a similar series, graduated to every 0·2 (which is quite close enough for ordinary purposes), should be provided for general use at sea, instead of the Hydrometers at present furnished by the Meteorological Department. Every case of such bulbs should include a small Thermometer suited for taking the exact temperature of the water of which the Specific Gravity has to be ascertained, with a Table whereby the observed Specific Gravity may be reduced to a common standard—say 60°. In every observation, the Temperature should be carefully noted, and the requisite correction applied. I have strong reason to believe that the neglect of such correction has led to no small exaggeration of the differences between the Specific Gravities of the surface-water of the Ocean in different regions*. The record of observations ought either to state the result *as corrected*, or to state the Temperature at which each Sp. Gr. determination, if uncorrected, was made.—In the present Report all the Specific Gravities are reduced to the Standard of 60° Fahr.

As it is not probable that I shall again take a personal share in the work of Deep-sea Exploration, and as the present Report will therefore close the account of the Physical inquiries which I have been led to consider as my special department, I have thought it desirable to develope somewhat at length what I regard as the bearings of the results obtained by these inquiries upon the Doctrine of a General Oceanic Circulation sustained by difference of Temperature. This has involved a careful examination of the whole question of the Gulf-stream: as elucidated, on the one hand, by the careful and elaborate Surveys prosecuted near the American Coast under the able direction of the late Prof. Bache; and, on the other, by the Thermometric observations on the Surface-temperature of the Ocean, which have been recently correlated as regards the Atlantic by our own Hydrographic and Meteorological Departments, and as regards the Arctic Sea by Dr. Petermann. As no similarly comprehensive examination has been made, so far as I am aware, by any other scientific inquirer, and as the doctrine put forth on the subject by Mr. Croll is likely, if not thus scrutinized, to command the unquestioning assent of those who regard him as a high authority "on the subject of Oceanic Currents and their bearings on Geological questions" †, I venture to hope that the inclusion of its results as an Appendix to this Report will not be deemed inappropriate.

* I cannot but think that there must be some such fallacy in the statement of Dr. Rattray (Transact. of Linn. Soc. vol. xxvii. p. 272), that he encountered a range of Specific Gravity in the Ocean-water of the Pacific from 1023¼ to 1029.

† Address of the President of the Geological Society, 1872, p. xxviii.

Part I.

Temperature-Phenomena of the Atlantic, in relation to those of other Seas and to the General Oceanic Circulation.

1. The first scientific work to which we applied ourselves was the obtaining, on the 16th of August, a deep Temperature-sounding in Lat. 41° 9′ N. and Long. 10° 2′ E.,—about 60 miles to the west of Oporto. The depth here struck was no less than 2100 fathoms; but unfortunately the line parted in reeling-in, and we lost two thermometers and a water-bottle. This incident was greatly to be regretted, as the results of Serial Soundings at this depth would have been of peculiar interest.—Circumstances were unfavourable, however, to the renewal of the attempt in this locality.

2. On the 17th we took three Soundings nearer the Coast of Portugal; the first two in Lat. 38° 20½′ N. and Long. 9° 23′ W., which, with a surface-temperature of 66°, gave a bottom-temperature of 54°·5 at 570 fathoms, and 56° at 600 fathoms; while the third, in Lat. 38° 13′ N. and 9° 21′ W., gave 53° in 590 fathoms. These temperatures were higher by 5° or more than those which were obtained last year, nearly in the same locality, at corresponding depths (see Report for 1870, § 79); and I cannot account for this elevation in any other way, than by supposing that as we know the bottom along this part of the coast to be very rocky and irregular, with many "holes," the area within which the higher temperatures were found may have been cut off, as regards its deeper stratum, from the basin of the Atlantic,—just in the same manner as the deeper portion of the Sulu Sea appears to be cut off from the China Sea (§ 12).

	Atlantic.		Mediterranean.	
	° Fahr.	Below surface.	° Fahr.	Below surface.
		°		°
Surface	68	....	69·5	
10 fathoms	68·5	+ 0·5	59	—10·5
20 ,,	62	— 6	57·5	—12
30 ,,	61	— 7	56·5	—13
40 ,,	59·5	— 8·5	55·7	—13·8
50 ,,	59·5	— 8·5	55·3	—14·2
60 ,,	59	— 9		
70 ,,	58·8	— 9·2		
80 ,,	58·5	— 9·5		
90 ,,	58	—10		
100 ,,	57·7	—10·3	54·7	—14·8

3. On the 19th of August we took another deep Sounding in Lat. 36° 47′ N. and Long. 9° 39′ W.,—about 60 miles to the S.W. of Cape St. Vincent. The surface-temperature being here 68°, the temperature of the bottom at 1560 fathoms was 37½°. A series of Temperature-soundings at intervals of 10 fathoms down to 100 fathoms having been then taken by Capt. Nares at my request, the results given in the Table, p. 539, were obtained, which it may be well to compare with the temperatures obtained at corresponding depths, and nearly under the same latitude, within the Mediterranean (Report for 1870, § 87, Series II.).

4. The continuity of the surface-temperature through the uppermost ten fathoms' stratum, with even a slight *increase*, was a phenomenon which I have not myself met with in any other instance, the temperature at 10 fathoms' depth being generally at least 3 degrees *below* that of the surface. But Capt. Chimmo, in his "Soundings and Temperatures in the Gulf-stream" (Proceedings of the Royal Geographical Society, vol. xv. 1871, p. 99) states that out of 45 carefully obtained observations between Halifax and the Mid-Atlantic, 26 gave a temperature at 12 fathoms *warmer* than the surface,—sometimes as much as 1½°,—10 colder, and 9 of the same temperature; whilst he further states that in the Pacific, off the Isalcos Mountains on the west coast of America, the temperature has been found not less than 10° or 11° higher at 12 or 15 feet below the surface than at the surface. This he presumes to be due to excessive evaporation, as he has often there found the difference between the wet- and the dry-bulb thermometer to be 9°, and on one occasion 11°. From this it seems probable that the marked differences observable between the rates of descent of the thermometers in the subsurface-stratum are partly referable to the *hygrometric* condition and movement of the air above, which will have an influence even more potent than Temperature *per se* upon the rate of evaporation from the surface.

5. Having been led to believe, by the bottom-soundings taken last year, that the temperature of the upper stratum of the Atlantic from 100 fathoms down to the "stratum of intermixture" would here fall pretty regularly to about 50° at 700 fathoms, I did not think it needful that time should be spent in carrying Serial soundings continuously downward, if the temperature at that depth should be found to accord with my expectations; and this it did in a very satisfactory manner, the thermometers sent down to 700 fathoms giving 50°·5. I therefore requested Capt. Nares to take successive temperature-soundings from 700 fathoms downwards, at intervals of 100 fathoms; and these also gave the result I had anticipated, the thermometer falling to 46° at 800 fathoms, to 42° at 900 fathoms, and to 38° at 1000 fathoms. From 1000 fathoms to the bottom at 1560 fathoms, the further reduction was only from 38° to 37½°.

6. Thus, then, this set of *Serial* soundings fully confirmed the deduction I had ventured to draw last year (Report for 1870, § 81), from the

bottom-soundings taken in the 'Porcupine' along the coast of Spain and Portugal, as to the well-marked distinction between the *upper warm stratum* and the *lower cold stratum*, with a *stratum of intermixture* between the two. For whilst the reduction of temperature beneath the superheated stratum of the surface was only from 57°·7 at 100 fathoms to 50°·5 at 700 fathoms—that is to say, 7°·2 in 600 fathoms, or at the rate of 1°·2 per 100 fathoms,—the reduction between 700 fathoms and 1000 fathoms was 12°·5, or at the rate of nearly 4°·2 per 100 fathoms; while the whole mass of the deeper water from 1000 fathoms down to the bottom at 1560 fathoms was found to have a temperature between 38° and 37½°.

7. The significance of these facts becomes most apparent when they are graphically contrasted (as in Plate III.) with the temperature-phenomena of the Mediterranean under nearly the same parallel; for, as was proved by the 'Porcupine' Temperature-soundings of 1870 in the Western basin, the temperature from 100 fathoms (at which depth the influence of direct insolation appeared to cease) down to the bottom at 1400 or 1500 fathoms is *uniform throughout* *, namely about 54½°. It is clear, therefore, (1) that depth *per se* does not give rise to any change in the temperature of sea-water occupying a basin which has only a superficial connexion with the great Ocean-system; and consequently (2) that the diversities of temperature shown in the different strata of the Atlantic must depend upon their derivation from different sources.

8. Now with regard to the *upper* stratum lying between 100 and 700 fathoms, there is no evidence of its derivation from any Southern source; on the contrary, the Temperatures taken at 200 and 400 fathoms, in the recent voyage of the New York School-ship 'Mercury' from Sierra Leone to Barbadoes (the Report of which has been kindly transmitted to me by Prof. Draper), indicate that below the stratum superheated by direct insolation, the temperature is even *lower* between the Tropics than it is in this more northern portion of the Atlantic area. For the average of nine observations at 200 fathoms, taken where the depth of the Oceanic basin was 1000 fathoms or more, was almost exactly 52°, the range being only between 54° and 50°; and the temperature at 400 fathoms, about 350 miles east of Barbadoes, was only 47°.

9. If this last fact should be confirmed by further inquiries, it will furnish a powerful argument in support of the doctrine of a *vertical* Oceanic Circulation which I have repeatedly advocated; since it is obvious that as it will be in the Intertropical area that the cold bottom-water will be most rapidly brought up to the surface (Report for 1870, § 127), the temperature of the stratum not yet within reach of direct insolation might

* This uniformity had been previously observed by Capt. Spratt; although, from the want of "protection" of his Thermometers, he had assigned too high a temperature to the deeper stratum of the Mediterranean basin.

be expected to be *lower* than it is in the Temperate area. And it is a very curious confirmation of this view, that three of the temperatures taken in the 'Mercury' at 200 fathoms, which ranged several degrees higher than the average,—viz. 60°, 60°, and 58°,—were over a bottom (between Sierra Leone and the Cape-Verde Islands) of which the depth ranged only from 290 to 680 fathoms; though to the south as well as to the north of these stations, where the depths were respectively 1000 and 1100 fathoms, the temperatures at 200 fathoms were 53° and 54°. For if bottom-water, in its gradual rise towards the surface, starts from great depths at an almost glacial temperature, it will obviously be much colder, until it has come under the direct influence of insolation, than if it has started from a shallower bottom at a temperature of (say) 45°.

10. But whilst the Temperature of the *upper* stratum in our Atlantic sounding does not give any definite indication of the derivation of its water from any remote source, the case is very different in regard to the temperature of the stratum *below* 1000 fathoms. For I am at a loss to conceive how the rapid fall in the Thermometer between 700 and 1000 fathoms, and the depression of Temperature below 38° which exhibits itself in this Sounding down to 1560 fathoms, and in the Soundings taken by the 'Porcupine' in 1869 from 1000 fathoms down to 2090 and 2435 fathoms, are to be accounted for, unless on the hypothesis that the temperature of this stratum has been reduced by the flow of glacial water from the Polar area towards the Equatorial. For it seems clear that any mass of sea-water which remains long enough on the same bottom, *must* come to acquire a temperature *at least as high* as that of the bottom, unless cooled below this either by the action of Atmospheric cold on its surface, or by the intrusion of colder water from some extraneous source. Now, in the case of the Western basin of the Mediterranean, the *uniform temperature* ($54\frac{1}{2}$°) of the whole stratum beneath 100 fathoms agrees so well on the one hand with the *mean temperature of the crust of the earth* in that region (Report for 1870, § 89), and on the other with the *lowest winter temperature* of its surface*,—while the comparative shallowness of the "ridge" between Capes Spartel and Trafalgar so effectually prevents the admission of any considerable body of water of a temperature below $54\frac{1}{2}$° from the depths of the Atlantic outside,—that we may fairly assume that temperature to be *the normal of the Latitude.* And since, in the Eastern Atlantic outside the Strait, the whole mass of water below 1000 fathoms has a temperature *from sixteen to eighteen degrees below this normal,* which cannot be attributed to the action of surface-cold in the locality itself, the inference seems irresistible that this depression must be produced and maintained by the *convection of cold* from the Polar towards the

* See the Memoir of M. Aimé on the Temperature of the Mediterranean, in 'Ann. de Chim. et de Phys.' for 1845.

Equatorial area,—the low conducting-power of water altogether forbidding the idea that such a permanent depression can be sustained by *lateral conduction* from the Polar area against the warming influence of the bottom beneath and of the air above. And if this be admitted as probably true with respect to the Temperate area to which my own inquiries have been limited, with how much more force does the same argument apply to the Intertropical region, where, with a surface-temperature rarely, if ever, falling below 75°, we find a temperature of 47° at so small a depth as 400 fathoms, and a *bottom*-temperature but little above the freezing-point of fresh water—the "protected" Thermometers used by Commander Chimmo in the Indian Ocean having registered 35°·2 at 1806 fathoms, 33°·6 at 2306 fathoms, and 32° at 2656 fathoms, in Lat. 3° 18½′ S., and Long. 95° 39′ E. (See p. 590.)

11. If any confirmation of this view be thought requisite, it is supplied by a similar comparison of the Temperatures of two other Inland Seas with those of the Oceanic Basins with which they communicate. The Red Sea, like the Mediterranean, is entirely cut off from communication with the deeper and colder stratum of the Arabian Gulf, with which its surface-layer communicates through the shallow Strait of Babelmandeb. Whilst the lowest (February) temperature observed in that surface-layer, even in the northernmost extension of the Red Sea known as the Gulf of Suez, is 71° (as I learn from Capt. Nares, who has been recently engaged in its survey), that temperature is there carried uniformly down to the bottom at 450 fathoms; and it may hence be pretty certainly affirmed that no lower temperature than this will be found in the southern portion of the Red Sea, even on a bottom exceeding 1000 fathoms in depth, since the lowest surface-temperature of that portion is probably never less than 75°. Yet in the Arabian Gulf the temperature at a depth of 2000 fathoms is certainly *not above*, and is very probably *below* 36°·5 *.

12. The like contrast is shown by the Temperature-soundings of Commander Chimmo (which have been kindly communicated to me by the Hydrographer), between the deep temperature of the Sulu Sea,—a small area between the north-eastern portion of Borneo and Mindanao,—and that of the China Sea. The former, though not ostensibly an inland sea, being but very partially surrounded by land, is so shut in by reefs and shoals, as to have only a very superficial and limited communication either with the China or with the Celebes Sea. Notwithstanding this enclosure, its depth is very great, ranging to 1778 fathoms; and its Temperature-phenomena present *exactly the same contrast* with those of the China Sea that the temperature-phenomena of the Mediterranean present when compared with those of the Eastern Atlantic, as will be seen in the Table in the next page.

* See "Report" for 1869, § 120.

	Sulu Sea.	China Sea.
Surface	83°	84°
30 fathoms	....	77
40 „	....	74
50 to 80 fathoms........	....	71
100 fathoms	64·5	
120 „	....	62
150 „	....	56
200 „	56·2	51
250 „	....	49
308 „	51·5	
416 „	....	41
500 to 1778 fathoms	50	
550 to 1546 „	....	37

Thus it appears that with surface-temperatures almost exactly identical, and with a rate of descent through the first 100 fathoms which seems nearly the same, there is a most marked difference beneath. For whilst in the Sulu Sea the thermometer only falls to 56°·2 at 200 fathoms, to 51½° at 308 fathoms, and to 50° at 500 fathoms, and the temperature is uniform from that point down to the bottom at 1778 fathoms, it descends rapidly in the China Sea to 51° at 200 fathoms, thence to 41° at 416 fathoms, and thence to 37° at 550 fathoms, at which point it remains stationary down to the bottom at 1546 fathoms. This difference is attributed by Capt. Chimmo—in my opinion with adequate reason—to the exclusion from the Sulu Sea of the deep Polar flow which lowers the temperature of the China Sea. That the uniform temperature of its *deep* water from 500 fathoms downwards is *lower* by 4° or 5° than that of the Mediterranean, notwithstanding that the temperature of its *superficial* stratum is considerably *higher*, can be easily accounted for on the very probable supposition, that there are passages between the reefs and islands which admit water of a temperature below 50° from the outside Sea; in fact we might almost certainly fix the *minimum* depth of such passages as about 250 fathoms. (See also the Addenda in pp. 589, 590). These contrasts—graphically represented in Plate III. (in which the *dotted* lines show the temperatures of the *inland* Seas, and the *continuous* lines the temperatures of the adjacent *Oceans*)—are readily explicable on the doctrine of an underflow of Polar water towards the Equator; and how they are otherwise to be accounted for, I am myself at a loss to conceive.

13. This *under*flow involves, as its necessary complement, a movement of the *upper* stratum, bearing with it the warmth of Temperate seas, towards the Pole; and of such a movement very cogent evidence is afforded by the following comparison of the Temperatures obtained in the 'Porcupine' Expeditions of 1869 and 1870, between 100 and 500 fathoms

depth, at four Stations having an extreme range of 23° of Latitude,—the *first* of these stations being off the coast of Portugal, the *second* about 240 miles to the S.W. of Cork, the *third* about 100 miles south of Rockall, and the *fourth* about 120 miles to the N.W. of Stornoway:—

	I. N. Lat. 36½°.	II. N. Lat. 49°.	III. N. Lat. 56°.	IV. N. Lat. 59½°.	Extreme difference between I. and IV.
Air	70°	63°	58°	54°	16°
Surface	68	62·6	57·3	52·6	15·4
100 fathoms	57·7	51·1	48·5	47·3	10·4
200 "	55	50·5	48	46·8	8·2
300 "	53	49·6	47·8	46·6	6·4
400 "	52	48·5	47·5	46·1	5·9
500 "	51·5	47·4	45·8	45·1	6·4
Difference between 100 and 500..	6·2	3·7	2·7	2·1	

Now in the first place it is to be noted how closely the Temperature of the *surface* of the Sea corresponds with that of the Air; the reduction of both, in passing northwards, being at almost precisely the same rate, and the extreme differences being nearly identical. Next it will be observed that these differences diminish in amount from above downwards, showing that as the influence of insolation (which is for the most part restricted to 100 fathoms' depth) is lost, there is an increasing tendency to uniformity of temperature. This influence extends the deepest, as might be expected, at the southernmost station; the range between 100 and 500 fathoms being there 6°·2, whilst at the northernmost station it is only 2°·1. But while the temperatures at Station I. from 100 to 500 fathoms so nearly correspond with the uniform temperature of the Mediterranean between the same depths as to justify our assuming this to be nearly the normal of the Latitude, but little modified by the convection either of heat or of cold from any extraneous source (§ 8), the temperature at Station IV. of the whole stratum of water between 100 and 500 fathoms is so much *above the normal of its Latitude* as clearly to depend upon a *translation* of warm water from a Southern area. And when we compare these extremes with the intermediate temperatures of corresponding strata at Stations II. and III., we see how *regularly gradational* is the transition; the rate of diminution being about *half* a degree (Fahr.) for every degree of Latitude at 100 fathoms, and coming down to little more than *a quarter* of a degree between 300 and 500 fathoms. The inference seems unavoidable, that the entire stratum of water down to at least 500 fathoms has flowed northwards to the parallel of 59½° from some lower parallel, losing heat in its way by imparting it to the air above; that loss of heat being chiefly shown in

the *superficial* layer, in which it is more than twice as great as it is beneath.

14. Now, that there *is* a slow N.E. movement of the upper stratum of the water of this part of the North Atlantic, is affirmed by Admiral Irminger as the result of the careful discussion he has made of the reckonings kept by ships of the Danish Navy in their passages to and from Iceland and Greenland; the rate of that movement being estimated by him at from 0·8 to 4·7 miles per day*. On the other hand, that beneath this N.E. flow there is, in what I propose to call the "Lightning Channel," between the Shetland and the Faroe Islands†, a S.W. flow of glacial water which can scarcely have any other than a Polar source, appears to me to be no less unmistakably proved by the observations recorded in my Reports for 1868 and 1869. For if this glacial stream were not continually being renewed from its Polar source, it could not retain its temperature of from 32° to 29½° against the warmth imparted to it not merely by the crust of the Earth beneath, but by the warm stream coming up against it. And a further significant indication of its movement is afforded by the comparison of the two sets of *Serial* Soundings taken in the Cold Area (Report for 1869, Proc. Roy. Soc. vol. xviii. p. 456); namely, No. 64 extending to a depth of 640 fathoms, and No. 52 at which the depth was only 384. For although the latter Station was a degree to the *southward* of the former, and its surface-temperature was 2°·4 higher, the glacial stratum was sooner reached; the stratum from 300 fathoms to the bottom at 384 having a temperature between 30°·8 and 30°·6, which was only met with in No. 64 over its deeper bottom at a depth of from 400 to 450 fathoms. This case, therefore, seems precisely parallel to the creeping of Polar water up the sides and over the ridges of submarine hills, of which the U.S. Coast Surveyors have obtained distinct evidence in the Florida Channel (§§ 129–133); and it does not seem explicable upon any other hypothesis, than that of a *flow* of the colder stratum, which, if left at rest, would gravitate to the deeper part of the channel.

15. Proceeding still further North, we find this glacial stratum lying nearer and nearer to the surface the nearer we approach the Polar ice-barrier. This appears very clearly from the Temperature-soundings which have been recently taken in the neighbourhood of Spitzbergen by M. Charles Martins and by MM. Payer and Weyprecht; these having effectually disposed of the notion that the deeper water is warmer than the superficial, so that the Thermometer *rises* as it descends. That such a rise not unfrequently presents itself to a limited extent is indubitable, the observations

* Proceedings of the Royal Geographical Society, May 10, 1869.

† The discovery of the remarkable contrast between the bottom-temperatures met with at like depths in adjacent areas of this Channel having been made by the Temperature-soundings taken in the 'Lightning' Expedition of 1868, it seems not inappropriate to give it the name of that Ship; more especially as the 'Lightning' has a further historical interest, as one of the first two steam-vessels built in 1825 for the British Navy

of M. Charles Martins on this point having confirmed those formerly made by Scoresby off the coast of Greenland, in indicating a temperature *above* 32° at 40 fathoms, when the temperature of the surface is *below* 32°. This is easily accounted for by the *inferior salinity* of the cold surface-stratum, the reduction of its temperature and of its salinity being alike due to the melting of ice. For if the ice be that of Icebergs formed as Glaciers on land, the water into which it dissolves will be entirely fresh; whilst if it has been formed by the freezing of sea-water, so large a part of its salt will have been left out in the act of congelation, that the product of its liquefaction will be of comparatively low Specific Gravity; and thus Sea-water at or even below 32° may float upon water some degrees warmer than itself. That this is really the explanation of many cases of this kind, seems probable from the fact, which I have learned from Capt. Toynbee (of the Meteorological Department), that when our American Packet-ships, in crossing the Atlantic, meet with a sudden depression of surface-temperature, they find the Specific Gravity also of the surface-water to be sensibly lessened; both results being pretty certainly due to the melting of icebergs borne southwards by the Arctic Current.—But the strata of Ocean-water do not always arrange themselves in accordance with their relative Specific Gravities and Temperatures. Two currents may meet, as on the Agulhas Bank, in such a manner that the inclination of the Sea-bed throws up the colder one nearer to the surface than the warmer; and the former may maintain this superiority of position as long as its force of translation lasts. This agency was doubtless operative in the following remarkable case cited by Dr. Petermann:—"Lieut. Rogers, in 1855, found "in the Asiatic part of the Arctic Ocean a warm surface-current with water "of low Specific Gravity; beneath it a cold current; and then, again, a warm "current of heavier water; and all these strata running in opposite direc-"tions." I am informed by Mr. Leigh Smith, who last summer penetrated to the N.E. of Spitzbergen, that he has encountered a similar succession.—Cases of this kind, however, do not invalidate the general fact, that the glacial stratum in Polar Regions extends from less than 50 fathoms below the surface to the bottom, however deep. Thus Scoresby found the temperature at about 120 fathoms to be 29°, when it was 34° at the surface. Sir Edward Parry found the surface-temperature off Spitzbergen to vary from 31° to 28°, and the bottom-temperature at depths down to 100 fathoms to vary from 30° to 28°. Sir John Ross found the temperature in Lat. 72° 33′ N. and Long. 73° 7′ W. to be 35° at the surface, and to decrease gradually to 28½° at a depth of 1000 fathoms. And the recent observations of M. Chas. Martins have shown that from 40 fathoms downwards the temperature constantly undergoes reduction, until the thermometer stands below 29°*. This is confirmed by the still more recent observations of MM. Weyprecht and Payer, which further show that the stratum above 32° becomes thinner as it flows northwards:—

* The foregoing Temperatures are stated on the authority of Mr. Prestwich. See his Presidential Address for 1871, in the Quart. Journ. of the Geological Society, p. [illegible]

Lat. 72° 30′ N. Long. 44° E.		Lat. 76° 40′ N. Long. 55° E.		Lat. 77° 26′ N. Long. 44° E.	
Depth	° Fahr.	Depth	° Fahr.	Depth	° Fahr.
12 to 114 feet	40·6	6 to 36 feet	36·5	6 to 36 feet	36 to 35·2
		48 ,,	33·8	45 ,,	32·5
		60 ,,	32·0	60 ,,	32·5
		72 ,,	30·9	75 ,,	30·6
		90 ,,	30·6	90 ,,	30·4
144 ,,	36·5	120 ,,	29·7	120 ,,	29·1
174 ,,	35·6	180 ,,	29·8	180 ,,	28·8
234 ,,	34·3				
294 ,,	32·9	300 ,,	29·8		
360 ,,	32·9	....		360 ,,	29·1
450 ,,	32·0				
600 ,,	31·3				
800 ,,	29·7				

"The transition of the water from the higher to the lower temperature," they say "is, near the northern limit, a very rapid one, and nearly everywhere occurs in closest proximity to the ice, so that we were able in the thickest fog to run close up to the barrier under the guidance of the thermometer" *.

16. Thus, then, the additional information obtained from various sources since my last Report has but confirmed the representation therein given (§ 82) of the relation between the Cold and the Warm stratum in the water of the Ocean; and of that relation the doctrine of an upper and an under-flow propounded by Pouillet seems to give the only satisfactory *rationale.*—The existence of such a Vertical Circulation, however, has been called in question by Mr. Croll, on the ground that it is mathematically demonstrable that the force to which I attribute the movement is utterly inadequate to sustain it; and he has even gone so far as to affirm that "it is needless to expect that any further observations in reference to currents in the ocean will in the least degree aid Dr. Carpenter's theory; for, supposing it were found that the waters of the ocean do circulate in some such manner as he concludes—*a supposition very improbable*—still we should be obliged to refer the motion of the water to some other cause than to that of differences of temperature" (Nature, Jan. 11, 1872). Now as the question is one of great interest to Physical Geographers and Geologists, and as I have very high authority for regarding the whole of Mr. Croll's reasoning on the subject as fallacious, I shall endeavour to show that the doctrine I advocate involves no inherent improbability, and that the correlation of such a body of observations as it is one of the chief purposes of the Circumnavigation Expedition to collect *may* establish it on a firm basis. At present I claim for it no higher character than that of a "good working hypothesis" to be used as a guide in further inquiry †.

* Geographische Mittheilungen, 1872, p. 70.

† At the Meeting of the Royal Geographical Society, held January 9, 1871, Sir

17. Two separate questions have to be considered, which have not, perhaps, been kept sufficiently distinct either by Mr. Croll or by myself:—*first*, whether there is adequate evidence of the existence of a General *vertical* Oceanic Circulation; and *second*, whether, supposing its existence to be provisionally admitted, a *vera causa* can be found for it in the difference of Temperature between the Oceanic waters of the Polar and the Equatorial Areas. It is obvious that while the admission of the existence of such a Circulation by no means involves the acceptance of the explanation of it which I have offered, no disproof of the validity of that explanation can neutralize the inferential evidence in its favour which is afforded by the facts of observation. But it is also obvious that if a Force can be shown to be in constant operation, which is adequate to produce the effect I assign to it, the inferences based on the facts of observation are greatly strengthened. And I cannot too strongly protest against the statement that *no body of observations can establish a doctrine*, in opposition to an asserted demonstration of its impossibility, the probative value of which entirely rests upon *a single set of observations*, and upon the *relevancy of those observations* to the question under discussion.

18. I am indebted to the Presidential Address of Mr. Prestwich to the Geological Society in 1871 for the following summary of the older doctrine on this subject, which had been generally lost sight of in this country through the prevalence of the erroneous doctrine of the universality of the temperature of 39° in the Deep Sea, to which I have alluded in former Reports (1868, p. 186, 1869, § 123):—"Humboldt states (Fragmens de Géol. et de Climatol. Asiat., 1831) that he showed in 1812 that the low temperature of the tropical seas at great depths could only be owing to currents from the Poles to the Equator.—D'Aubuisson, in 1819, also attributed the low temperature of the sea at great depths at or near the Equator to the flow of currents from the Poles (Traité de Géognosie, p. 450).—Lenz, in 1831, gave the results of some experiments he had made at great depths in the ocean; and concluded that between the Equator and Lat. 45° the temperature decreases regularly to the depth of 6000 feet, when the decrease becomes insensible. The lowest temperature he recorded was 36° Fahr. (Edinb. Journ. of Science, vol. vi. p. 341).—Pouillet briefly discusses Ocean-temperatures, and concludes that, although all the difficulties of the case are not solved, it seems certain that there is generally an upper current carrying the warm Tropical waters towards the Polar seas, and an under-current carrying the cold waters of the Arctic regions from the Poles towards the Equator (Elém. de Phys. 1847, tom. ii. p. 667)."—The doctrine

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Roderick Murchison (speaking through Sir Henry Rawlinson) is reported to have said, with reference to the Paper I read on that occasion:—"From what he had heard of the "Paper on the Law of Oceanic Circulation, it appeared to him that if its conclusions were "borne out by experiment, the announcement would rank, amongst the discoveries in "Physical Geography, on a par with the discovery of the Circulation of the Blood in "Physiology."

was still more explicitly set forth two or three years later by Prof. Buff, in his "Physics of the Earth," as I showed in my First Report (1868, p. 187, *note*); and although his experimental illustration was inappropriate, he very clearly attributed the initiation of the movement to Polar Cold, rather than to Equatorial Heat. It was not to this, as I shall hereafter show (Appendix I., §§ 92, 93), but to the clearly untenable doctrine of Capt. Maury, as to the production of the Gulf-stream and other *sensible currents* by the elevation of level resulting from Equatorial Heat, that Sir John Herschel's trenchant criticism applied; and, as will presently appear, the doctrine advocated in my last Report was explicitly accepted by him shortly before his lamented death (§ 37). So that the general opinion of the most eminent authorities in Physics may be fairly said to have been in favour of the opinion which Mr. Croll dismisses as "a supposition very improbable."

19. The *facts* on which I would lay most stress as justifying the provisional acceptance of this doctrine, may be summarized as follows:—

I. The marked horizontal division of the North Atlantic—the only open Ocean of which the temperature has been yet examined by Serial Soundings—into an *upper* Warm and a *lower* Cold stratum; as shown by the rapid descent of the thermometer in the "stratum of intermixture" between them (§§ 5, 6).

II. The entire absence of any such horizontal stratification in the water of the Mediterranean (§ 7), as also in that of the Red Sea (§ 11) and of the Sulu Sea (§ 12); the temperature of these Inland Basins, beneath the stratum heated by direct insolation, being *uniform* down to their greatest depths.

III. The prevalence of a Temperature averaging 52° at a depth of only 200 fathoms, and of 47° at 400 fathoms, in the Atlantic Ocean between the Tropics (§ 8); where the water, even at these small depths, is *lower* in temperature than at the greatest depths in the Mediterranean.

IV. The prevalence of a Temperature only a few degrees above 32° over the Deep Sea-bed of the North Atlantic generally (Report for 1869, §§ 113–118), as also in the deepest parts of the Indian Ocean, not only to the south (§ 10) but to the north of the Equator (Report for 1869, § 120), and in the China Sea (§ 12); and the deep flow of Polar water, which, as will be shown hereafter (§ 129), underlies the Gulf-stream from the Banks of Newfoundland to the Florida Channel, and there passes beneath the outflowing current *into* the Gulf of Mexico.

V. The prevalence of a Temperature of from 32° to 29½° in the deeper stratum of a portion of the "Lightning Channel" (§ 14); indicating, with the Boreal character of the Fauna of that area, a S.W. underflow of glacial water from the Arctic Sea.

VI. The existence of a Temperature much warmer than the normal of the Latitude, down to a depth of more than 500 fathoms, in another part of the same Channel (§ 13), indicating a N.E. movement of the whole upper stratum in this part of the Atlantic; and the extension of that movement as shown by Thermometric observation, into the Arctic Sea.

20. All these facts (of which Mr. Croll has not even attempted to offer a *rationale*) are brought into harmonious relation by the doctrine of a General *vertical* Oceanic Circulation, from which the Mediterranean and other Inland Seas are excluded (II.) in virtue of the shallowness of their communications with the general Ocean-system. For (I.) the horizontal division of the Atlantic into two strata separated by a marked difference of temperature is thus accounted for, if the upper be conceived as slowly creeping from the Equator towards the Pole, whilst the lower creeps as slowly from the Pole towards the Equator. Next, it fully explains the glacial temperature of the Deep-sea bottom (IV.), which, as the cases of the Mediterranean and the Sulu Sea clearly prove, cannot be attributed to depth *per se*. Again, it gives an adequate reason for the relatively low temperature of the whole mass of Tropical water beneath 200 fathoms depth (III.); this having been drawn into the Tropical area as glacial water, and having risen towards the surface as it received warmth from the crust of the earth on which it rests; the warmer stratum above it being drafted away *towards* the Pole, whilst its place is taken beneath by a fresh arrival of colder water *from* the Pole. Further, it affords an adequate *rationale* for the great body of facts now known (VI.) regarding the abnormally high temperatures met with between Greenland, Iceland, and the coast of Northern Europe; for which the ordinary hypothesis of the extension of the Florida Current or *true* Gulf-stream into that region fails to account, as will be shown in Appendix I. to this Report. The Vertical Circulation is (as it were) epitomized in the "Lightning Channel;" in which there is evidence, from Nautical Observation, of the N.E. movement of the upper warm stratum (VI.); whilst there is inferential evidence of the S.W. movement of the glacial stratum beneath (V.).—Although no experimental evidence has yet been obtained, either of this movement, or of the flow of the deeper stratum of Oceanic water generally from the Polar basins towards the Intertropical zone, indications of such a movement are not wanting. The descent of Icebergs into comparatively low latitudes, in opposition to the current of the Gulf-stream, has been frequently recorded. Two instances of this kind are cited by Dr. Hayes, as having fallen under the notice of Capt. Courtney. On the 27th of April, 1829, he passed an iceberg from 80 to 100 feet high, in N. Lat. 36° 10′ and W. Long. 39°; and on the 17th of August, 1831, he met with another in N. Lat. 36° 20′ and W. Long. 47° 45′; both having been carried further south than the *southern* edge of the Gulf-stream, which they must thus have crossed. —So, again, according to the statement of Mr. Newall, of Gateshead (kindly communicated to me by Sir Henry Rawlinson), a red-painted buoy, which was attached by a wire rope to the end of the Atlantic Cable cut adrift in 1865, in N. Lat. 51° 28′ and W. Long. 38° 42′, was seen seventy-six days afterwards in Lat. 42°, Long. 40°; so that it had travelled *nearly due south* about 600 nautical miles, or about eight nautical miles a day. Though it cannot be affirmed with certainty that this change of place was due to the

action of the southerly underflow upon the long rope hanging to the buoy, yet the coincidence of this fact with the preceding, and of both with what the doctrine in question would predicate, may be fairly regarded as evidence of some value in its favour.

21. I now proceed to the second head of the discussion,—viz. the demonstration, which Mr. Croll considers himself to have given, that the difference of Temperature between Polar and Equatorial water cannot possibly produce the effect I attribute to it. He affirms (1) that the utmost difference which can be supposed to exist between the *levels* of the Polar and the Equatorial columns will not serve to produce a movement of the Equatorial surface-water towards the Poles; and (2) that the utmost difference that can be supposed to exist between the *Specific Gravities* of the Equatorial and Polar columns will not serve to produce a flow of bottom-water from the Polar area towards the Equator.—Assuming the temperature of a column of water at the Pole to be 32° from the surface downwards, and the temperature of the Equatorial column to be 80° at the surface, gradually falling to 32° at 10,000 feet, Mr. Croll calculates that the extreme elevation of level of the Equatorial column above the Polar would be 18 feet. "The distance from the Equator to the Poles is about 6200 miles. "The force impelling the water down this slope of 18 feet in 6200 miles "would therefore be equal to about 1-1,820,000th that of gravity. For "example, the force impelling a cubic foot (64 lbs.) of water at the surface of "the ocean would scarcely be equal to the weight of one fourth of a grain. "But in reality it would not nearly equal this"*. As evidence of the inadequacy of this force to produce motion in Ocean-water, Mr. Croll relies on the experiments of M. Dubuat; who "found that when the inclination "was 1 in 500,000, the motion of the water was barely perceptible; and "he came to the conclusion that when the inclination is reduced to 1 in "1,000,000, all motion ceases. But the inclination afforded by the difference "of temperature between the sea in Equatorial and Polar regions does not "exceed the half of this; and consequently it can have absolutely no effect "whatever in producing currents."

22. Now the experiments of M. Dubuat had reference, not to the *slow restoration of level* produced by the motion of water *on itself*, but to the *sensible movement* of water flowing over *solid surfaces* and retarded by its friction against them; and I have the authority of Mr. Hawksley (whose large experience in the construction of Waterworks enables him to speak with high authority on this point) for the statement, that whilst the latter source of retardation is one with which Hydraulic Engineers are well acquainted, the friction with which the former is attended is so slight that it is altogether disregarded in practice. According to Mr. Croll, if a trough 1,820,000 feet long were partly filled with water, and enough water were then poured into it at one end to raise the surface at that end one foot, with a uniform slope to the other end, *that inclination would be permanent*;

* "On Ocean-currents," in Phil. Mag. Oct. 1870, p. 249.

since "a pressure of one fourth of a grain on the cubic foot of water would "be totally inadequate to overcome the mere molecular resistance of the "water to go into motion." Now not only Mr. Hawksley, but every high Mathematical authority whom I have consulted, has assured me that the "viscosity" of water is so far from having been satisfactorily determined, that the assertion that it would be adequate to prevent water whose level has been disturbed to the extent just mentioned, from ever finding it again, is *totally inadmissible*. The very small degree in which this "viscosity" operates to bring to a stand the *motion of water upon itself*, is shown (as Mr. Hawksley has suggested to me) by the long persistence in the open sea of the "swell" produced by a gale of wind (provided that after the subsidence of the gale there be no counteracting wind), and by the propagation of this "swell" to a great distance*.

23. Now, so far from asserting (as Captain Maury has done) that the trifling difference of level arising from inequality of Temperature is adequate to the production of "ocean-currents," I simply affirm that as fast (or as slowly) as the level is disturbed by change of Temperature, it will be restored by Gravity; and I venture to think that as this is a direct corollary from the fundamental conception of a fluid, the *onus probandi* of the contrary rests on Mr. Croll. When he shall have shown experimentally that water is so viscid a fluid that it does *not* find its own level when time is given to it to do so, he will have obtained a definite foundation for reasoning which is at present simply baseless.

24. But, further, there is an *experimentum crucis* in constant progress, which appears to me completely to disprove Mr. Croll's position. It will not, I presume, be denied by him that the semi-diurnal Tide is produced by the attraction of the Moon, augmented or partly neutralized (as the case may be) by that of the Sun. Now, according to Sir John Herschel†, the Moon's maximum of power to disturb the Earth's waters is only about 1-11,400,000th part of gravity; while that of the Sun does not exceed 1-25,736,400th part of gravity. Yet the Moon's attractive force, even when partly counteracted by that of the Sun (as at neap-tides), suffices to raise and to propagate a vast wave, which, though neither high nor strong in the open Ocean, rises and becomes more rapid where its onward movement is checked, and especially where a large body of water is forced to flow in a narrow channel. Now since the disturbance of Gravity produced by difference of Temperature is many times greater than that produced by the *combined* action of the Sun and Moon, it is for Mr. Croll to show why such a vast force is without effect.

25. Mr. Croll proceeds to say (*loc. cit.* p. 251):—"Suppose that at the "Equator we have to descend 10,000 feet before water equal in density to

* I happen to have myself been subjected for three days continuously, in the middle of the Atlantic, to a heavy swell with a dead calm,—one of the most disagreeable of all nautical experiences.

† 'Outlines of Astronomy,' 3rd Ed., p. 496, note.

"that at the Poles is reached. We have in this case a plane with a slope of "10,000 feet in 6200 miles, forming the upper surface of the water of "maximum density. Now *this slope exercises no influence* in the way of "producing a current, as some seem to suppose; for this is not a case of "disturbed equilibrium, but the reverse. *This slope is the condition of "static equilibrium* when there is a difference between the temperature of "the water at the Equator and the Poles." Taken by itself, this proposition would seem irreconcilable with the simplest principles of Physics; but I presume that it is to be understood in its connexion with what Mr. Croll seems to regard as a permanent inequality of level between Equatorial and Polar waters. For he goes on to say:—"The only slope that has any "tendency to produce motion of the water is the slope formed by the sur"face of the ocean in the Equatorial regions being higher than the surface "at the Poles; but this is a slope of only 18 feet in 6200 miles."—Now if, as I maintain, there is *no* such permanent inequality of *level*—any difference produced by inequality of Temperature always tending towards equalization,—the inclined plane of denser because colder water, of which the slope (as admitted by Mr. Croll) must be nearly *two feet per mile*, constitutes a serious disturbance of equilibrium, that must be capable of producing very decided effects.

26. Mr. Croll's whole manner of treating the subject is so different from that which it appears to me to require, and he has so completely misapprehended my own view of the question, that I feel it requisite to present this in fuller detail, in order that Physicists and Mathematicians, having both sides fully before them, may judge between us: and this must be my apology for dwelling at some length upon considerations so elementary, that I should not otherwise have thought it requisite even to advert to them.—Such, in the first place, is the mode in which Cold applied to the *surface* of an extensive basin of *fresh* water operates in reducing the Temperature of the whole mass. Supposing the cold to be applied to the *entire area* of the basin (as when frost acts on a pond or lake), so that the whole surface-film is chilled at the same time, that film will sink through the subjacent water, carrying downwards its reduced temperature—it may be to the bottom of the basin; and a new film from the warmer layer immediately beneath the surface rises into its place. This, being cooled in its turn, sinks until it meets with a layer as cold as itself; its place on the surface being taken by another film from immediately beneath. Of course, these films do not subside without some intermixture and interchange of temperature with the layers through which they successively descend; but they retain enough of their integrity to produce *a downward convection of Cold*, precisely answering to the *upward convection of Heat* which takes place when Heat is applied to the bottom of a vessel of water. This can be readily shown experimentally, by diffusing either colouring-matter, or solid particles of some substance that will remain in suspension when finely divided, through the surface-film. By the successional descent of the surface-

films, and by their successional renewal, the temperature of the whole mass of the water will be progressively reduced, until its density reaches its maximum at 39°·2; after which the further application of cold no longer causes the descent of the surface-film. And thus it comes to pass that the temperature of all the water beneath that superficial stratum which undergoes further reduction of Temperature with increase of Specific Gravity until it freezes, is uniform throughout, down even to the very bottom of such deep basins as some of the Italian and Swiss lakes; and that, as there is no downward convection of Heat when the summer sun acts on their surface, and the only warming influence will be that of the crust of the Earth beneath, the temperature of 39°·2 is found to prevail throughout the year from a small distance beneath the surface of the deeper lakes down to their very bottom.

27. But the case is very different with *Sea*-water, which does not attain its maximum of density until it freezes (at or below 27°); for the downward convection of Cold will continue until any further reduction of temperature produces the formation of ice; and thus we should expect to find the *deepest* stratum of *Sea*-water that has been acted on by surface-cold showing the *lowest* temperature, which is precisely what we have ourselves met with in the "Lightning Channel" (§ 14), where we found 29°·6 at 640 fathoms, and what Messrs. Payer and Weyprecht met with in Lat. 78° very near the surface (§ 15)*.

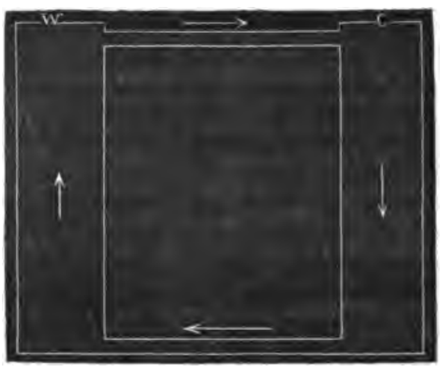
28. Let us next suppose that only *a portion of the area* of the basin is exposed to the action of surface-cold; the movement of the liquid takes place somewhat differently. For the surface-film which descends is then replaced, not from beneath, but by an inflow from the neighbouring area, as may be proved experimentally by the means already indicated†: and

* Mr. Croll (Phil. Mag., Oct. 1871, p. 248, note) speaks of it as "a well-established "fact that in Polar regions the temperature of the sea decreases from the surface down-"wards; and the German Polar Expedition found that the water in very high latitudes "is actually less dense at the surface than at considerable depths, thus proving that the "surface-water could not sink in consequence of its greater density."—Now if, as all recent observations concur to prove (§ 15), the temperature of Polar water shows a progressive reduction, with increase of density, from the surface downwards,—save where the melting of ice has reduced the salinity as well as the temperature of surface-water, thus making it lighter as well as colder,—surely the decrease of temperature indicates that as fast as the surface-water is chilled by the colder air above, it sinks and carries its cold downwards. In what other way the *deepest* water can come to be the *coldest*, I am at a loss to comprehend.

† Mr. Croll objects (*loc. cit.* p. 244) to the Experiment which I described in my last Report (§ 129), on the ground that the mode in which the heat was applied at the opposite end of the trough would establish a circulation by "a horizontal propulsion of "water caused by the expansive force of vapour, and that the movement was not in any "way due to difference of specific gravity."—Now if Mr. Croll will try this experiment for himself, he will find that the circulation is clearly *initiated* at the *cold* end of the trough; the coloured water, as it comes in contact with the ice, *tumbling down* (as it were) to the bottom of it, and then flowing horizontally without any application of heat to the other end of the trough. And this movement goes on, though at a progressively

when this chilled surface-film has reached the bottom, instead of limiting itself to the area of superficial exposure to cold, it flows off laterally in every direction; and this lateral outflow will go on so long as the disturbance of equilibrium is maintained by the cooling of the surface-films which are successively drawn into the area.

29. That such *must* be the case, appears to me so self-evident that I am surprised that any person conversant with the principles of Physical Science should hesitate in admitting it, still more that he should explicitly deny it. But since others may feel the same difficulty as Mr. Croll, it may be worth while for me to present the case in a form of yet more elementary simplicity.—Let us suppose two cylindrical vessels of equal size to be filled with Sea-water to the same level, but the temperature of the water in one of them (W) to be 60°, while that of the other (C) is 30°; it is obvious that the pressure exerted by the latter column upon the bottom and sides of the cylinder it occupies must be greater than that exerted by the former, in proportion to its excess of Specific Gravity. If, now, communications be opened between the two by transverse pipes at the top and bottom of each cylinder, an outflow of cold water will take place by the lower pipe from C to W, and an inflow of warm water by the upper pipe from W to C, until



equilibrium is restored; which it will be by the transfer of *the lower half* of the *cold* column from C to W, and of the *upper half* of the *warm* column from W to C. But if the water which flows through the upper pipe from W to C be subjected, on entering the top of C, to a surface-cold which

diminishing rate, provided the action of Cold is continuously maintained; and would thus continue until the whole of the water is reduced to the temperature of its greatest density, when it would come to an end by the equalization of specific gravity throughout. The application of Heat at the other end of the trough only serves to *maintain* the circulation, by keeping up the *difference* of temperature which is necessary for the continued disturbance of the equilibrium which the circulation is always tending to restore. The *horizontal* flow of the chilled water along the *bottom* of the trough is clearly due to the greater *downward* and therefore *lateral* pressure of the column at the cold end, resulting from its constantly renewed excess of density.

reduces its Temperature to 30°, and proportionally augments its Specific Gravity, then (the levels of the two columns being equalized by such inflow) the excess of weight in column C will be maintained, until, by the continued outflow through the bottom pipe, the whole of the warm water in column W has been transferred to C; for this outflow lowers the level in C, and at the same time tends to lift up the superincumbent warm water in W, which will consequently flow back through the upper pipe into C, so as to restore the equality of the levels of the two columns. And if *all* the water which passes from W to C is cooled down on entering C, this circulation must continue, though at a gradually diminishing rate, until equilibrium is restored by the depression of the temperature of the entire water of *both* columns to 30°.—But let us suppose, further, that the cold water, as it enters the bottom of W by the lower pipe, has its temperature *raised* again by warmth applied beneath; and that this elevation is further increased by surface-heat as the water rises to the top of the column; the result will be that, as the temperature of the water in column W, except of that lowest stratum which has last arrived from C, will be kept constantly above that of column C, whilst there will be a constant tendency to the equalization of the levels of the two columns, there will be a continually renewed difference between their Specific Gravities and consequently between their absolute weights. This will produce a continual transference of water from the *bottom* of C to the bottom of W, and from the *top* of W to the top of C, with a constant *descending* movement in C and a constant *ascending* movement in W. The descending movement in column C will not consist in a successional descent of surface-films from above downwards, unless it should happen that the surface-cold is intense enough to reduce their temperature below that of the subjacent water; but it will be *a downward movement of the entire mass*, as if water in a tall jar were being drawn off through an orifice at the bottom. And in like manner the upward movement in column W will not so much consist in a successional ascent of bottom-films, as in *an upward movement of the entire mass*; since the films warmed at the bottom will only ascend until they meet with water of which the temperature is as high as their own; and this will be the case so soon as they have passed through the cold stratum which has last flowed in from C. The force which will thus lift up the entire column of water in W, is that which causes the descent of the entire column in C, namely, the excess of Gravity constantly acting in C; the levels of the two columns, and consequently their heights, being maintained at a constant equality by the free passage of surface-water from W to C.

30. The whole of Mr. Croll's discussion of this question, however, proceeds upon the assumption that the levels of the Polar and Equatorial columns are *not* kept at an equality, in consequence of the inadequacy of the excess of level in the Equatorial column to put in motion the intervening water; and that instead of there being an *excess of weight* in the Polar column, there is a condition of *static equilibrium* between the two.—

Having shown the untenableness of this assumption (§ 22), I might dismiss his arguments without further notice, were it not that I should leave it in his power to reply that, granting *my* view of the case to be the correct one, the force of Gravity which I regard as the *primum mobile* of the Circulation cannot be greater than it is on *his*.

31. In order to show how a disturbance of Static Equilibrium produced by difference of Temperature would operate through a vast extent of Ocean, I shall suppose an elongated Inland Sea like the Mediterranean, with a uniform temperature of 54° throughout, to have one-tenth of its length cut off from the rest by a septum, and the whole surface of this portion to be acted on by Polar Cold, until the temperature of the entire body of water it contains should be reduced to 27°; its level being thus lowered, and its Specific Gravity augmented, without any increase in its absolute weight. If, now, we suppose the septum to be removed, it is evident that not only will the depression of level produce a surface-inflow into the Cold area, from the portion of the basin next adjacent to it, but that the compensatory movement *must* extend, though in a gradually diminishing ratio, to the opposite extremity of the basin: for, if it do not, either water is *not* a liquid, or liquids do *not* find their own level. The water of the Cold area, having its surface now raised to the general level of the basin, will have its absolute weight augmented in proportion to its increase in density; and both its downward and its lateral pressure will therefore be in excess of that of every other column of equal height and base in the entire basin. Now Mr. Croll's position is, that the difference in Gravity between the two columns A and Z at the opposite ends of the basin is a force too small to give motion to the whole intermediate series of columns (B to Y); which is equivalent to saying that the heavier water in column A will be *permanently banked up* by the resistance of columns B to Z; so that the state of disturbed Equilibrium produced by the original reduction of Temperature will last so long as that reduction is maintained.

32. Now as such a position is inconsistent, not merely with the *theoretical* conception of a liquid, but with *facts* capable of being verified by daily observation, there must obviously be a fallacy somewhere: and this fallacy seems to me to lie in Mr. Croll's treatment of the whole mass of water *en bloc*, as if it were a solid body; instead of regarding it as a liquid, of which each component part, being free to move upon every other (*retarded* only by its "viscosity"), carries from point to point along the whole length of the basin the action of the force *initiated* at the cold end. In order to determine how this force will operate, we must begin by considering what change will occur between the water of the cold and heavy column (A), and that of the warmer and lighter column (B) next adjacent to it, when free communication has been opened between them. The difference in the weight of the two columns involves an excess of lateral pressure in column A, increasing from above downwards; and this will cause a *bottom outflow* of the heavier stratum from A to B, which, by lowering the level

of A and raising that of B, will produce a further *surface-indraught* from B into A. Supposing B to be cut off by a septum from the rest of the basin, as A was in the first instance, this interchange would proceed until half of the heavier water of column A has flowed into the lower part of column B, and half of the lighter water of column B has flowed into the upper part of column A, so as to equalize the pressure and the level in the two. If now the septum be removed between columns B and C, the excess of pressure which B shares with A will operate in like manner (though in a diminished measure) against C; and the same interchange will take place until the equilibrium is restored. And thus by a propagation of the like interchange through the whole succession of columns in a progressively diminishing ratio, the stratum of colder and heavier water (supposing its temperature not to be altered by conduction from below or from above) would ultimately spread itself at a uniform level over the entire floor of the basin. This extension, in fact, would be simply a question of *time*, like the equalization in the level of its surface; the rate of the movement being determined by the "viscosity" of the water.

33. But we have now to trace out the result of the *continued* action of severe Cold on the surface of the limited area at one extremity of the basin. As fast as the *bottom-outflow* tends to restore the equilibrium between the Polar column and the rest of the basin, a new disturbance will take place by the cooling of the water brought in by the *surface-inflow*; and this disturbance will be propagated onwards from column to column, along the whole length of the basin, in the manner just indicated. If the glacial stratum were not to acquire any heat in its diffusion over the floor of the basin, its thickness would be continually augmented by fresh exchanges between column and column,—the warmer *surface*-water flowing *towards* the cold extremity of the basin, as fast as cold *bottom*-water flows from it; until at last the *entire mass* of the water in the basin (supposing its surface-temperature not to be kept up by insolation) would have its temperature progressively reduced by *downward* convection in the area over which the cold directly acts, and thence by *lateral* convection, until the whole would be brought down to 27°, when no further movement would take place.

34. But since the Glacial stratum, as it creeps along the sea-bed, is continually receiving heat from the crust of the earth beneath and from the warmer layer of water above, its thickness would diminish with the increase of distance from the Polar area; or, in other words, its surface would form an inclined plane, lying at a greater and yet greater depth, the further it has flowed from its source,—which is just what, so far as our present knowledge extends, proves to be the case. And further, as the upper stratum is being continually draughted off in the opposite direction, the portion of the glacial stratum which has had its temperature raised by the agencies just noted, and which overlies the rest, will be lifted into its place by the intrusion of a fresh arrival of colder water beneath, so as to

come under the direct influence of insolation; whereby its temperature will be greatly augmented, and its level raised, so as to promote the return-flow. Thus at the two extremities of the basin there will be two forces in constant renewal, the opposing actions of which will as constantly tend to a *disturbance* of equilibrium; and to deny that a movement will thus be produced and sustained, tending to the *restoration* of equilibrium through the entire basin, is simply to assert that a constant force acting on particles that are free to move does not move them.

35. It is to be remembered that, however small the original amount of movement may be, a *momentum* tending to its continuance *must* be generated from the instant of its commencement; so that if the initiating force be in constant action, there will be a *progressive acceleration* of its rate, until the increase of resistance equalizes the tendency to further acceleration. Now if it be admitted that the propagation of the disturbance of equilibrium from one column to another is simply *retarded*, not prevented, by the "viscosity" of the liquid, I cannot see how the conclusion can be resisted, that the constantly maintained difference of Gravity between the Polar and Equatorial columns really acts as a *vis viva* in maintaining a Circulation between them. A remarkable confirmation of my argument as to the sufficiency of difference in Specific Gravity to keep up a Vertical Circulation, is afforded by recent researches on the Dardanelles Under-current. (See § 173.)

36. Those who are accustomed to look only at the movements of *great volumes* of fluid, and to discuss them *en masse*, are apt to ignore facts perfectly well known to such as have had occasion to observe those minuter changes which are constantly taking place within any collection of liquid under the influence of slight alterations of Temperature.—The following is a remarkable example of this kind, which fell within my own knowledge many years ago, and made a strong impression upon me. Mr. West, a very ingenious mechanician at Bristol, having heard of the success of Mr. Peter Barlow's plan of constructing object-glasses for Telescopes of moderate aperture, in which the double concave of flint-glass was replaced by a highly refracting fluid (such as sulphuret of carbon, or oil of cassia), carried out this plan on a large scale in the construction of an object-glass of eighteen inches in diameter. By the mathematical aid of Mr. Barlow, and his own great mechanical ability, this object-glass (exceeding in diameter that of any refracting telescope which had been at that time constructed) was completed with theoretical correctness; but when it was brought into use, its performance was found to be so seriously impaired by movements produced in the fluid by the very slight disturbances in the equality of the Temperature of its different parts, occasioned by atmospheric currents, that in spite of every precaution which could be taken for its protection, it was found to be valueless for the purpose of Astronomical research.—Those, again, who have been accustomed to the minute observation of Aquaria, have frequently noticed currents, marked by the movement of minute suspended particles,

that could only be attributed to slight differences of Temperature; and Dr. Möbius*, who has given much attention to this inquiry, has ascertained that a difference of *half a degree* of Reaumur is quite sufficient to produce *sensible movement.*—Further, Mr. Rainey, who has paid great attention to the internal motions of small collections of liquid entirely enclosed within glass and placed under the Microscope, has recently published a series of observations, which show that definite currents, made obvious by the translation of suspended particles, take place in liquids thus enclosed, under conditions that seem to exclude any other agencies than inappreciable differences of Temperature. (St. Thomas's Hospital Reports, New Series, vols. i. & ii.)

37. I have not, indeed, learned that any eminent Physicist endorses Mr. Croll's objections to the doctrine I advocate; whilst I have, on the other hand, had the satisfaction of receiving from both Sir John Herschel and Sir William Thomson an explicit acceptance of it. The former was kind enough to write to me, only a short time before his last illness, as follows:—"Assuredly, after well considering all you say, as well as the com-"mon sense of the matter, and the experience of our hot water circulation-"pipes in our greenhouses, &c., there is no refusing to admit that an Oceanic "circulation of some sort must arise from mere Heat, Cold, and Evaporation, "as *veræ causæ*; and you have brought forward with singular emphasis the "more powerful action of Polar Cold,—or rather, the more *intense* action, "as its maximum effect is limited to a much smaller area than that of the "maximum of Equatorial Heat. The action of the Trade and Counter-trade "Winds in like manner cannot be ignored; and henceforward the question "of Ocean-currents will have to be considered under a twofold point of view. "The Wind-currents, however, are of easier investigation. All the causes lie "on the surface: none of the agencies escape our notice; the configuration "of coasts, which mainly determines their direction, is patent to sight. It is "otherwise with the other class of movements: they take place in the depths "of the Ocean; and their movements, and directions, and channels of concen-"tration, are limited by the configuration of the sea-bottom, which has to be "studied over its whole extent by the very imperfect method of Sounding."

38. Imperfect as that method is, however, I cannot but believe that a sufficient number of *Serial* Temperature-soundings, taken in different parts of the Oceanic area, especially in the Southern Hemisphere, will afford adequate data for the settlement of the question whether such a Circulation actually exists; and also that means may be found for determining its rate by observation and experiment. If such determinations can be made, I am assured by Mathematicians that they will afford valuable data for the accurate determination of the "viscosity" of water, which is at present by no means the *known* quantity assumed by Mr. Croll. It is consequently with great satisfaction that I look forward to the results of the inquiries to be prosecuted in this direction by the Circumnavigation Expedition now being fitted out by Her Majesty's Government.

* See 'Annals of Natural History,' Ser. 4, vol. viii. p. 201.

39. If the views I have propounded be correct, it may be expected that near the border of the great Antarctic Ice-barrier, a Temperature below 30° will be met with (as it has been by Parry, Martins, and Weyprecht near Spitzbergen, § 15) at no great depth beneath the surface; and that instead of *rising* at still greater depths, the thermometer will *fall* to nearly the freezing-point of salt water. The greater the distance from the ice-barrier, the greater would be the depth at which the surface of the Glacial stratum would be met with; but in consequence of the free communication between the Antarctic area and the great Southern Oceans, it may be expected that the deepest parts of these will be found to show a temperature as low as 32°, or perhaps lower, even near the Equator.—The bottom-temperature of the North Pacific will afford a crucial test of the truth of the doctrine. For since the sole communication of this vast Oceanic area with the Arctic basin is a Strait so shallow as only to permit an inflow of *warm* surface-water, its deep cold stratum must be entirely derived from the *Antarctic* area; and if its bottom-temperature is not actually higher than that of the South Pacific, the glacial stratum ought to be found at a greater depth *north* of the Equator than *south* of it. In the North Atlantic, again, the comparative limitation of communication with the Arctic area may be expected to prevent its bottom-temperature from being reduced as low as that of the Southern Atlantic. But it will be a matter of peculiar interest to determine the bottom-temperature of the deep channel that separates Greenland and Iceland, to ascertain the thickness of the Glacial stratum which (I venture to predict) will be found to occupy a large proportion of its entire depth, and to trace the gradual thinning-out of this, as it diffuses itself over the vast area of which the bottom-temperature is reduced by it.

40. It will further be extremely important to ascertain by Mechanical means, if possible, whether this Glacial flow has a movement of which the direction and rate are determinable. And I would especially point out that the "Lightning Channel" (§ 14) affords a peculiarly favourable opportunity for such investigation, in consequence of the well-marked distinctness of its two strata. If my view be correct, a "current-drag" suspended in the *upper* stratum ought to have a perceptible movement in the N.E. direction; whilst another, suspended in the *lower* stratum, should move S.W. And though the *rate* of movement in each may be very slow, yet the opposition of their *directions* may be expected soon to make itself apparent, in the separation of the surface-buoys from which the drags are suspended.—Should the examination of this Northern portion of the Oceanic area be beyond the scope of the Circumnavigation Expedition, I would urge that it should be undertaken by any Polar Expedition which the British Government may fit out. For the difference in the Thermal conditions of the Northern and Southern Oceans, consequent upon the difference in their respective relations to the Arctic and Antarctic basins, is a question of peculiar interest in a Geological point of view.

41. If, again, as may be anticipated, it should prove that a considerable

difference exists between the bottom-temperature of the North Pacific and that of the North Atlantic, it is evident that if there ever was a time at which a continuous Continent extended from Greenland to Scandinavia, the North Atlantic would then have been cut off from the Polar basin almost as completely as the North Pacific is now, and its bottom-temperature would be higher than we now find it. If, on the other hand, there should ever be such a subsidence of the Peninsulas of Alaska and Eastern Siberia, as would open a free communication between the Arctic basin and the North Pacific, the temperature of the lower stratum of the latter would probably be greatly reduced.

42. How important would be the Geological and Biological effect of such changes, and how great a light may be thrown on Palæontology by such inquiries as I have indicated, was admirably shown by Mr. Prestwich in the Presidential Address which he last year (1871) delivered to the Geological Society. After pointing out how entirely the Thermal condition of the Mediterranean differs from that of the Atlantic, in consequence of its exclusion from the general Oceanic circulation, he suggests whether the deep Sea in which the Chalk of Europe, with its more Tropical genera, was deposited may not also have been a sea shut out from direct communication with Arctic seas. As the Cretaceous Ocean formed an east and west belt across the present Continents of Europe and Asia, we must look for "dry "land on the confines of that ocean; and it is probable that the latter may "have been, to the north, in the direction between Greenland and Scotland "and Scandinavia, where the present ocean is some hundreds of fathoms "shallower than further south. We know that towards the end of the "Cretaceous period a change took place in the Fauna, arising apparently "from the shallowing of the sea that preceded the deposition of the Maes-"tricht beds. Many of the great Cephalopods disappeared, and Reptiles "increased in numbers; at the same time the Lamellibranchiate Mol-"lusca became more predominant." . . . . "If such a northern land "barrier as that which I have alluded to existed at the period of the Chalk, "and that barrier was submerged during the early part of the Tertiary "period, it would (taken in connexion with the very different conditions "of depth under which the Chalk and Lower Tertiaries were formed) go far "to account for the great break in the Fauna of the two periods. Some years "since I had occasion to show on other grounds that the Thanet Sands, "which repose on the Chalk in the South-east of England, exhibited a "Fauna essentially of temperate or cold latitudes; and I inferred the inset "of currents from the north."

43. It is regarded by Mr. Prestwich as highly probable that the old Cretaceous ocean was prolonged into the American Cretaceous area, across the present Atlantic; and he thinks "that the hypothesis with regard to "the continuity of that sea-bed from the period of the Chalk to the present "period is one of high probability." Now this *continuity* is all that it was intended by Prof. Wyville Thomson and myself to express by the phrase

used in my First Report (1868, p. 193), that "we may be said to be still living in the Cretaceous epoch." We then anticipated—what our own subsequent inquiries and those of our friends of the United States Coast Survey have abundantly confirmed—that with the persistence of the Globigerine life which *forms* the Cretaceous deposit, there would prove to be a like persistence of numerous types characteristic of the old Cretaceous Fauna; so that the *Globigerina*-mud now in process of increase on the sea-bed of the Atlantic may be regarded "not merely as *a* Chalk-formation, but as a continuation of *the* Chalk-formation." Now we have been told on high authority that Geological epochs are essentially marked out by great changes in the Fauna,—whole series of Animal forms disappearing, and their places being taken by others which then first make their appearance; and further, that the termination of the Cretaceous Epoch must be considered, in this sense, as coinciding with the disappearance of those numerous types of chambered Cephalopods which were so eminently characteristic of the Old Chalk, and of which very few appear to have survived the changes that inaugurated the Tertiary epoch*.

44. There is nothing really inconsistent, however, between Sir Charles Lyell's view of the case and our own. If his definition of a Geological Epoch be accepted, then our doctrine that "we may be said to be still living in the Cretaceous epoch" requires to be expressed in somewhat different language. But if that which we meant to be understood by it, and which has been admitted as probable by so eminent an authority in Geology as Mr. Prestwich, should be really the case, then, I submit, some new term must be invented to designate that state of things which will present itself to the "Geologist of the future," whenever the present bed of the Atlantic shall be raised into dry land. For there will then be found superimposed upon the newest beds of what is now known as the "Cretaceous Formation," not a series of deposits resembling the Tertiaries of Europe and America, but an unbroken succession of layers of a substance resembling the Old Chalk in all essential particulars†, and containing numerous Animal types which do not differ more from those of its uppermost beds than these do from the types found in the earlier members of the Formation. Doubtless there will be a great and perhaps a sudden change in certain portions of the Fauna; but whilst the material of the deposit continues unchanged, whilst its Stratification remains conformable, and a large number of Generic types can be traced continuously throughout, I venture to think that the

* Sir Charles Lyell's 'Student's Elements of Geology,' p. 263.

† Much stress has been laid on the fact that the specimens of the Atlantic mud brought up by our dredge proved on analysis to contain a considerable admixture of Sand, and could not be said to be true Chalk. But this admixture may very probably have been purely *local*, resulting from the drift of the Northern *detritus* along a line of specially rapid underflow. According to Dr. Bailey and Prof. Huxley, the Atlantic mud brought up by the Sounding-apparatus from the Mid-ocean scarcely contained a trace of sand; and the non-communication of the Cretaceous Sea with the Polar area may not improbably be the explanation of the purity of the Old Chalk.

"Geologist of the future" would scarcely think himself justified in limiting the "Cretaceous Formation" to beds distinguished only by the presence of a certain set of Animal forms not to be found in those above them. If, however, he should think proper so to limit that term, he would have to invent a new one for the later beds deposited in absolute continuity with it, and perhaps in all but one particular under the same conditions. That particular, if Mr. Prestwich's very ingenious speculation be accepted as provisionally true, was Temperature; a reduction in that of the Cretaceous Sea, consequent upon the opening of a communication with the Arctic basin (by changes in level perhaps thousands of miles off), being an adequate *vera causa* for the disappearance of all those animals which are dependent on a warmth approaching the Tropical; whilst those which could adapt themselves to the change (perhaps with some modification of structure) would maintain their ground, and would in their turn leave their remains to be entombed in the mass of *Globigerina*-mud, the production of which on the Atlantic sea-bed seems to have gone on uninterruptedly through the whole of the Tertiary Period to the present time.

45. I have thought it not inappropriate to conclude this portion of my present Report with a recurrence to a subject brought into prominence by Prof. Wyville Thomson and myself three years ago; partly because I thus obtain an opportunity of explaining what was really meant by a somewhat incautious expression, and partly because a new value is given to the doctrine which that expression was intended to convey by Mr. Prestwich's explicit acceptance of it, coupled with his ingenious application of the doctrine of a General Oceanic Circulation to account for the disappearance of the more Tropical portion of the Cretaceous Fauna, without change in any other condition than Sea Temperature.—This, as it appears to me, is a singularly felicitous example of the valuable results which may be expected from the wider extension and more systematic prosecution of the inquiries in which it has been my privilege to take part, and which I now leave in the hands of the valued Colleague by whom they were originally suggested; entertaining the fullest confidence that they will be carried on with zeal and ability, and a sanguine hope of their distinguished success.

Part II.

Further Investigation of the Gibraltar Currents.

46. The inquiries made in the 'Porcupine' Expedition of 1870 having shown that the information obtainable by the Hydrometer respecting the *stratification* of Atlantic and Mediterranean water in the Strait of Gibraltar might afford valuable evidence in regard to their movement, I made the determination of Specific Gravities my own special charge, whilst Capt. Nares, in consultation with me, carried out the Mechanical portion of the investigation. As his separate Report of the inquiries he conducted, and of the deductions he drew from them, has been already communicated to

the Royal Society by the Hydrographer to the Admiralty, and has been published in its 'Proceedings' (Jan. 18, 1872), there will be no occasion for me to do more than state the results of my own Sp. Gr. determinations, and compare these with the results obtained by the use of the "current-drag," in such a manner as to enable every reader to judge for himself whether my own conclusions are or are not justified by the facts now adduced. —That the *complete* elucidation of the question requires a much more prolonged and systematic study than it was in our power to make on this occasion, is the opinion at which we have both arrived; and since the knowledge to be gained by such an investigation would be highly serviceable to the Navigator, whilst affording most valuable data for the Scientific study of Tidal and Current movements generally, I shall include in this Section of my Report some suggestions for its further prosecution.

47. It will be remembered that between Capes Trafalgar and Spartel, at the western or Atlantic entrance of the Strait, is a "ridge" which constitutes a kind of marine "watershed," looking on one side towards the Atlantic, and on the other towards the Mediterranean. The depth of the northern half of the channel across this section scarcely anywhere exceeds 50 fathoms; whilst in its southern half the depth does not seem anywhere to reach 200, and may be considered to average 150 fathoms. From this "ridge" the Atlantic slope deepens gradually westwards, until, at a distance of about 45 miles, a depth of from 500 to 600 fathoms is reached. On the other hand, the Mediterranean slope deepens gradually eastwards along the whole length of the Strait (about 35 miles), as far as its *embouchure* in the Mediterranean between Gibraltar and Ceuta, where the depth of the deepest part of the channel exceeds 500 fathoms. Thus it appears that the Strait is to be considered as a prolongation of the Mediterranean basin, not of that of the Atlantic. If its bottom were to be elevated 200 fathoms, the "ridge" would become dry land, entirely cutting off the Mediterranean from the Atlantic; but though the channel between the European and the African shores would be considerably narrowed, it would still extend further west than Tangier. If thus completely cut off from the Atlantic, the Strait would be in every sense a part of the Mediterranean, and would be entirely filled with the denser water of that great Inland Sea. But in virtue of its communication with the Ocean outside, and of the continual inflow (modified by tidal changes) of a surface-current from the Atlantic, the whole *upper* stratum of the water of the Strait has a purely Atlantic character, which is as distinctly recognizable by the Specific-Gravity test at the Mediterranean as at the Atlantic end of the channel. On the other hand, the *lower* stratum was last year found no less distinctly to correspond in Specific Gravity with the denser water of the Mediterranean; so that its presence could be recognized by this character no less certainly on the summit of the "ridge" than in the deepest portion of the Mediterranean embouchure. (See Report for 1870, §§ 61–69.)

48. I made it, therefore, my first object to ascertain whether the pre-

sence of Mediterranean water could be detected by an excess in the Specific Gravity of the bottom-water, on the *Atlantic* side of the slope; our previous inquiries having shown that no such excess is to be found in the ordinary water of the Atlantic, which appears to be even a little *more* saline *at* the surface than below it,—the effect of this excess on Specific Gravity being neutralized by the expansion produced by the more elevated Temperature (Report for 1870, § 84). The average of our determinations of the Sp. Gr. of Atlantic water had been 1·0265, the *minimum* (bottom) being 1·0261, and the *maximum* (surface) 1·0269; and this agrees very well with the results obtained by Forchhammer,—who, moreover, explicitly states that while the water taken between Lat. 20° and 30° off the western coast of Africa has a Salinity approaching that of Mediterranean water (which he attributes to the great evaporation, and to the absence of river-return of fresh water), the surface-water of the Atlantic embouchure of the Straits, for some distance westward, has the average density of Atlantic water*.

49. The first sample we obtained of bottom Atlantic water within the influence of the Mediterranean outflow, was by the deep Sounding already mentioned (§ 3) as having been taken on the 19th of August, in Lat. 36° 47′ N., and Long. 9° 39′ W., about 45 miles W.S.W. of Cape St. Vincent. The Sp. Gr. of the *surface*-water being here 1·0268, that of the *bottom*-water was 1·0281.—Our next sample was obtained on the 20th of August from a depth of 665 fathoms in Lat. 36° 2′ N. and Long. 7° 43′ W., in the direct line of the axis of the Strait, and about 90 miles to the west of the "ridge." The Sp. Gr. of the *surface*-water was here 1·0269, and that of the *bottom*-water 1·0280. Proceeding eastwards in the direction of Cape Spartel, we took on the following day a succession of samples from the gradually shallowing bottom of the Atlantic slope; and the Specific Gravities of these, as compared with those of the surface-water at the same points, are given (with the preceding) in the following Table:—

Station.	North Latitude.	West Longitude.	Depth in Fathoms.	Sp. Gr. at Surface.	Sp. Gr. at Bottom.
1.	36° 47′	9° 39′	1560	1·0268	1·0281
2.	36° 2′	7° 43′	665	1·0269	1·0280
3.	35° 47½′	6° 41′	355	1·0269	1·0282
4.	35° 43′	6° 34′	290	1·0270	1·0281
5.	35° 40′	6° 28′	225	1·0270	1·0282
6.	35° 38′	6° 25′	115	1·0270	1·0278
7.	35° 47′	6° 21′	325	1·0270	1·0285

Thus it appears that along the whole of this line of Soundings there is a most decided excess in the Specific Gravity of the *bottom*-water over that of the *surface*-water, and that this excess increases as the "ridge" is approached,—the exception shown at Station 6 being really a proof of the rule; for at that Station there was obviously a rise of the bottom from a

* Phil. Trans., 1865, pp. 220, 223.

deep valley, since we passed from 115 to 325 fathoms by merely shifting our position about 7 miles to the northward; and the heavier water would of course gravitate towards the greater depth, where (Station 7) we found the *maximum* of density anywhere met with on the *Atlantic* slope of the "ridge."

50. Taking these facts in connexion with those to be next stated in regard to the Specific Gravity of the water at different depths within the Strait (*i. e.* on the Mediterranean slope of the "ridge") and on the "ridge" itself, I cannot but regard it as a fair conclusion that this excess is due to the *deep outflow of Mediterranean water* over that ridge. It is not a little significant that the effect of this outflow should be so distinctly traceable as far as Station 1,—a fact which seems to me to negative the hypothesis of Captain Spratt, that this dense bottom-water is derived from the remote region of the Atlantic to the south of 30° Lat.: for (1) the excess of evaporation in that area must be effectually neutralized, in the area between Cape St. Vincent and Mogador, by the vast quantity of fresh water brought down by the Guadiana, the Guadalquiver, and the rivers of Morocco; whilst (2) all our knowledge of the movement of Oceanic water along the N.W. coast of Africa (§ 63) indicates that it has a *southward* rather than a northward direction. Further, the Temperature observations made both last year (Report, §§ 75, 76) and this year (§ 62), indicate that any *surface-outflow* of Mediterranean water is more likely to take place along the European than along the African side.

51. Proceeding within the Strait, we took up on August 21st our first position off Tarifa, where a sample of water taken from the *bottom* at 330 fathoms gave the high Specific Gravity of 1·0293; clearly showing its Mediterranean character. Samples taken at depths of 200 and 150 fathoms gave almost exactly the same Sp. Gr., as did also a second pair of samples taken in nearly the same part of the Strait at 150 fathoms and 125 fathoms; from a comparison of which observations with those taken between Gibraltar and Ceuta (§ 54), it appears that the dense Mediterranean water lies about 100 fathoms nearer the surface over a 300 fathoms bottom, than it does where the bottom sinks to more than 500 fathoms.—It was not a little unexpected to find a marked excess in the density of the *surface*-water in the first set of observations, its Sp. Gr. being as high as 1·0275. This seemed attributable to the prevalence of a fresh Easterly wind, producing a westward drift of Mediterranean water that mingled with the surface inflow from the Atlantic; it must, however, have been local and transient, since at a later part of the day, in nearly the same spot, the Sp. Gr. of the surface-water was found to have returned to its ordinary standard (within the Strait) of 1·0271.—The observations made this day with the Current-drag, although not so continuous or complete as those taken subsequently, made it clear that both the upper and the under-current are not only greatly influenced in *rate*, but may be completely reversed in *direction*, by Tidal agency. One hour after High water, the

surface-stratum was found to be moving *eastwards* at the rate of 1·7 mile an hour; 3½ hours later, the direction was still the same, but the rate had diminished to 1·5 mile an hour; 2 hours after Low water, when the tide was rising, the surface-current was moving to the *westward* at the rate of 1 mile per hour; and at 4–5 hours after Low water, its direction being still the same, its rate of movement had fallen to 0·4 mile per hour. In the earlier part of the day, the *under*-current at 150 fathoms was found to be moving to the *eastward*, but an accident prevented the determination of its rate. In the afternoon, however, 2 hours after Low water, a current-drag suspended at a depth of 125 fathoms moved *westward* at a rate of 1·35 mile an hour; which, allowance being made for the resistance to the motion of the surface-buoy, was considered by Capt. Nares to indicate that the rate of westward flow of the under-current was really 1·5 mile per hour, exceeding that of the surface-movement in the same direction by 0·5 mile.

52. On the following day we took up our position on the north part of the "ridge" between Capes Spartel and Trafalgar, over a bottom of 125 fathoms depth. The weather being very calm, and the water smooth, every thing was favourable to the prosecution of our inquiries. The Specific Gravity of the *surface*-water being 1·0271, and that of the *bottom*-water 1029·2, the presumption was very strong that, while the *upper* stratum consisted of Atlantic water, the *lower* stratum consisted of Mediterranean water; and the first question to be determined was the depth of each. We found that at 50 fathoms the Sp. Gr. had increased to 1·0273, and that at 100 fathoms the Sp. Gr. showed a further increase to 1·0276; but that a marked increase from this to 1·0290 showed itself before the depth of 110 fathoms was reached; so that while the lower part of the Atlantic stratum showed a sufficient admixture of Mediterranean water to affect very sensibly its Specific Gravity, the stratum below 110 fathoms might be considered as consisting essentially of Mediterranean water.

53. The experiments with the Current-drag were commenced at 2½ hours after High water, when the ebb-tide was running eastwards. The current on the surface was running inwards at the rate of 1·25 mile per hour; and this was also found to be its rate at 25 fathoms depth. When the drag, suspended from a small boat, was lowered to 100 fathoms, it still moved to the *east*ward, but at a slower rate; the surface-current passing the boat at the (estimated) rate of 0·25 mile per hour. The drag being then lowered to 119 fathoms, and suspended from a buoy, it first moved *east*wards at the rate of 0·18 mile per hour; and then at Low water, while the *surface*-current was still moving *east*, it began to move *west*wards, retarded at first by the action of the surface-current on the floating buoy. At 1 hour after Low water, the surface-current having stopped running, the current-drag had drifted 0·18 mile to the west; but as it was found then to have grounded in 118 fathoms, its line was shortened to 108 fathoms. It immediately ran away with its buoy to the *west*ward, drag-

ging it fast through the surface-water, which was itself setting in the same direction, against a light west wind; and its rate, at 2½ hours after low water, was estimated at 1·2 mile per hour, that of the surface being estimated at 0·5 mile per hour.—Thus it appeared that in this situation both the upper (Atlantic) current and the lower (Mediterranean) current followed the *direction* of the Tidal movement, and that their *rates* were greatly affected by it; but a comparison of the *relative amounts* of the flow in each case showed a decided excess in the movement of the *upper* current to the *east*ward, and a no less decided excess in the movement of the *under*-current to the *west*ward. For taking 2½ hours after each turn of the tide as the time of greatest velocity of each current, the comparison of estimated rates (§ 60) stands thus:—

	2½ hours after L. W.	2½ hours after H. W.
Surface-current	W.⟵ 0·5 mile per hour.	⟶E. 1·25 mile per hour.
Under-current	W.⟵ 1·2 mile per hour.	⟶E. 0·88 mile per hour.

Thus the excess of *in*flow of the *upper* current being at the rate of 0·75 mile per hour, that of *out*flow of the *under*-current was 0·32 mile per hour.

54. Having then proceeded to Gibraltar, our first day's work (Aug. 26) at the Mediterranean end of the Strait was chiefly devoted to experiments with Mr. Siemens's Photometric apparatus; the results of which, as they were not altogether satisfactory, I think it better not to record. But the opportunity was taken to obtain a series of Specific Gravities at different depths in mid-channel between Europa Point and the African coast. A strong easterly wind was blowing through the day, raising a considerable swell; and this had an obvious effect on the Sp. Gr. of the upper stratum, which showed a decided admixture of Mediterranean water with the Atlantic water properly forming it; for instead of the 1·0271, which we had found last year in this situation when a moderate westerly wind was blowing, the Sp. Gr. of the surface-water proved to be 1·0277; at 100 fathoms it had increased to 1·0279; and the same Sp. Gr. was found at 200 fathoms. At 250 fathoms, however, as in the two observations made in the former year at the same part of the Strait, we came upon unmistakable Mediterranean water, its Sp. Gr. being 1029·5; this showed itself again at 300 fathoms, and again in water taken from the bottom at 480 fathoms. Notwithstanding, therefore, the unusually high Sp. Gr. of the upper stratum, the slightness of the increase down to 200 fathoms, and the marked increase encountered between 200 and 250 fathoms, clearly showed that its character was essentially Atlantic; and thus we had distinct evidence that the Mediterranean stratum here lies *at least* 200 *fathoms from the surface.* It was further interesting to remark that the *whole* of the lower stratum was composed of *densest* Mediterranean water; whereas in two observations made last year at an interval of six weeks, the water at the bottom was found to be of *less* Sp. Gr. than the water at 250 fathoms. It thus appears that in this peculiar channel considerable changes may

be produced, even at a depth of from 400 to 500 fathoms, by the action of Winds and Tides.

55. On the 28th of August we proceeded to make a series of experiments with the Current-drag in the deep water off Point Cires on the African side; first, however, making a series of observations on the Specific Gravity of the water at different depths, for the purpose of ascertaining the position of the plane of separation between the two strata. This was very clearly indicated as lying between 200 and 225 fathoms; for while the Sp. Gr. of the surface-water was 1·0279 (being evidently raised by the westerly drift of Mediterranean water, kept up by the continuance of a strong East wind), and that of the water at 200 fathoms was 1·0283, the Sp. Gr. at 225 fathoms was 1·0298, and at 250 and 300 fathoms it was 1·0296. The continued strong East wind (force 6) obviously had a decided effect in retarding the *surface*-current, which, as the tide was ebbing during the whole series of observations, would probably have set very strongly to the eastward (it being near the date of full-moon) but for this opposing influence. The *under*-current, on the other hand, flowed *east*-wards during the same ebb at a rate in the first instance *exceeding* that of the surface-current, though subsequently *much less*, as is shown in the following Table:—

	Surface-Current.	Under-current.	
Time.	*Rate per hour.*	*Depth.*	*Rate per hour.*
1½ to 2 hours after H. W.	1·0 mile.	225 faths.	2·6 miles.
2½ „ „ „	2·0 miles	300 „	2·4 „
3 „ „ „	3·8 „	50 „	3·8 „
3½ „ „ „	4·4 „	100 „	2·6 „
4 „ „ „	—	150 „	1·9 „
4½ „ „ „	3·3 „	225 „	1·4 „

It thus appeared (1) that the influence of the ebb on the under-current was exerted earlier and more strongly than on the upper current, the former at first moving eastwards at the rate of 2·6 miles an hour, whilst the rate of the latter was only 1 mile; but (2) that at a more advanced period of the ebb the relative rates were reversed, that of the surface-current having increased to 4·4 miles an hour, and being still 3·3 miles when the drag, hanging at the same depth (225 fathoms) as in the first observation, showed a movement of the under-current of only 1·4 mile.—This suggests the question whether the rapid flow of the Under-current on this occasion *towards* the east may not—anomalous as the fact may seem—have been really due to the strength of the wind blowing *from* the east. For, as was pointed out by Sir Wm. Thomson and Prof. Stokes, in the discussion which took place on a paper which I submitted to the Mathematical and Physical Section at the Edinburgh Meeting of the British Association *, when a strong wind continues to blow *into* a loch or long narrow inlet, it

* See 'Nature,' Aug. 17, 1871, p. 316.

raises a "head" of water, the pressure of which must produce an *outward* Under-current; as is evidenced by the *continuance* of the surface in-current, which would otherwise cease as soon as the rise of the water is sufficient to neutralize by excess of gravity the force of the wind. Now here, although the westerly drift produced by the wind is not antagonized by the direct obstacle of a land-barrier, yet the strong in-current which meets it will obviously have the like tendency to raise a "head" of water, especially in that narrowest part of the Strait which lies between Tarifa and Point Alcazar; and the increase of pressure produced by this, acting in the direction of the Tidal movement of the under-current, would obviously tend to accelerate it.

56. With the view of obtaining another set of observations on the deeper (southern) part of the "ridge," we proceeded thither during the night, and took up our position, on the morning of the 29th, about 7 miles W.S.W. of Cape Spartel. There was a slight Westerly wind, with a smooth sea. The Specific Gravity of the surface-water, taken 2 hours after low water, when the flow of the tide was giving a westerly movement to both strata, was 1·0275; thus showing, by its excess above the ordinary standard of Atlantic water, that the surface-drift from the Mediterranean produced by the continuance of strong Easterly wind had reached this end of the Strait. The Sp. Gr. at 130 fathoms was 1·0282, at 155 fathoms it was 1·0284, and was the same on the bottom at 180 fathoms. Thus it appeared that as the lower stratum here consists of a *mixture* of Mediterranean and Atlantic water, the predominance of the outward under-current is not indicated so strongly by Sp. Gr. as it is on the shallower (northern) part of the ridge, where the lowest stratum consists of nearly *pure* Mediterranean water (§ 52).—The results of this day's experiments with the Current-drag confirmed the conclusion indicated by the previous series, as to the complete Tidal reversal of both currents at this end of the Strait; whilst they gave even stronger evidence of the excess of *in*ward movement in the upper stratum, and of *out*ward movement in the lower. From 2 to 5 hours after Low water, the *surface*-current was running *west*wards at the rate of 0·75 mile; it then slackened, became stationary at High water, and then flowed *east*wards, its rate increasing at 3½ hours after High water to 1·2 mile per hour. On the other hand, the *under*-current at 3½ hours after Low water was shown, by a drag hung at 155 fathoms depth, to be flowing *west*wards at the estimated rate of 1·25 mile per hour, and from 3½ to 4½ hours was still running at the rate of 1·10 mile; whilst at from 3 to 3½ hours after High water it flowed *east*wards at the estimated rate of only 0·67 mile per hour; the excess of *in*flow of the *upper*-current, and that of *out*flow of the *under*-current, being thus each 0·45 mile per hour.

	3 hours after L. W.	3 hours after H. W.
Surface-current	W.⟵ 0·75 mile per hour.	⟶E. 1·20 mile per hour.
Under-current	W.⟵ 1·12 mile per hour.	⟶E. 0·67 mile per hour.

57. On the 30th of August (Full Moon) the ship was taken into the

position occupied on the 28th; with the view of experimenting in the narrow part of the Strait for as long a period of the *flood*-tide as possible, so as to compare the results with those previously taken during the ebb. The wind was here still from the East, with a force of from 3 to 4. A *surface*-current-drag having been put over very soon after Low water, it drifted at first nearly due South, or *across* the channel, but in the second and third hours changed its course to the S.W., moving at the rate of 1·6 and 1·5 mile per hour. Having got into the in-shore current near the African side, the drag was then taken up and started afresh in the middle of the Strait, close to the under-current-drag; and it then, between 4 and 5 hours after Low water, drifted W.S.W. at the rate of 0·55 mile per hour. At High water, the surface-drag became stationary, and soon after commenced drifting to the eastward. As the wind freshened at the same time to a force of 6, the surface-drag was taken up; but the drift of the ship during the rest of the ebb-tide denoted a strong east-running surface-current. The direction of the *under*-current, as indicated by a current-drag at 250 fathoms depth, was very decidedly *west*ward, while the surface-buoy was moving nearly due south; and its rate at 2½ hours from the beginning of the ebb was estimated at 1·8 mile per hour. At from 3 to 4 hours after low water, the rate of the *west*ward under-current was 1·6 mile; at from 4 to 5 hours it was still 1·25 mile; and between 5 hours and high water it was 0·8. The direction of the under-current changed at High water; and at 1 hour afterwards it had acquired an *east*ward rate of 1·0 mile, which increased to 1·6 at 2 hours after H. W., its *in*ward rate thus approaching to an equality with its *out*ward rate during the flood, which may not improbably have proceeded (as on the 28th) to an absolute excess.—It may fairly be questioned, however, whether the strength of this *easterly* Under-current during the ebb was not to be attributed (as already suggested) to the continuance of the strong Easterly wind, which by heaping up the surface-water, would augment the downward pressure on the under-stratum. It is considered by Capt. Nares, whose judgment on such a point is entitled to great weight, that the observations made at the Mediterranean end of the Strait are not by any means so satisfactory as those made on the "ridge" at the Atlantic end; since "the eddies "which would naturally be expected at this part, in consequence of the "funnel-shaped mouth of the Strait, complicate the movements and prevent "such exact demonstrations as those found further to the westward, where "the current-stream runs more steadily."

58. Giving our chief attention, then, to the phenomena presented at the Western extremity of the Straits, we find these to be as follows:—

I. There is a reversal, with every ebb and flow of the Tide, in the direction alike of the *upper* and of the *under*-current: but while the *under*-current is the stronger during the *flood* or *out*going tide, the *upper* current is the stronger during the ebb or *in*going tide; and while the balance of the *out*ward and *in*ward movement during each tide is decidedly *in*wards

in the *upper* current, it is decidedly *out*wards in the *under*-current (§§ 53, 56).

II. The *upper* stratum here always consists of nearly unmixed Atlantic water; showing that the westerly flow which takes place during the ebb is insufficient to bring Mediterranean water from the opposite end of the Strait. The increase sometimes observable (§§ 51, 54) in the Sp. Gr. of the surface-water of the Strait appears attributable rather to wind-drift than to tidal movement.

III. On the other hand, the *under* stratum here (as at the opposite end of the Strait) either consists entirely of *Mediterranean* water, or shows a large admixture of it; and as this is the case alike during the *in*ward and the *out*ward movement of the under stratum, it seems obvious that Mediterranean water must extend some distance to the westward,—since, if it merely came up to the "ridge" without crossing it, the tidal inflow would replace it by Atlantic water. And this inference is confirmed by the fact that an admixture of Mediterranean water is distinctly traceable far into the Atlantic basin (§ 49).

IV. The stratum of Mediterranean water which is found at a depth between 100 and 125 fathoms on the "ridge" (§ 52) must be derived from the stratum which, in the deeper part of the Strait (extending westward as far as Tarifa), lies at more than 200 fathoms from the surface. Hence the lower or Mediterranean stratum must form an inclined plane from Tarifa to the "ridge," the surface of which is about 100 fathoms higher at its western than at its eastern end; and as the distance between these two points is about 15 miles, the gradient will be about $\frac{100 \text{ fathoms}}{15 \text{ miles}}$ or about 1 in 132. But it must be remembered that we have not here to do with the *absolute weight* of this body of water, but only with the *difference* in Sp. Gr. between water of (say) 1·027 and water of 1·029; which is less than 1-500th part of the absolute weight of the water which thus runs up-hill. And with the evidence we have in the Florida Channel (§§ 129–133) of the passage of the denser because colder water over the shallowest part of the Narrows, there is no improbability in the like flow of the denser because more saline water over the marine watershed which separates the Mediterranean from the Atlantic basin, if there be a predominant westerly movement in the under-stratum. But if there were no such movement,—the under-stratum being either at rest, or its flux and reflux being equal,—there seems no reason why this inclination of its surface should be maintained against the operation of Gravity, which will be constantly tending to bring it to a level.

59. I venture, therefore, still to maintain that the existence of a Tidal flux and reflux in the Gibraltar currents does not prove that they are entirely sustained by tidal action; any more than the existence of an alternate ebb and flow in a river proves that there is no down-flowing stream. In every tidal river, the ebb is stronger on the whole than the

flood; the excess being proportional to the amount of fresh water which the upper part of the river brings down. And thus it happens that a floating body thrown into such a river is at last carried out to sea, though it may have been brought back by the tide twenty, fifty, or a hundred times, each time stopping at a point a little further down than before. Now there can be no doubt whatever, that, putting the action of Wind out of the question, a vessel which enters the Western end of the Strait would be gradually carried into the Mediterranean by the predominant *easterly* movement of the *upper* current; though its general easterly progress would be interrupted by a succession of returns to the westward, or, when there might be no actual return, by periods of rest. And the evidence appears to me just as conclusive, that if a body could be so weighted as to remain freely suspended in the Mediterranean stratum off Gibraltar, and its movements could be watched from above, we should find it in like manner gradually working its way towards the opposite end of the Strait, and at last clearing the "ridge" to descend along the Atlantic slope beyond. This it could not do unless the Tidal action were supplemented by an outflow produced by some other agency; and such an agency exists in the excess of lateral pressure in the Mediterranean column above that of the Atlantic column, which is proportional to the excess of the Specific Gravity of Mediterranean water.—In the strong continuous Under-currents whose existence in the Dardanelles and in the Baltic Sound has now been conclusively established, we see what larger differences in Specific Gravity, uncomplicated by Tidal action, can effect. (See Appendix II.; also § 134.)

60. In one respect it appears to me that the Specific Gravity observations,—by which it can be certainly determined whether the water at any given depth is Atlantic, Mediterranean, or a mixture of the two,—are more satisfactory than those made by the Current-drag. For the *actual* movement of the drag can only give the *real rate* of movement of the stratum in which it is hanging, when that rate is the same as that of the surface-stratum in which the suspending buoy is floating; in which case there will be no *pull* of the buoy upon the drag, or of the drag upon the buoy (see Appendix II.). If the *under*-current be flowing faster than the upper, the drag will be kept *back* by the buoy; whilst if the *surface*-current be flowing faster than the deeper stratum, the drag will be pulled *onwards* by the buoy. If, again, the two strata be moving in opposite directions, the reciprocal influence of the drag and the buoy will make the *observed* rate still more different from the *real* rate, and perhaps will even render both *stationary*. Hence the *actual* rate of the under-current can only be *estimated* from the observed rate of movement of the suspending buoy; and the rule adopted by Capt. Nares in making such estimates was to halve the difference between the observed rate of the suspending-buoy and that of the surface-current, and to *add* this half to the former when the rate of the under-current was the greater, *deducting* it when the rate of the surface-current was the greater. Thus when the surface-current was running at 1·0 mile an hour,

and the buoy moved in the same direction at the rate of 1·35 mile per hour, the real rate of the under-current was estimated at 1·35+0·17, or 1·52 mile per hour. On the other hand when the surface-current was running at 4·4 miles per hour, and the buoy moved in the same direction at the rate of 3·2 miles per hour, the real rate of the under-current was estimated at 3·2 — 0·7, or 2·5 miles per hour.

61. In the event of any renewal of such experiments, either in the Strait of Gibraltar or elsewhere, I should recommend that the relative resistances of the Current-drag and of the Suspending-buoy at different rates of movement should be tested, in the first instance, by drawing them separately through still water with a Dynamometer attached to the line; and further, that by suspending the current-drag at different depths, and by testing the resistance then offered in each case by the drag and the line conjointly, the amount due to the latter should be determined by the elimination of the former. If in this manner the amount of resistance that is offered by (say) every 20 fathoms of the line from which the drag is suspended be ascertained, we shall have satisfactory data for determining, with a near approach to accuracy, the actual rate of the current in which the drag is suspended. For as it may be pretty certainly determined by Specific Gravity observations to what depth the upper stratum extends at the point of observation, it will be easy to compute, on the one hand, the force exerted by its current upon the suspending-buoy and on the upper part of the line that hangs from it; and, on the other, the force of the under-current, not only upon the drag, but upon the portion of the suspending line that hangs in it: and from these data the *actual rate* of the under-current can be readily worked out with a near approach to accuracy.

62. *Temperature of the Strait of Gibraltar.*—It will be remembered that in the previous year's work a marked reduction of Temperature was observed in the mid-stream of the Strait, as compared with the water nearer the Spanish side; and the inference drawn from these observations was, that "either the water of which the in-current consists is drawn from " a part of the Atlantic at least as far north as Lisbon, or that it is derived " from a stratum of the neighbouring ocean somewhat beneath the surface, " so as to have received less of the solar superheating than the actual surface- " water" (Report for 1870, § 74). On the other hand, the excess of temperature in the surface-water of the northern side of the Western *embouchure* of the Strait was considered as indicating that there is a predominant surface-outflow of Mediterranean water along the Spanish coast; a fact, I was informed, well known to those who have navigated it (§ 75). —Being desirous of obtaining further information on this point, I requested Capt. Nares to take observations of surface-temperature at short intervals, on the two occasions on which we were running obliquely across the Strait. One of these series (I.) extends along a line of about 10 miles from the neighbourhood of the Pearl Rock, at the entrance of Gibraltar Bay, to a short distance west of Point Cires on the African Coast; the other

(II.) from Tarifa to the eastern entrance to Tangier Bay, a line of about the same length, but about ten miles further west:—

I.			II.			III.		
Pearl Rock	72½°	Fahr.	Tarifa......	67°	Fahr.	Cape Spartel	68°	Fahr.
"	70½	"	"	66	"	"	68	"
"	68½	"	"	64	"	"	65	"
"	65½	"	"	63	"	"	64	"
"	62	"	"	62	"	"	63	"
"	60	"	"	62	"	"	63	"
"	59	"	"	63	"	"	64	"
"	60	"	"	62	"	"	62½	"
"	60	"	"	62	"	"	62	"
Point Cires	59½	"	Tangier Bay	60½	"	Tangier Bay	60½	"

Besides these, another Series (III.) was taken along the African coast, between a point a little to the west of Cape Spartel and the western entrance of Tangier Bay. And other observations taken to the S.E. of Europa Point gave temperatures of 72° and 71°·5, which corresponded with those obtained last year near the entrance of the Mediterranean, its proper temperature being here somewhat reduced by the inflow of colder water through the Strait (Report for 1870, § 77).—Hence it appears that whilst the water nearest the Spanish coast in Series I. had the temperature of the Mediterranean, there was a rapid fall in the thermometer as we came into the mid-stream, and a still further reduction occurred towards the African side,—the lowest temperature observed being 13°·5 beneath the highest, simply in changing our place a few miles to the southward. At Tarifa the influence of the Mediterranean temperature was less marked; but the temperatures taken near the African side within the embouchure of the Strait were nearly as low as in Series I.

63. I learn from Dr. Hooker, who has lately visited Morocco, that the prevalence along that coast of a temperature decidedly below that of the opposite coast of Spain, is a fact which has long been known locally; and that it is indicated at present by the character of the Flora, whilst a still more marked reduction in past times is marked by the Boreal character of the Shells found in the later Tertiary deposits. Looking to the fact that a general *southerly set* is traceable in the water of this part of the Atlantic,—partly produced, it would appear, by predominant northerly wind-drift, and partly by the indraught requisite to supply the westerly Atlantic drift produced by the Trade Winds (§ 120),—I am disposed to think that the lower temperature of the Gibraltar in-current is due to its original derivation, not from the portion of the Atlantic immediately outside it on the same parallel, but from an area of lower surface-temperature to the north-west. But it seems at the same time not improbable that just as the water which flows over a mill-dam is drawn, not so

much from the surface-stratum, as from a stratum a little below it*, so the current which is (as it were) sucked-in through the Strait to supply the excess of evaporation in the Mediterranean should be drawn from a sub-surface stratum, and should thus bring with it a lower temperature.

64. On our entrance into the Mediterranean, we kept in the first instance along the African coast, and took frequent observations of surface-temperature; but we found that we soon lost all indication of the depression observed within the Strait, the thermometer never falling lower than 70°; so that this cold in-current speedily loses its peculiar character by mingling with the general body of Mediterranean water.

Part III.

Physical Researches in the Mediterranean.

65. *Temperature.*—The Temperature-phenomena of the *Eastern* basin of the Mediterranean were found to correspond in all essential points with those previously determined in the Western. The limitation in the thickness of the stratum superheated by direct insolation, and the almost exact uniformity of temperature beneath this stratum down to the greatest depths, are features no less characteristic of the former than of the latter,—being, in fact, yet more remarkable in the circumstance that the range of depth in the Eastern basin is about 500 fathoms greater than in the Western, some parts of its bottom lying at nearly 2200 fathoms depth from its surface. But certain differences were also noted which are sufficiently important to deserve being stated in detail.

66. It should be mentioned, in the first place, that the heat of the latter part of September 1871 was felt at Malta to be peculiarly oppressive, the temperature of the air occasionally rising to 90°, and that of the sea to 80°; and that this high temperature did not suffer much reduction as we proceeded towards Egypt during the first fortnight of October. On the 3rd of that month, in Lat. 35° 54′ N. and Long. 16° 23′ E., the surface-temperature of the sea was still 80°; and on the 11th, in Lat. 32° 17½′ N. and Long. 26° 44′ E., it was 79°: at the former of these stations the *bottom*-temperature at 1650 fathoms was 56°,—thus corresponding exactly with the bottom-temperature which we obtained last year at 1743 fathoms (in Lat. 36° 31′ N. and Long. 15° 46′ E.) a little to the east of Malta,—and at the latter it was 56°·7 at 1970 fathoms.

67. A *series* of Soundings was made (at my request) by Capt. Nares at each of these Stations, for the determination of the Temperature at successive intervals of 10 fathoms, from the surface down to 100 fathoms; and below this at intervals of 50 fathoms, until the depth should be reached at which the temperature becomes uniform down to the bottom. The results of these soundings are given in Columns III. and IV. of the

* See Maury's 'Physical Geography of the Sea,' § 387.

following Table, in correlation with those of two sets of serial soundings (Columns I. and II.) taken last year in the Western basin:—

Summer Temperature of the Mediterranean, as shown by Serial and Bottom-Soundings.

	Western Basin.				Eastern Basin.			
	I.		II.		III.		IV.	
	Lat. 36° 0′ N. Long. 4° 40′ W.		Lat. 35° 59′ N. Long. 5° 55′ E.		Lat. 35° 54′ N. Long. 16° 23′ E.		Lat. 32° 17½′ N. Long. 26° 44′ E.	
	Degrees Fahr.	Reduction below Surface.	Degrees Fahr.	Reduction below Surface.	Degrees Fahr.	Reduction below Surface.	Degrees Fahr.	Reduction below Surface.
Surface	74·5		77·0		80·0		79·0	
10 fath. ...	69·3	5·2	71·0	6·0	76·2	3·8	76·0	3·0
20 „ ...	65·0	9·5	61·5	15·5	72·6	7·4	76·0	3·0
30 „ ...	63·0	11·5	60·0	17·0	66·2	13·8	75·5	3·5
40 „ ...	61·7	12·8	57·3	19·7	63·5	16·5	69·0	10·0
50 „ ...	59·7	14·8	56·7	20·3	61·0	19·0	67·0	12·0
60 „ ...	...	...	...	...	59·7	20·3	65·5	14·5
70 „ ...	...	...	...	...	59·2	20·8	61·8	17·2
80 „ ...	...	...	...	...	58·8	21·2	61·0	18·0
90 „ ...	...	...	...	...	58·5	21·5	59·8	19·2
100 „ ...	55·1	19·4	55·5	21·5	58·5	21·5	59·5	19·5
150 „ ...	...	...	...	...	58·0	22·0	59·5	19·5
200 „ ...	...	...	...	...	56·5	23·5	58·5	20·5
300 „ ...	...	...	...	...	...	...	57·0	22·0
400 „ ...	...	...	...	...	...	...	56·0	23·0
500 „ ...	...	...	...	...	...	...	57·0	22·0
586 „ ...	55·0	19·5	..	...	...	...	...	...
1456 „ ...	...	...	55·0	22·0	...	...	...	...
1650 „ ...	...	...	...	...	56·0	24·0	...	...
1970 „ ...	...	...	...	...	...	...	56·7	22·3

These results are also graphically expressed, as regards the superficial stratum, by the Curves in Diagram II., Plate IV.

68. Now it will be observed that in these four Series the rates of descent are by no means in accordance with each other, the differences between the first three, however, being much less than between any one of them and the fourth. The nearest resemblance is that which exists between Nos. I. and III.; for although the surface-temperature is 5½° higher in the latter, the rates of reduction at successive depths so nearly correspond, as to give the two curves a general parallelism; the temperature in No. III. at 100 fathoms being still 3°·4 above the temperature at the same depth in No. I. But while in No. I. the temperature exhibits no further reduction down to the bottom at 586 fathoms, it falls 2° between 100 and 200 fathoms in No. III., so as to come down to within little more than a degree of the temperature of the constant stratum in No. I., and

nearly the whole of this excess it retains to the bottom at 1650 fathoms. The rate of reduction in No. II. differs from the preceding in being much *more* rapid between 10 and 20 fathoms; the reduction in that stratum being 9°·5, as against 4°·3 in No. I., and 3°·6 in No. III. And this depression extends to the subjacent strata; the temperature falling almost to its minimum at 50 fathoms. On the other hand in No. IV. the temperature of the upper stratum shows a much *less* rapid reduction than in either No. I., II., or III.; the high surface-temperature being carried down to 30 fathoms with a loss of only 3½°. Between 30 and 40 fathoms, however, the reduction is more considerable, amounting to 6½°; but from this depth to 60 fathoms, the temperature is still about six degrees higher than at the like depth in No. III.; and whilst at 60 fathoms the temperature in No. III. is only 3°·7 above that of the bottom, it is 8°·8 above that of the bottom in No. IV. Whilst in No. III., however, the further reduction takes place very slowly, so that the temperature only falls 1°·2 between 60 and 100 fathoms, there is a fall of 6° in this stratum of No. IV., which brings down the temperature at 100 fathoms to within one degree of the temperature at the same depth in No. III. A slight excess appears to be maintained at yet greater depths; the temperature at 200 fathoms being 2° higher in No. IV. than in No. III., and being still 1°·8 higher in No. IV. than the temperature of the bottom, whilst in No. III. it is at that depth only 0°·5 higher; and the bottom-temperature of No. IV., even at a depth of 1970 fathoms, was 0·7 in excess of the bottom-temperature of No. III. at 1650 fathoms.

69. It would thus appear:—(1) that the high surface-temperature which we met with during the early part of October, in proceeding from Malta to Alexandria, extended to a much greater depth in Long. 26° 44′ E. than it did in Long. 16° 23′ E.; (2) that the stratum of which the temperature is considerably raised by insolation was much thicker at the former than at the latter station; and (3) that the temperature of the stratum between 100 and 200 fathoms still exhibits in the Eastern basin some evidence of being influenced by direct insolation, the bottom-temperature which is met with in the Western basin at all depths below 100 fathoms being here not encountered until 200 fathoms have been passed.

70. How far these differences are due to the different conditions of the Eastern and Western basins, or are attributable to the remarkable prolongation of a high Summer temperature in the season of 1871, is a point which can only be determined by a much more extended series of observations. There are, however, some considerations which may help us to a probable conclusion.—In the first place, it appears certain that a higher *surface*-temperature prevails over the Eastern basin generally than over the Western. This is doubtless partly due to its lower Latitude; for whilst the axis of the Western basin may be considered to correspond with the parallel of 38°, that of the Eastern corresponds with the parallel of 34°. But what has probably a yet greater effect, is the absence along the Eastern portion

of North Africa of any mountain-barrier like that of the Atlas and other ridges which intervene between the Sahara and the coasts of Morocco and Algèria, and cool down those heated winds which blow from Central Africa without any moderating influence over the low expanse of Tripoli and Egypt. And further, while the surface-temperature of the Western basin is also kept down by the admission of colder water through the Straits of Gibraltar, that of the Eastern basin will be elevated rather than depressed by the discharge of the vast body of water brought down by the Nile through the hottest months of the year. And thus it comes to pass that the summer isotherm of 80° which runs through the interior of Northern Africa from Mogador to the Syrtis, thence follows the coast-line between Tunis and Syria, then turns northwards along the coast of Syria, and returns along the coast of Asia Minor, so as to enclose the Levant in a "bight" of high temperature.—Looking, again, to the fact that the *bottom*-temperature was found in the previous year to be somewhat higher, even in the Western basin, to the east of Sardinia, than nearer the Strait of Gibraltar, I think it probable that the permanent bottom-temperature of the whole Eastern portion of the Mediterranean is about 2° higher than that of the Western. The cause of this difference, also, is to be sought in the conditions which affect the surface-temperature; for, as I showed in my last year's Report (§§ 88–91), the complete separation between the deeper water of the Mediterranean and that of the Atlantic will cause the temperature of the former to be the *lowest mean* of the locality; and thus as the *winter* surface-temperature of the Eastern basin is somewhat above that of the Western, its *bottom*-temperature also will be higher. A remarkable confirmation of this view is afforded by the fact already stated respecting the temperature of the Gulf of Suez (§ 11).

71. It might be anticipated, therefore, that such an excess of temperature in the whole superficial stratum heated by direct insolation would show itself in the Eastern basin, as is represented by Series III. (Table and Diagram) when compared with Series I. and II. But the extraordinary elevation shown in the superficial stratum of No. IV. seems to betoken some special calorifying agency; for although the difference of about $3\frac{1}{2}°$ of Latitude in favour of that Station might account for a general elevation, yet as this does not show itself at all in the *surface*-temperature, and scarcely at all in the *bottom*-temperature, there seems no reason for its showing itself to such a marked degree in the *intermediate* temperatures,—especially in those of 20 and 30 fathoms. Now if I was correct in the interpretation which I gave, in my last year's Report (§§ 90, 91), of the mode in which the Sun's heat penetrates downwards by *convection*,—the surface-films successively concentrated by evaporation sinking until their excess of density is lost by diffusion,—it is obvious that the longer the continuance of surface-heat, especially if combined with dryness, the deeper would its influence extend, though the surface-temperature might not itself be raised. For, as I then remarked, "the continual repetition of this process

"through the hot season will carry the elevation of temperature further "and further down; but so soon as the temperature of the Air falls much "*below* that of the Sea, the surface-layer being cooled will become heavier "and sink, and will thus carry down *cold* instead of heat." Now as Series IV. was taken not merely eight days later than Series III., but also considerably nearer to the northern coast of Africa, from which a hot dry wind was continually blowing, I am inclined to believe that this remarkable downward extension of a temperature as high as the mean between the surface-temperatures of Nos. I. and II. is attributable to these accidental influences, the local effect of which would be very little interfered with in this part of the Mediterranean by any horizontal movement of water,—neither tides nor currents having any considerable force along the Tripoli Coast.—It may be hoped that as Serial temperature-observations down to 100 fathoms can be made with very little difficulty or expenditure of time, such observations may be systematically prosecuted in different parts of the Eastern basin, so that it may be ascertained what are the conditions by which the rate of reduction of temperature from the surface downwards is determined.

72. *Density of Water.*—It might be anticipated that a higher density would prevail in the Eastern basin of the Mediterranean than in the Western, for two reasons:—first, that the hot and dry winds of the Libyan desert, unchecked and untempered by the interposition of any mountain-range, would produce a greater evaporation from its surface; and second, that the reduction produced by the inflow of Atlantic water would scarcely operate at so great a distance. Accordingly the analyses of Prof. Forchhammer gave a larger proportion of saline matter in the water to the eastward of Malta; and my own determinations of Specific Gravity lead to the same result. These observations, which were frequently repeated upon surface-water as we proceeded eastward from Malta, did not at first indicate any decided increase in density; for in the previous year (Report for 1870, § 92) we had found the Sp. Gr. of the surface-water in the neighbourhood of Sicily to average 1·0280, sometimes rising as high as 1·0284; whilst between Malta and Crete we now found it to vary between 1·0284 and 1·0288. The Sp. Gr. of the sample brought up by the water-bottle from the depth of 2000 fathoms, in the Sounding taken on the 3rd of October about 100 miles to the east of Malta, was 1·0291; thus being slightly in excess of that of the surface-water, which was 1·0288. In the deep Sounding taken eight days afterwards, not far from the Gulf of Solloom on the Libyan coast, the density was found still greater; the Sp. Gr. of the bottom-water obtained from a depth of 1650 fathoms being 1·0294, while that of the surface-water was 1·0293. In another Sounding taken on the following day nearer the coast of Africa, the density of the bottom-water at a depth of 365 fathoms was found to be much more in excess of that of the surface-water; the Sp. Gr. of the former being 1·0302, while that of the latter was only 1·0294. These results agreed in a very marked manner

with those obtained last year in the Western Basin (Report for 1870, § 94), as to the fact of the difference in Salinity between the surface- and bottom-water being greatest where the depth is moderate; and the fact of course becomes yet more significant when the general increase of Salinity is so marked. There appears, then, no reason to doubt the explanation offered last year; viz. that supposing an equal degree of concentration by surface-evaporation to take place in two or more equal areas, the elevation of the Sp. Gr. of the entire column of underlying water will be inversely proportional to the height of that column: for the diffusion of equal amounts of concentrated water through columns whose heights are in the proportion of 1, 2, 3, will raise the Sp. Gr. of these columns respectively in the proportion of 3, 2, 1; the *shortest* column being *most* affected, while the *longest*, in which the same amount of concentrated water is diffused through three times the quantity, has its density but little raised.

73. *Nature of Bottom.*—The character of the bottom indicated by the two deep Soundings which we took in the Eastern basin, corresponded with that which was ascertained last year to prevail in the deeper part of the Western,—the samples of the deposit brought up by the Sounding-apparatus showing it to consist entirely of clayey mud composed of particles in a state of extremely minute division, and the water brought up by the water-bottle from the stratum immediately overlying the bottom being rendered turbid by the presence of like finely divided particles in suspension. Mud of a similar character has been almost universally found by Capt. Spratt* and Capt. Nares (both of whom have been engaged in surveying the Eastern Basin of the Mediterranean) at depths exceeding 250–300 fathoms; sometimes, however, containing minute Foraminiferal shells. A careful examination of the two samples just alluded to gave no trace either of Foraminifera or of any other Organic forms; and as it is only in comparatively shallow waters that such traces are met with, I am inclined to believe that wherever they occur they must have been derived from such a littoral bottom as we found along the Tripoli coast, where Foraminifera abounded at depths of from 100 to 150 fathoms.—The material of the muddy deposit on the bottom of the Eastern basin must probably be for the most part derived from the Nile; and that it should have exactly the same character near the western border of the basin as it has in much closer proximity to the mouths of that river, is a striking indication of the enormous amount of such material which must be brought down by its current, and of the length of time required for the subsidence of the most finely divided particles.

74. In my last year's Report (§§ 100–103) I laid great stress upon the slow subsidence of the sedimentary particles, and the turbidity of the bottom-water which is thus produced, as possibly accounting for the scantiness of Animal life on the deep-sea bottom of the Mediterranean; and I threw it out for the consideration of Geologists, whether the same explanation

* 'Travels and Researches in Crete,' vol. ii. p. 319.

might not apply to various cases in which there is a like scantiness of Animal remains in deposits of considerable thickness.—This view, however, has been called in question by Dr. McIntosh; who adduces, as an objection to it, that many Annelids and Nemerteans abound in muddy water, while some live nowhere else than amongst mud or muddy sand; and also that many littoral Sponges are found on extremely muddy ground, while "the Siliceous Sponges all over the world affect a muddy bottom." "In general," he adds, "muddy ground is found to be "much more productive in marine life of all kinds, than where the "rocks, sea-weeds, and sands are pure. . . . . . Even where the margin "of the sea is rendered perfectly turbid from mud (and this, too, calca-"reous), as at White-Cliff Bay, in the Isle of Wight, marine animals are "abundant between tide-marks"*. None of the instances cited by Dr. McIntosh are really parallel to that of the abyssal deposit of the Mediterranean; for they are all cases of *littoral* deposit, consisting of particles very much coarser (often mixed with sand), which the tidal movement of the suspending water will keep washing-*off*, as much as washing-*on*, the respiratory surfaces. In no other part of our survey of the deep-sea bottom has there been any difficulty in clearing the water immediately overlying it by filtration. On our wonderfully rich "Holtenia-ground," the deposit in which the Siliceous Sponges were imbedded was any thing but "muddy," for it mainly consisted of Sand and Globigerinæ; and the water above it was free from any turbidity, save that arising from the presence of very young Globigerinæ. The whitening of the littoral water produced by the disintegration of Chalk-cliffs is not in the least degree comparable to the turbidity which results from the presence of very finely divided particles of clay; for the Calcareous particles settle down comparatively quickly, and are easily washed off again; whilst the far more minute particles of Clay are very long in subsiding, but, when they *have* been deposited, are extremely adhesive.

75. My own dredging-experience certainly does not bear out Dr. McIntosh's statement as to the more productive character of muddy bottoms. Those who have worked at Tenby or in Lamlash Bay will be slow to admit that "purity" of the "rocks, sea-weeds, and sands" in those localities is less favourable to Marine Life than the foulness of "odoriferous mud;" though there are doubtless animals to which the latter is congenial.—The following passage from Dana's recent Treatise "On Corals and Coral Islands" gives the experience of a very accomplished observer, acquired over a very extensive range. "The effects of sediment on grow-"ing Zoophytes are strongly marked, and may often be perceived when a "mingling of fresh water alone produces little influence. We have men-"tioned that the *Porites* are reduced to flattened masses by the lodgment of "sediment. The same takes place with the hemispheres of *Astræa*; and it "is not uncommon that in this way large areas at top are deprived of life.

* Ann[illegible] Nat. Hist. Jan. [illegible]

"The other portions still live unaffected by the injury thus sustained. Even "the *Fungiæ*, which are broad simple species, are occasionally destroyed "over a part of the disk through the same cause, and yet the rest remains "alive. It is natural, therefore, that wherever streams or currents are "moving or transporting sediment, there no Corals grow; and for the same "reason we find few living Zoophytes upon sandy or muddy shores" (p. 121). —I venture, therefore, still to maintain that the doctrine I propounded in my former Report has not been set aside by Dr. McIntosh's facts and arguments.

76. *Gases of the Bottom-water.*—I am now disposed, however, to attribute more influence to the other condition which I suggested in my Report for 1870 (§ 103) as likely to operate prejudicially to Animal life; namely, *the stagnation produced by the almost entire absence of Vertical Circulation.* In the great Oceanic system, if the doctrine previously advocated be correct, every drop of water is in its turn brought to the surface, and exposed to the purifying influence of prolonged exposure to Atmospheric air; whereby a large proportion of its Carbonic acid and other products of the decomposition of Organic matter will be removed, and Oxygen will be absorbed in their place. But from this movement, the water of the Mediterranean may be said to be virtually excluded. The effect of the Gibraltar Currents is limited to a stratum of which the depth is very small in comparison with that of the principal area of each basin; and to whatever extent they may produce a change in the water of the deeper part of the Western basin in the neighbourhood of the Strait, it is obvious that the amount of such change must diminish as the distance increases, and that it cannot in any degree affect the water of the Eastern basin. Now, as the Nile is constantly bringing down a very large quantity of Organic matter, the finer particles of which seem to be diffused through the whole mass of the water in the basin and to be slowly gravitating to its bottom, it might be anticipated that in their gradual decomposition they would generate Carbonic acid at the expense of the Oxygen dissolved in the water; so that the abyssal water, being separated from the atmosphere by an intervening stratum of many hundred fathoms, and being never brought to the surface, would come to be unfit for the maintenance of Animal life.

77. It will be recollected that in the 'Porcupine' Expedition of 1869*, it was found that the presence of a very large proportion of Carbonic acid in the bottom-water was not incompatible with the existence of Animal life in great abundance. In fact there was reason to believe that there was a general relation of conformity between the proportion of Carbonic acid and the quantity of Animal life on the bottom as indicated by the dredge-results; the effect of the respiratory and other changes produced by the latter being to increase the proportion of Carbonic acid at the expense of the Oxygen. Thus whilst the percentage of Oxygen in *surface*-water averaged about 25 per cent., and that of Carbonic acid averaged something less than

* See "Report," pp. 483–486.

21, the Oxygen in *bottom*-water did not average above 19·5 per cent., while the Carbonic acid increased to nearly 28,—the percentage of Nitrogen being reduced at the same time from 54 to 52·5. The percentage of Carbonic acid in bottom-water often rose much higher than this, being frequently between 30 and 40, and in one instance more than 48; but the percentage of Oxygen did not show a corresponding reduction, being never less than 16, while that of Nitrogen came down from 54 to 34·5. Thus it appeared that *so long as Oxygen was present in sufficient proportion*, the increase of Carbonic acid to nearly half the total amount of the gases removable by boiling did not exert any unfavourable influence on Animal life; from which it might be surmised that the Carbonic acid *dissolved* in water under great pressure is in a condition altogether different from that of *gaseous* Carbonic acid as regards its relation to Animal Respiration*.

78. Having had an opportunity, in the 'Porcupine' Expedition of 1869, of making myself acquainted with the method on which the Analyses of the Gases of Sea-Water were conducted, and having provided myself on this occasion with the requisite Apparatus, I believed myself able—though not claiming to be an expert in Chemical Manipulation—to carry on similar analyses with sufficient accuracy to determine whether the condition of the abyssal water of the Mediterranean is really such as I had suspected it to be. Each of the samples taken in the two deep Soundings (§ 72) was boiled until no gas came over; and the total quantity of gas given off, which corresponded very closely with the average formerly obtained, was divided in each case into two parts, so that I had four specimens in all. The composition of these specimens agreed very closely; the percentages being approximately (for I do not pretend to minute accuracy) as follows: Oxygen 5, Nitrogen 35, Carbonic acid 60. Thus it appeared that very nearly the whole available Oxygen had been converted into Carbonic acid; so that while the proportion of Oxygen to Carbonic acid was never in the open Ocean less than *one third*, it was here no more than *one twelfth*, —a difference fully adequate to account for the paucity of Animal life on the deep bottom of the Mediterranean. That this condition does *not* extend to those moderate depths in which the water is subjected to the disturbing action of winds, tides, and currents, may be fairly presumed: but whether it prevails through the whole stratum beneath 250 or 300 fathoms, so as to constitute the essential condition by which Animal life is limited to these depths, it would obviously be premature to assert.

* It is by no means improbable that such sluggish animals as Mollusks and Echinoderms may be able to bear a much larger proportion of Carbonic acid in the water they breathe than Fishes and Crustacea. Experimental inquiries on this point, which might be readily carried out in connexion with any large Aquarium, would give results of great Physiological interest.

Part IV.

Biological Researches in the Mediterranean.

79. The Dredgings which we carried on upon the Adventure and Skerki Banks, between Sicily and the Coast of Africa, chiefly at depths between 100 and 200 fathoms, did not add any thing of importance to the collection which we made in the previous year in the same locality, save some Sponges of considerable interest. We were struck with the limitation of the areas to which particular types appeared restricted; for when encouraged by "a good haul" to put down the dredge a second time in the same locality, it frequently came up again almost empty. Whether this depended on a slight change of depth, or on the direction of under-currents, which probably have a considerable influence on these shallows, it is impossible to say. This fact, however, seemed constant,—that below 150 fathoms Animal life was extremely scanty; whilst even down to that depth *Nullipores* were often very abundant, the dredge coming up full of them. As there is no doubt of the Vegetable character of *Nullipora*, I deem this fact of considerable significance, as indicating that Light must penetrate to that depth in sufficient amount to enable this plant to decompose carbonic acid.—Our Dredgings along the African shore, though by no means unproductive, did not furnish any specimens of novel interest. Mr. Gwyn Jeffreys has kindly supplied me with the following List of the more remarkable shells obtained along the Tripoli Coast:—

Brachiopoda. *Platydia anomioïdes*, Scacchi.

Conchifera. *Pleuronectia fenestrata*, Forbes: *Leda pygmæa*, v. Münster: *Arca obliqua*, Philippi: *Lepton nitidum*, Turton: *L. sulcatulum*, Jeffreys: *Kellia? cycladia*, S. Wood: *Axinus transversus**, Bronn: *Astarte sulcata*, Da Costa; var. *elliptica*, Brown: *A. triangularis*, Montagu; and var. margine integro: *Crassatella planata**, Calcara, = *Gouldia modesta*, H. Adams: *Venus effossa*, Bivona: *Pecchiolia granulata**, Seguenza.

Solenoconchia. *Cadulus subfusiformis*, Sars.

Gastropoda. *Scissurella crispata*, Fleming: *Rissoa cimicoïdes*, Forb.: *R. subsoluta*, Aradas: *R. Stefanisi*, Jeffr.: *Odostomia unifasciata*, Forb.: *O. minuta*, A. Adams: *Mesalia pusilla*, Jeffr.: *Cerithiopsis Metaxa*, Delle Chiaje: *Natica fulminea*, Risso: *Solarium discus**, Ph.: *S. Architæ*, Costa: *Trophon vaginatus*, Jan: *Pseudomurex lamellosus*, Jan: *Nassa limata*, Chemnitz; var. *nana*: *Pleurotoma torquata**, Ph.: *P. Renieri**, Sc.: *P. hystrix**, Jan: *Columbella haliæeti*, Jeffr.: *Voluta pumilio*, Brusina (the Marquis de Monterosato has rightly determined this to be the very young of *Cypræa lurida*; the first whorls are exquisitely reticulated, as in *Defrancia*): *Actæon pusillus*, Forb.

Of these, the species marked * are especially interesting, because until lately they were known as fossil only.

80. The Marine Fauna of the Mediterranean, it must be borne in mind, has been carefully explored by numerous Zoologists; and the barrenness of its depths seems to leave little to be added by those explorations which have been so productive elsewhere. True it is that in the 'Porcupine' Expedition of the previous year, a dredging at 1415 fathoms depth brought up several species of Shells,—some of them of considerable interest as having been previously known only in a fossil condition (Report for 1870, § 50). But that dredging was taken in the comparatively narrow extension of the Mediterranean basin between the coasts of Spain and Africa, which is most within the influence of the Gibraltar Currents, and in which, therefore, the stagnation which I believe to exist elsewhere may be somewhat disturbed by their agency. And the fact that among these shells was a freshwater *Planorbis*, was an indication that either by gravitation or by some horizontal travelling of bottom-water (such as might be there produced by the Gibraltar undercurrent), the shells properly inhabiting the littoral zone might have been conveyed thither. And this view of the case is not invalidated by the fact that many of these shells are the representatives of types previously known only as fossils; since the same is true of many species found in shallow water in this extension of the Mediteranean, which seems to have been less explored than most other parts of its shores (Report for 1870, §§ 48, 49).

81. I am disposed to believe, therefore, that in the Mediterranean Basin the existence of Animal life in any abundance at a depth greater than 200 fathoms will be found quite exceptional; and that, without pronouncing its depths to be absolutely *azoic*, we may safely assert them to present a most striking contrast, in respect of Animal life, to those marine Paradises* which we continually met with in the Eastern and Northern Atlantic at depths between 500 and 1200 fathoms. And I have the satisfaction of finding that my conclusion on this point is entirely borne out by the results of the dredgings carried on in the Adriatic by Dr. Oscar Schmidt; who found the like barrenness at depths below 150 fathoms, except as regards *Foraminifera*, *Bathybius*, and *Coccoliths*†. After a most careful microscopic examination of the mud obtained from the depths of the Mediterranean, I feel justified in saying that even of these lowest organisms scarcely any traces are to be found.—Thus it appears that Edward Forbes was quite justified in the conclusion he drew *as regards the particular locality he had investigated*; and that his only mistake lay in supposing that the same conditions would prevail in the open Ocean.

82. I venture to think, however, that I have shown that the Physical conditions of any Inland Sea, which, like the Mediterranean, is cut off from the General Oceanic Circulation, must be such as greatly to modify its relation

* I use this word in the sense familiar to every Greek scholar.

† "On Coccoliths and Rhabdoliths," in Ann. of Nat. Hist., Nov. 1872, from 'Sitzungsbericht der k. k. Akad. der Wissenschaften in Wien,' Bd. lxii. (1870) Abth. i. pp. 669–682.

to Animal life; and that it is a matter of great Scientific importance, especially in relation to Geological inquiry, that these conditions should be carefully examined. The Red Sea would probably be found to present a striking contrast, in many particulars, both to the Mediterranean and to the open Ocean. Its *Thermal* condition, as I have already shown, is altogether peculiar; for while its surface-temperature rises as high as that of any Intertropical portion of the Ocean, that temperature seems to be maintained with very little reduction even to its greatest depths. Now it seems to be the universal opinion of those who have most carefully studied the existing Coral Formations, that the Reef-building Corals do not live and grow at a greater depth than 20 fathoms; and as it is affirmed by Mr. Dana, as a deduction from the Distribution of Coral Formations, that the existence of the reef-builders is *geographically* limited by the isocrymal line of 68° *, I cannot but suspect that the *bathymetrical* limit may be essentially a *thermal* one. For all we know of the relation of Temperature to Depth (§§ 3, 8) would indicate that even within the Intertropical area of the open Ocean, the temperature at 20 fathoms may not be above 68°; and that in the next 10 fathoms it suffers considerable reduction. Now if the temperature of the Red Sea nowhere falls below 71°, it is obviously a most interesting question to determine whether the reef-building Corals are, or are not, to be found in that Sea at a greater depth than in the Oceanic area; and if so, what is the greatest depth at which they there exist. For since our own inquiries show that Stony Corals similar in physiological characters to the reef-building types can live and grow at the depth of *many hundred* fathoms, there seems to me no *à priori* reason why the latter should not thrive at like depths, if the Temperature be congenial to them.

83. The Red Sea further differs essentially from the Mediterranean, in not being the recipient of any great Rivers bringing down *detritus* from the land. This, of course, will affect the condition of the bottom; on which we should not expect to find the abundant sedimentary deposit that is everywhere settling down on the abyssal depths of the Mediterranean. It will also leave the bottom-water clear; and in this respect the condition of the bed of the Red Sea will be more favourable to Animal life than that of the Mediterranean. But the absence of *organic* sediment, if the views previously advanced be correct, will constitute a still more important difference between the conditions of the two Seas, in relation to Animal life: for while its progressive decomposition, in the abyssal water of the Mediterranean, consumes its Oxygen and imparts to it Carbonic acid, at a greater

* On Corals and Coral Formations, p. 108.—It is a very significant fact that the cold current which comes up from the south on the Western coast of South America, and which I regard as the *indraught* of the Pacific Equatorial Current (as the similar current on the Western coast of South Africa is of the Atlantic Equatorial Current, § 120), pushes the southern Isocryme of 68°—the Coral-Sea boundary—to the north of the Equator, between the South American coast and the Galapagos, which, though under the Equator, lie outside of that boundary.

rate than "diffusion" can counterbalance without any vertical circulation in the water itself, and thus tends to render the depths of that Sea uninhabitable, the absence of the like source of impurity in the water of the Red Sea may be expected to leave its abyssal water in a condition fit to support a moderate amount of Animal life; since the process of "diffusion," even without a vertical circulation, will maintain a certain amount of interchange of gases between the superficial and the deep strata.

84. These views are *suggested* merely as fair inferences from our present very limited knowledge, to be confirmed or set aside by the results of future inquiries. I have experienced, in the course of the researches in which it has been my privilege to be engaged, so much advantage from the *clues* which have offered themselves from time to time for their guidance, that I should consider myself wanting in my duty to those who may hereafter take up the same line of investigation, if I were to be kept back from making such suggestions, by any apprehension that my personal credit would suffer from having propounded ideas which subsequent research did not bear out.—"Truth," it has been well said, "emerges out of Error, rather than out of Chaos;" and the history of Science is full of instances in which erroneous doctrines have been more *productive*, because more *suggestive*, than well-determined facts that open no access to the Unknown beyond.

Addenda.

As this sheet is passing through the press, I have been kindly supplied by Mr. Scott, Director of the Meteorological Department, with the following extract from a letter received from Prof. Mohn, Director of the Meteorological Institute at Christiania, giving an account of some observations recently made by him during his voyage of inspection of the Meteorological Stations on the Norwegian coast:—

"On my voyage I have taken the opportunity of making deep-sea temperature observations in the Throndhjem Fjord and the Sogne Fjord. I found exactly what I expected. You know our Fjords are deep in their middle part, and shallower at their mouth. In the deep part of the fjords the lower part of the water—say from 40 fathoms downwards—is of a constant temperature of 6° to 6°·5 Cent. [43° to 44° Fahr.]. So I found in the Sogne Fjord, 16° [61° Fahr.] on the surface, 6°·4 in 10 fathoms, and not lower than 6°·5 in different depths down to 700 fathoms. Of course the Casella-Miller's thermometer was used. This agrees with the results found by myself in the Throndhjem Fjord, and with those found in the Hardanger Fjord by Prof. Seue, and by the survey-steamer 'Hansteen' (?), Capt. Wylle. In those two fjords the constant temperature commenced in a depth of about 40–50 fathoms, and was six degrees and some tenths Centigrade. Lieutenant A. W. Müller, the Commander of the steamer running between Bergen and Iceland, has found this summer, by

several observations taken at different times, a temperature of about +8° Cent. [44° Fahr.] on the bottom, in a depth of 300 fathoms, between Iceland and the Faroe Islands. What a contrast to the distribution of temperature in the Channel between the Faroe Islands and Shetland!"— It is very interesting thus to find the deep Norwegian Fjords showing the same phenomenon of uniform temperature as other deep land-locked Seas. In my Report for 1869, I pointed out (§ 104) that the comparative shallowness of the bottom between Iceland and the Faroe Islands would interpose an effectual barrier to any glacial current moving southwards at a depth exceeding 300 fathoms.

The Hydrographer to the Admiralty has been good enough to furnish me with the following very interesting series of Temperatures recently taken in the Eastern Seas by Capt. Chimmo with the "protected" Thermometers.—As only a single Thermometer was used in each observation, it seems probable that the Temperatures marked (?) may be more or less incorrect.

Station...	A.	B.	C.	D.	E.	F.
	°Fahr.	° Fahr.	° Fahr.	° Fahr.	° Fahr.	° Fahr.
Surface ...	85·0	81·0	82·8	82·5	84·0	82·5
10 Fath.	83·2	......	82·8	82·0	......	81·5
40	......	......	82·2			
50	82·8	......	......	......	71·5	
70	......	......	68·8			
100	58·5	......	......	......	62·0	65·0
150	......	......	54·2			
200	......	......	......	54·0	51·0	56·2
250	......	......	50·5			
300	......	......	......	......	46·0	
350	......	......	......	......	42·5	
400	......	......	......	40·0		
450	......	......	......	......	41·5	
550	......	......	......	40·7(?)	37·0	
750	......	......	40·2	......	37·5	
800	......	......	......	39·0	......	54·0
850	......	......	......	......	37·0	
900	43·0	......	......	......	37·0	
1100	......	......	......	......	......	50·0
1300	......	......	36·9	43 (?)		
1400	42·8					
1778	......	......	......	......	......	50·0
1800	......	......	35·2			
1900	33·0(?)					
2270	......	34·8				
2306	34·5	......	33·6			
2656	......	......	32·0			
2667	......	......	......	38·5		

Indian Ocean, between Ceylon and Sumatra.
Station A. Lat. 0° 35′ N., Long. 89° 26′ E.
Station B. Lat. 3° 24′ N., Long. 84° 44′ E.
Station C. Lat. 3° 18′ S., Long. 95° 39′ E.

Celebes Sea.
Station D. Lat. 4° 14′ N., Long. 123° 9′ E.
The comparatively high bottom Temperature of this Sea, notwithstanding its great depth, is exactly what might be expected from its *partially* land-locked position.

China Sea, near Paracel's Islands.
Station E. Lat. 17° 50′ N., Long. 111° 24′ E.

Sulu Sea.
Station F. Lat. 8° 5′ N., Long. 119° 43′E.

APPENDIX.

I. On the Gulf-stream, in relation to the General Oceanic Circulation.

85. The question how far the Climate of Northern Europe, and the temperature of the Arctic Sea between Iceland and Nova Zembla, is modified by the Gulf-stream, has of late been much discussed among Physical Geographers; and opinions the most contrary have been expressed on the subject. For whilst it is maintained by some that there is ample evidence of the amelioration of that Climate by a North-easterly *stream of Oceanic* water, bearing with it the warmth of a more Southern latitude, and further, that this flow is essentially a prolongation of the Florida Current or Gulf-stream proper, still bearing with it a large measure of the initial force which it derived from the Trade-winds, it is asserted by others that the Gulf-stream proper dies out in the Mid-Atlantic, shortly after encountering the Arctic Current off the Banks of Newfoundland; and that it consequently exerts no other influence on the Climate of Northern Europe and on the temperature of the Arctic Sea, than as contributing to the warmth of the South-westerly winds which blow from the area traversed by the Gulf-stream towards the British Islands and the Scandinavian Peninsula, and, by their prevalence over all others, maintain a North-easterly *surface-drift* over the portion of the North Atlantic which washes their shores.

86. Now since the doctrine of a General Oceanic Circulation dependent on difference of Temperature, which I have advocated in my successive Reports, will, if admitted, reconcile these opposing views,—by giving a *vera causa* for that *deep* flow of water from the warmer area of the North Atlantic towards the Polar Sea which Thermometric observations unmistakably indicate; whilst it accepts as of no less value the conclusions of those who, after a careful study of the Florida Current, are satisfied of its entire inadequacy to perform the function assigned to it,—I think it worth while to develope this doctrine somewhat more fully with special reference to the question of the Gulf-stream; more especially since my views have been strangely misunderstood by some of those who have taken prominent parts in the controversy.—In order to place this question fully before the Scientific public, I shall briefly summarize the opinions which have of late years found advocates among men whose knowledge of the subject entitles their views to respectful consideration.

87. The doctrine of the Gulf-stream prevalent thirty years ago was thus unhesitatingly expressed by Humboldt in the first volume of his 'Kosmos' (1844):—"It pours itself from the Caribbean Sea and the Mexican Gulf "through the channel of the Bahamas, and, following a direction from "S.S.W. to N.N.E., deviates more and more from the coast of the United

"States; until, deflected still further to the east by the banks of New-"foundland, it crosses the Atlantic, and casts an abundance of Tropical "seeds on the coasts of Ireland, of the Hebrides, and of Norway. Its "north-easternmost prolongation mitigates the cold of the ocean, and exer-"cises a beneficent influence on the climate of the northernmost point of "Scandinavia. At the point where the stream is deflected to the east by "the banks of Newfoundland, it sends off an arm toward the south, not "far from the Azores." (Sabine's Translation, vol. i. p. 301.)

88. General Sabine appears to have been the first to suggest, in a valuable Note to the passage just cited (p. 454), that the transport of Tropical products, as well as of the warmer water of the Mid-Atlantic, to the North Cape, may be due to other agencies than the propulsive force of the Florida Current.—"It appears to require a further investigation to decide whether "the Stream-current referred to in the text, which flows along the coast of "Norway and round the North Cape of Europe, and is, at least, mainly "supplied from the accumulated waters of the drift propelled by the West "and South-west winds which prevail to the northward of the Trades, de-"rives any portion whatsoever of its force from the original impulse given to "the waters of the Gulf-stream at its outlet from the Gulf of Mexico, in the "Bahama Channel. The transport of West-Indian seeds to the coast of "Norway is undoubted; and even parts of the cargoes of vessels wrecked on "the coast of Africa have reached the Norwegian coast, after having made the "circuit of the West-Indian Islands:—[such an instance occurred when the "Editor was at Hammerfest, near the North Cape of Europe, in 1823; casks "of palm-oil were thrown on shore, belonging to a vessel which had been "wrecked at Cape Lopez, on the African coast, near the Equator, under "circumstances which had made her loss a subject of discussion when the "Editor was in that quarter of the globe, the year preceding his visit to "Hammerfest]. But it is quite conceivable that objects conveyed a certain "distance by the Gulf-stream, and thrown off on its north side into the "waters which do not participate in its movement, may be subsequently "drifted by the prevailing Westerly and South-westerly winds, in accom-"paniment with the surface-water of the sea, across the remaining portion "of the Atlantic. The stream-current which terminates in ordinary years "at the Azores, and which in rare instances extends to the coasts of Europe, "is unquestionably traceable the whole way back to the outlet of the Gulf "of Mexico, by a continuous strength of current and warmth of water; but "with respect to a northern branch of the Gulf-stream, supposed to detach "itself to the N.E., and to convey the waters which have issued through "the Bahama Channel in a continuous stream to the North Cape of Europe, "*positive information is greatly wanting.*"

89. The elaborate investigations of Prof. Dove on the Temperature of the Earth, embodied in the Isothermal charts which he published in 1847–48, afforded strong confirmation of the doctrine that a N.E. movement of Atlantic water serves to carry a large amount of heat from the Temperate zone

within the Arctic area; and he inferred, from the direction of the Isothermal curves, that the influence of the Gulf-stream extends as far as Nova Zembla*.

90. The doctrine of the Gulf-stream as the efficient *heater* of North-western Europe, and of the Sea that stretches from it towards the Pole, found an enthusiastic and eloquent advocate in Capt. Maury. "There is," he says, "a river in the ocean: in the severest droughts it never fails, and in the "mightiest floods it never overflows; its banks and its bottom are of cold "water, while its current is of warm; it takes its rise in the Gulf of Mexico, "and empties into Arctic Seas: this mighty river is the Gulf-stream,"—which, he affirms, discharges over the Atlantic in a winter's day a quantity of heat "sufficient to raise mountains of iron from zero to the melting-point, and "to keep in flow from them a molten stream of metal greater in volume than "the waters daily discharged through the Mississippi river;" or as "sufficient to raise the whole column of atmosphere that rests upon France and "the British Islands from the freezing-point to summer-heat."—Capt. Maury, however, contests the doctrine originated by Dr. Franklin and supported by Rennel, as to the propulsive force by which the Gulf-stream is sustained: for instead of attributing it, like his predecessors, to the action of the Trade-winds in driving the Equatorial Current into the Gulf of Mexico, where, by the narrowness of its channel of exit it is converted from a superficial *drift*-current into a deep *stream*-current, he assigns it to a combination of other agencies, the operation of which, however, he rather indicates than distinctly specifies. Thus, he says, "modern investigations "seem to encourage the opinion that this stream, as well as all the *constant* "currents of the sea, is due *mainly* to the *constant* difference produced by "Temperature and Saltness in the Specific Gravity of water in certain parts "of the ocean. Such difference in specific gravity is inconsistent with "aqueous equilibrium; and to maintain this equilibrium these great cur-"rents are set in motion. The agents which derange equilibrium in the "waters of the sea, by altering specific gravity, reach from the Equator to "the Poles; and in their operations they are as ceaseless as heat and cold; "consequently they call for a system of perpetual currents to undo their "perpetual work." Again,—"As for the seat of the forces which put and "keep the Gulf-stream in motion, theorists may place them *exclusively* "on one side of the Ocean with as much philosophical propriety as on the "other. Its waters find their way into the North Sea and the Arctic "Ocean in virtue of their specific gravity; while water thence, to take "their place, is, by virtue of its specific gravity and by counter-currents, "carried back into the Gulf. The dynamical force which causes the Gulf-"stream may therefore be said to reside both in the polar and in the inter-"tropical waters of the Atlantic."

91. The whole of Capt. Maury's reasoning on this point, however, is

* "Ueber Linien gleicher Monatswärme," in Berlin Abhandl. (Phys.) 1848, p. 209; and British Association Report for 1848, p. 84.

directed to prove that the various causes which produced disturbance of equilibrium (among which he enumerates the abstraction of Carbonate of Lime from the Ocean-water by the Coral-forming polypes) are adequate to sustain *sensible currents*, such as the Gulf-stream. "A constantly acting "power," he says, "such as the force of gravitation, is as necessary to keep "fluids as it is to keep solids in motion. In either case the projectile force "is soon overcome by resistance; and unless it be renewed, the current in "the sea will cease to flow onward, as surely as a cannon-ball will stop its "flight through the air when its force is spent. . . . . A propelling power, "having its seat only in the Gulf of Mexico, or the trade-wind region, "could no more drive a jet of water across the ocean, than any other single "impulse could send any other trajectile that distance through either air "or water. The power that conveys the waters of the Gulf-stream across "the ocean is acting upon them every moment, like gravity upon the "current of the Mississippi river; with this difference, however, the Mis-"sissippi runs down hill, the Gulf-stream on the dead level of the sea. "But if we appeal to salt and vapour, to heat and cold, and to the secreting "powers of the insects of the sea, we shall find just such sources of ever-"lasting changes and just such constantly acting forces as are required to "keep up and sustain, not only the Gulf-stream, but the endless round of "currents in the sea, which run from the equator to the poles, and from "the poles back to the equator; and these forces are derived from differ-"ence in specific gravity between the flowing and reflowing water. The "waters of the Gulf as they go from their fountain have their Specific "Gravity in a state of perpetual alteration in consequence of the change of "Saltness, and in consequence also of the change of Temperature. In these "changes, and not in the Trade-winds, resides the power which makes the "great currents of the sea." (Physical Geography of the Sea, Chapter II.)

92. The doctrine of Capt. Maury was powerfully and convincingly opposed by Sir John Herschel; who showed, beyond all reasonable doubt, *first*, that the Gulf-stream really has its origin in the propulsive force of the Trade-winds, and *secondly*, that the greatest disturbance of equilibrium which can be supposed to result from the agencies invoked by Capt. Maury would be utterly inadequate to generate and maintain either the Gulf-stream or any other sensible current (Physical Geography, §§ 51–60). But Sir John Herschel did *not* regard the initial force of the Gulf-stream as adequate to carry it across the Atlantic Ocean, still less to propel it as far as the Arctic Sea; for he speaks of it as *dispersed, and in fact destroyed*, by the process of thinning-off and superficial extension, about the 42nd or 43rd parallel of N. Latitude; and regards the N.E. flow of warm water towards the opening of the Arctic Sea between Spizbergen and Norway as mainly a drift-current swept on by the south-west Anti-trades of extra-tropical latitudes. The Polar current which comes down partly from Baffin's Bay along the Labrador coast, and partly from the east side of Greenland, meeting the Gulf-stream on the Banks of Newfoundland,

and thence passing southwards between the Gulf-stream and the Coast of the United States, is regarded by Sir John Herschel as the return-current of that portion of the N.E. Drift which passes onwards into the Arctic basin, completing what (borrowing a term from Anatomy) I would call the *longer circulation.* On the other hand, the southern deflection of a large portion of the Gulf-stream to the east of the Azores, and its re-entrance into the Equatorial current, complete the *shorter circulation.*

93. As Sir John Herschel, when placing himself in antagonism to the doctrine of Oceanic Circulation propounded by Capt. Maury, was himself an adherent of the doctrine of the uniform Deep-sea temperature of 39°, which had been propounded by Sir James Ross, on the basis of observations now proved to have been vitiated by the imperfection of his Thermometers, his repudiation of Capt. Maury's views must be understood as extending only to the question of *sensible currents.* As to the existence of any *more general,* though less perceptible, *surface*-movement of Oceanic water from the Temperate towards the Polar seas, Sir J. Herschel offered no opinion; nor did he at that time (so far as I am aware) anywhere express himself as either favouring or opposing the hypothesis of a movement of *deep* water from the Polar to the Equatorial areas, which would seem necessary for the maintenance of a bottom-temperature even of 39° in the latter, much more of a bottom-temperature but little above 32°*.

* As the doctrine advocated in my previous Reports, of a General Oceanic Circulation sustained by difference of Specific Gravity arising out of difference of Temperature, differs essentially from that of Capt. Maury,—in that it leaves unquestioned the Trade-wind origin of the Gulf-stream, and touches only that *deep* N.E. movement of Warm water which the Gulf-stream (on Sir J. Herschel's own showing) could not produce, and that *still deeper* return-movement of Cold water of which Sir J. Herschel was then ignorant,—I cannot but feel surprised at finding his authority cited against me, not only by Mr. Croll, but also (inferentially at least) by Sir Charles Lyell; who, in the recently published Eleventh Edition of his 'Principles of Geology,' speaks as follows (vol. i. p. 505):—"Besides those sensible currents arising from the various "causes already mentioned, a theory of general oceanic circulation first propounded by "Maury has lately been brought into great prominence by Dr. Carpenter." Now, as I have shown, the "theory of general oceanic circulation propounded by Maury," had special reference to *sensible currents*; and it was the inadequacy of the causes assigned by him (not by me) for their production, which was the subject of Sir John Herschel's demonstration, cited by Sir C. Lyell in a subsequent page (p. 507). Yet by representing my views as identical with Capt. Maury's, Sir C. Lyell has (perhaps unintentionally) given the impression that Sir J. Herschel's demonstration was equally applicable to both. And he has been so understood by an anonymous writer in the 'Athenæum' (March 2, 1872); who, in a notice of Sir Charles Lyell's new edition, asserts, with reference to "the question of Ocean-Currents, which has been lately brought very pro- "minently forward by Dr. Carpenter," that "Sir Charles shows that the theory which "refers oceanic circulation to difference of specific gravity is founded upon erroneous "observation and incorrect application of the facts observed."—How differently Sir John Herschel thought of the doctrine as advocated by me (not *in substitution* for the Trade-wind origin of the Gulf-stream, but as *supplemental* to the whole Oceanic

94. The general acceptance of the doctrine of the uniform Deep-Sea Temperature of 39°, based on the observations of Sir James Ross, and sanctioned by the authority of Sir J. Herschel, caused the earlier doctrine of Humboldt and Pouillet (§ 18) to be lost sight of, until it was brought again into notice by the more trustworthy observations recently made with Thermometers 'protected' against the effects of pressure (§ 10).

95. The views of Sir John Herschel and others in regard to the non-extension of the Gulf-stream *as such* into the Polar area, were warmly contested twenty years ago by Dr. Petermann; who, in a letter addressed to Sir Francis Beaufort (then Hydrographer to the Admiralty *), explicitly asserted, on the basis of Temperature-observations, that the influence of the Gulf-stream extends from Newfoundland along the north coast of Europe and Asia, as far as Cape Jakan, in the vicinity of Behring's Strait. And this doctrine he has since urged in several successive Memoirs in the 'Geographische Mittheilungen;' with the addition that he now regards as a branch of the Gulf-stream a current of warm water which passes up the east side of Davis's Straits, and which is traceable as far as Smith's Sound.—But, as will presently appear (§ 106), Dr. Petermann uses the term "Gulf-stream" in a sense very different from that in which it is ordinarily employed in this country.

96. On the other hand, Mr. A. G. Findlay, in a Paper on the Gulf-stream presented to the Royal Geographical Society, Feb. 8, 1869, essayed to prove—on the basis of the data supplied by the operations of the United States Coast Survey as to the rate, volume, and temperature of the Gulf-stream in its passage through the Florida Channel, and by the changes in these three conditions which it undergoes during its course as far as the Banks of Newfoundland,—that after encountering the Arctic Current on the Banks of Newfoundland, the Gulf-stream loses all its original characters, and can no longer be distinguished from the general North-easterly drift of the North Atlantic; and that our mild climate, being in no way dependent on its influence, is to be accounted for as follows:—"The great "belt of Anti-trade or passage winds which surround the globe northward "of the Tropics, passing to the north-eastward, or from some point to the "southward of west, passes over the entire area of the North Atlantic, and "drifts the whole surface of that ocean towards the shores of Northern "Europe and into the Arctic basin, infusing into high latitudes the temperature and moisture of much lower parallels; and this alone would be "sufficient to account for all changes in climate by their variations, without any reference whatever to the Gulf-stream."

97. The views of Mr. Findlay as to the limited agency of the Gulf-

Current system kept up by the action of Winds on the surface), has already been shown (§ 37).

* "Further Correspondence and Proceedings connected with the Arctic Expedition," Parliamentary Blue Book, 1852, pp. 142 *et seq.*

stream proper have found much support in the United States, from men eminently qualified, by their knowledge of its Hydrographical phenomena, to estimate its real influence on the temperature of the North-east extension of the Atlantic. Thus Mr. G. W. Blunt, "the head of the well-known "house that has for nearly a century prepared and published the charts "that have guided the American Mariner in every quarter of the globe," says in a letter to the President of the American Geographical and Statistical Society,—"Beyond the Western Islands, I believe the Gulf-stream "has no existence; and its alleged effects on the climate of the British "Islands are matters of assertion only. The Gulf-stream, as a current, I "believe, entirely ceases and loses all its equatorial heat to the eastward of "W. Long. 40°; the set to the east is that of the general set of the North "Atlantic, and the temperature of the water is that of the general tempe-"rature in those regions" *. And Dr. Hayes, the Arctic explorer, who formerly participated in the prevalent belief as to the extended agency of the Gulf-stream, has recently been so completely convinced by Mr. Blunt's representations, that he has written an elaborate article on "The Real Gulf-stream" †, for the purpose of correcting the prevalent idea that the Gulf-stream influences the temperature of the North Atlantic. "It "is an error," he says, "into which many persons (including the writer "of this article) have been led through lack of familiarity with facts, "to suppose that the Gulf-stream sends off a branch from its northern "limit to the Arctic Ocean, or even, as we shall presently have occasion "to show, that it touches any part of the coasts of Europe." . . . . . "Being a resultant current, and having no force applied to it to keep it in "motion, its strength diminishes; the air of a higher latitude brings its "temperature down to that of the North Atlantic generally; the water "loses all its Gulf-stream character, as to course, warmth, and flow; and "it dies away into the sluggish Atlantic drift which sets from a westerly to "an easterly direction."—Dr. Hayes adopts the view of Mr. Findlay, that the amelioration of the climate of the British Isles and of regions further north is mainly due, as Dr. Stork had previously maintained (Nautical Magazine, 1857), to the prevalence of South-westerly winds; and he affirms, with Dr. Stork, that the temperature of the sea rather *follows* than *controls* that of the superincumbent air.

98. On the other hand, the doctrine of Dr. Petermann finds an enthusiastic advocate in Capt. Silas Bent ‡; who not only maintains that the thermal influence of the Gulf-stream is traceable to Spitzbergen and Nova Zembla, but attributes to its agency the whole difference between the

* Journal of the American Geographical and Statistical Society, vol. ii. (1870) part 2, pp. cvi *et seq.*

† Harper's Atlantic Monthly, Jan. 1872.

‡ Addresses on "The Thermometric Gateways to the Pole," delivered before the Historical Society of St. Louis (U. S.), 1869, and before the St. Louis Mercantile Library Association, Jan. 6, 1872.

winter climate of Naples and that of New York,—even going so far as to affirm that the Gibraltar Current is a branch of the Gulf-stream carrying its heat into the Mediterranean, although Thermometric evidence shows that the Gibraltar Current rather *reduces*, than raises, the temperature of that inland sea.

99. The doctrine of the extension of the Gulf-stream proper to the Polar area, carrying with it a vast amount of Equatorial heat, has been advocated with great ability by Mr. James Croll; who, employing the modern method of computing *units of heat*, essays to prove that the quantity of heat carried from the Equatorial area by the Gulf-stream is so enormous, as to be competent not only to do all that Dr. Petermann attributes to it, but a great deal more*. In this, indeed, he goes far beyond Dr. Petermann himself; for, as will presently appear, Dr. Petermann regards the Florida Stream as furnishing only a part of the thermal power exerted by the vast body of water derived from various sources, to which he gives the name "Gulf-stream."

100. Without attempting to follow Mr. Croll through his calculations, I may stop to point out what appear to me to be the fallacies of his method; since if this can be proved erroneous, Mr. Croll's great array of figures is utterly valueless.

101. In the *first* place, in Mr. Croll's preliminary comparison of the Temperatures of the Northern and Southern Hemispheres, he altogether ignores the influence on the Distribution of Heat over the Globe which is exerted by the great relative preponderance of Land in the Northern Hemisphere. Yet of the vast importance of this influence, no Meteorologist or Physical Geographer can entertain the smallest doubt. The manner in which it affects the relative Temperatures of the North and South Atlantic will be hereafter shown (§ 118) to be precisely conformable to that in which it manifests itself in the Climates of Continental Stations; and to affirm, as Mr. Croll does, that "the lower mean Temperature of the Southern Hemi-"sphere is due to the amount of heat transferred over from that Hemi-"sphere to the Northern by Ocean-Currents," is to repudiate all that has been established by the researches of Meteorologists, as to the relative effects of Land and Sea, not only upon Temperature, but upon Atmospheric Vapour, Barometric Pressure, and the prevalent Direction of winds,—in all of which particulars the contrast between the Northern and Southern Hemispheres is so marked, that any transfer of Heat from the latter to the former which can be fairly attributed to Ocean-currents, must be comparatively insignificant in its effects.

102. Secondly, in computing the Heat imparted by the Sun to the Equatorial area from which the Gulf-stream is fed, Mr. Croll assumes

* See his Papers "On Ocean-currents," parts I. and III., in Philosophical Magazine, Feb. and Oct. 1870; and "On the Influence of the Gulf-stream," in Geological Magazine, April 1869.

that this heat, being wholly taken up by the Water of the Ocean, is transferred by its currents towards the Polar regions; whilst of the heat which falls upon the Land, a very large proportion is lost by radiation, passing off into the stellar spaces. "It is in the Equatorial regions," he says, "that the *earth* loses as well as gains the greater part of its heat; so "that, of all places, here ought to be placed the substance best adapted "for preventing the dissipation of the earth's heat into space, in order "to raise the general temperature of the earth. *Water*, of all substances "in nature, seems to possess this quality to the greatest extent; and, besides, "it is a fluid, and therefore adapted by means of currents to carry the heat "which it receives from the sun to every region of the globe"*. Now in this assumption two facts are entirely ignored:—*first*, the very small depth to which the superheating influence of direct insolation penetrates, as is shown in the temperature-soundings taken during my two Mediterranean cruises (see § 67); and *second*, the conversion of a vast amount of the solar heat which falls upon the Ocean into the elastic force of vapour, its surface-temperature being *kept down* to a tolerably regular maximum by evaporation, just as the surface-temperature of the Human body is kept down to a uniform maximum by its insensible perspiration. The *maximum* surface-temperature ever observed in the open Ocean is never (so far as I have been able to ascertain) above 86°, the ordinary maximum being about 82°; and any higher temperature seems only to show itself in the near neighbourhood of land,—as along the Guinea Coast, and in the Red Sea, where a temperature of 90° is said to have been noted. Yet the direct heat of radiation, measured by a thermometer with a blackened bulb, laid upon a black surface, has been seen at Aden (as I am informed by Col. Playfair) to raise the mercury to 215°. The enormous amount of heat thus converted into the elastic force of vapour must be carried into the upper regions of the atmosphere; and whether that vapour be there condensed into the heavy rain which falls in the region of equatorial calms, or be transported by atmospheric currents to some remote region, there to undergo condensation, the heat thus lost by *evaporation* from the *sea* must be far greater than that lost by *radiation* from the *land*.

103. In the third place, Mr. Croll leaves almost entirely out of the question the N.E. transportation of an enormous amount of heat from the *general surface of the Atlantic* by the agency of the *Aqueous Vapour* thus raised; although the importance of this agency has been insisted on by the most eminent authorities in Meteorology. "Thus," says Sir John Herschel (Meteorology), "aqueous vapour becomes an agent in the transfer "of heat, in its latent state, from one part of the globe or from one region "of the atmosphere to another." And Sir Charles Lyell, after citing this passage, continues:—"The upper trade-winds (or Anti-trades), which

* "On Ocean-currents in relation to the Distribution of Heat over the Globe," in Philosophical Magazine, Feb. 1870, p. 86.

"pass freely above the peaks of all but the highest mountains, are able "to pursue an almost unbroken course over tropical and subtropical "latitudes, until they come to those more northern regions where the "act of condensation releases probably three-fourths of their heat"*.—But, says Mr. Croll, "the heat of our south-west winds is derived, not "directly from the Equator, but from the warm water of the Atlantic—"in fact from the Gulf-stream . . . . . The greater part of the moisture of "the south-west and west winds is derived from the ocean in temperate "regions . . . The greater part of the moisture received at the Equator is "condensed and falls as rain in those regions." Now to any one who examines the proportion borne by the area of the true Gulf-stream, before its easterly bend, to that of the great Sargasso sea of the Mid-Atlantic, which has a temperature only a few degrees lower than that of the Florida Current in corresponding latitudes, and in no way dependent upon it, the assertion that the warmth and moisture of our S.W. winds is essentially dependent on the Gulf-stream, is obviously untenable. So, also, is the statement that moisture cannot be thus conveyed from Intertropical to Temperate regions, because the heated air charged with it *must* ascend at once into regions so cold that all its moisture will be condensed: for if this were the case, the enormous mass of water raised by evaporation from the surface of the Red Sea (calculated by Dr. Buist as equal to *eight feet* of depth per annum) ought to fall back again into the Red Sea or the area immediately surrounding it; and yet, as is well known, the greater part of that area is almost entirely *rainless*.

104. Fourthly, until corrected by Mr. Findlay, Mr. Croll assumed that the whole of the true Gulf-stream continues to flow in a N.E. direction; whereas it is unquestionable that a considerable proportion of it (probably more than one half) turns southward to the east of the Azores, and re-enters the Equatorial current. This portion, however, Mr. Croll regards as the principal *heater* of the S.W. winds which impart warmth to the British Islands; whilst the portion of the current which he assumes to flow past our Islands up into the Arctic seas, he regards as the *heater* of the S.W. winds which go to warm Norway and the North of Europe.—Now the fact is, as I shall show hereafter (§ 147), that the large division of the Gulf-stream which thus turns *southwards* possesses no excess of temperature above the general mass of Atlantic water in that region, being only recognizable by its movement. And as to the portion which passes *northwards*, we shall see that its Area is so small in relation to the general surface of the Atlantic through which it passes, and that its Temperature is so little in excess of that of the water on either side of it, as to render its Thermal effect very insignificant (§§ 150, 157).

105. The difference between Mr. Croll's representation of the amount of Heat actually carried out by the Gulf-stream, and the actual facts of

* 'Principles of Geology,' Eleventh Edition, vol. i. p. 238.

the case, will be more fittingly pointed out hereafter (§§ 139–142). At present I venture to think that I have made it evident that the real influence of the Gulf-stream upon the Thermal condition of the North Atlantic must be determined in some other way than by a theoretical computation of "units of heat," for which no truly reliable data can be said to exist. It is from *the position and course of the Isothermal Lines*, which have now been laid down over its whole area for different seasons on the basis of a large number of observations, that we derive our surest information as to the actual effect produced on the surface-temperature of the Mid-Atlantic by the transfer of water in the Gulf-stream from the Intertropical to the Temperate zone, and as to the course and amount of the further movement whereby the water of the Temperate zone is carried towards the Polar area.—This is the method of investigation which has been followed by Dr. Petermann in his recent very important Memoir on "The Gulf-stream, and our knowledge of the Thermal condition of the North Atlantic Ocean and its Continental Borders, up to 1870" *; and it will be found that however much I may seem to differ from him in refusing to the "Gulf-stream" any important share in the amelioration of the Climate of the British Islands, and of the regions lying to the north of them, the difference is more verbal than real.

106. Dr. Petermann's view will be best expressed in his own words:—

"My own ideas, in 1865, of the extent and of the immense volume of "the Gulf-stream, I expressed as follows (Mittheilungen, pp. 150 *et* "*seq.*):—'Instead of a weak and insignificant drift from Newfoundland "'toward Europe, as heretofore represented, I consider the northern part "'of the Gulf-stream one of the mightiest currents in the world, although "'comparatively but slow, not very perceptible on the surface of the "'ocean, and therefore of no great moment to navigation. I do so, be-"'cause Ocean-Currents have to perform yet other functions than those of "'a strong surface-stream. In that view I conceive the Gulf-stream "'to be a *deep permanently warm current* from Newfoundland to the "'coasts of France, Great Britain, Scandinavia, and Iceland, up to Bear "'Island, Jan Mayen, and Spitzbergen; and along the western coast of "'the latter up to the 80th degree of N. Lat., thence to Nova Zembla into "'the Polar Sea, passing the northernmost capes of Siberia and the New "'Siberian Islands, where it appears on the charts as the Polynja of the "'Russians, discovered by Hedenström sixty years ago, and fully corro-"'borated by Wrangell and Anjou, its influence being felt perceptibly

* 'Geographische Mittheilungen,' 1870, pp. 201–272.—A translation of this Memoir, with supplemental Notes, and other subsequent Papers in the 'Mittheilungen,' has been issued by the Hydrographic Office of the United States; and of this translation I have freely availed myself,—referring, however, to the original wherever I had reason to suspect that the meaning was inaccurately or imperfectly expressed.

" 'even as far east as Cape Jakan in the vicinity of Behring's Strait.'— " In two Monographs since published ('The Northernmost Land of the " Globe,' and 'The Expeditions to the Arctic Ocean north of Behring " Strait, 1648 to 1867'), I have traced a branch of the Gulf-stream up " to Smith's Sound, and another warm current from the Pacific Ocean " through Behring's Strait up to the Polar Land discovered by Kellett " and Long.—Although, since 1865, high authorities have pronounced " against my theory of the extent and volume of the Gulf-stream, I " cannot but still maintain the same; and shall now produce the figures " of the actual observations on which it is based, and without which we " but drift into arbitrary suppositions."

107. With reference to the objections advanced by Mr. Findlay, and by those who maintain the same opinion with him as to the limited agency of the *real* Gulf-stream, Dr. Petermann says:—"That the Gulf-stream, " in its course towards Europe, receives and unites with a drift corre- " sponding in direction, is probable and natural; but it is equally cer- " tain that the former is the main body or principal stream of the North " Atlantic waters at all times of the year. Of continental rivers the ex- " pression is used that they have their source or head and their mouth; " but this does not imply that all the water emptying at the mouth comes " exclusively from the small source. The Florida Stream may be com- " pared with the head of a great river, which is swelled, in its course to " the mouth, by tributaries. In the same manner, then, as a great river- " basin is named from the principal river, it appears proper and to the " purpose to retain for the warm North Atlantic current the name 'Gulf- " stream.' It surely would be difficult to ascertain where the Florida " Stream ceases really; where it receives tributaries, and how many; and " what part of the temperature of the combined stream is ascribable to the " Florida Stream, and what to the tributaries. The name 'Gulf-stream' " has been adopted so generally for the great Oceanic current which bathes " the European shores, that it would be better to call the head and the " first part of the course 'Florida Stream,' than to use for the other " part a new and complicated name, instead of one long known and now in " general use."—This view has lately received the sanction of Sir Charles Lyell:—" It has been objected that we have no right to attribute to the in- " fluence of the Gulf-stream the warmth of all the water which we may find " in the Northern Atlantic above the normal temperature of the latitude. " But when recognizing the influence of that stream in the Atlantic, we do " not, as Dr. Petermann justly observes, refer the whole of it to the Current " which flows out of the Gulf of Mexico, or deny that it has received " accessions upon its way: we rather retain the name of Gulf-stream just " as we do that of a river from its source to its delta, although many tri- " butaries coming from different regions may have swollen and modified " its volume"*.

* 'Principles of Geology,' Eleventh Edition, vol. i. p. 504.

108. I cannot but regret that an analogy so fallacious should have been put forth by a Physical Geographer of Dr. Petermann's reputation, and should have been adopted by the distinguished author of the 'Principles of Geology.' A River-current (provided it has an adequate fall) continually *increases* in strength and volume by the accession of water brought into it by its tributaries; and the retention of the same name from its principal source to its mouth is therefore perfectly appropriate. But an Oceanic Stream-current, maintained by its initial force alone, tends gradually to *diminish* in strength, through the retardation it is constantly undergoing by friction, until at last it merges in the general body of water through which it flows. And if that water has a movement of its own, quite independent of any slight residual propulsive force which it may receive from the current discharged into it, nothing but confusion can arise from assigning to it a name which implies that it is as much the continuation of that Stream, as a river at its mouth is of a river at its source.—Of the misapprehension liable to arise from such an inappropriate use of names, I shall now give a notable instance.

109. Dr. Petermann, after quoting from my Lecture (delivered at the Royal Institution, February 11, 1870), on the results of the first 'Porcupine' Expedition, a passage in which I express a doubt whether the influence of the "Gulf-stream proper" reaches the European shores of the Atlantic, and citing Mr. Jeffreys as to the evidence afforded by the Arctic character of the Mollusca of this part of the Ocean in disproof of the extension of the Gulf-stream to the coasts of Ireland and Scotland, continues as follows:—"The upper warm stratum of the North Atlantic Ocean, between "Ireland and the Faroes, was computed, from the soundings of the "'Lightning' and the 'Porcupine' Expeditions, to have a depth of not "less than 700 to 800 fathoms, which, of course, excludes the idea of a "drift or surface-current, and most strongly fortifies the theory of *a deep,* "*voluminous, warm stream.* Therefore Carpenter and Jeffreys, while "denying the extension of the Gulf-stream into those latitudes, *produce* "*the strongest evidence against their own assumption.*" Now it would scarcely be supposed from this reference to my views, that in the sentences immediately preceding and following the one cited by Dr. Petermann, I most distinctly recognized the existence of this "deep, voluminous, warm stream" moving from the South-west towards the North-east; making its very depth and volume the basis of my argument that it "*cannot be a* "*continuation of the Florida Gulf-stream,* but that it must form part of *a* "*great general movement of Equatorial water towards the Polar area,*" the complement of "*the great general movement of Polar water towards the* "*Equatorial area,* which depresses the temperature of the deepest parts of "the great Oceanic basins nearly to the freezing-point."

110. Again, in a previous part of the same lecture, I had spoken of this doctrine of a *great general Oceanic Circulation* sustained by difference of Temperature alone, as one of which Physical Geographers could not recog-

nize the importance, so long as they "remained under the dominant idea "that the Temperature of the Deep Sea is everywhere 39°." Now, since in my previous Report of the 'Lightning' Expedition for 1868, I had specially drawn attention to the prevalent error of regarding the temperature of 39° as that of the Deep Sea generally (showing that it is *not* that of the greatest density of Sea-water, which contracts until it freezes), and had demonstrated the non-existence of the supposed line of equal temperature at all depths, separating the Polar sea, in which the temperature *rises* with increase of depth, from the Equatorial, in which it *sinks*, I cannot but feel greatly surprised at finding these errors repeated by Dr. Petermann, as well as by his coadjutors Drs. Von Preeden and Mühry, and determining their interpretation of many of the phenomena which they bring under discussion. Thus Dr. Petermann says of the Gulf-stream that "after coming forth intact from its two struggles with the Polar Stream "east of Newfoundland and east of Iceland, its waters cool more and "more towards the north, until they are reduced to a temperature of 39°·2, "*at which they attain their highest density and greatest weight.* At this "temperature *it sinks beneath* the Polar Stream, in July, north of Iceland "and Spitzbergen, and on both sides of Bear Island .... North of the iso-"thermal curve of 39°·2 toward the Pole, the temperature generally in-"creases with the depth, while southward towards the Equator it decreases." —And Dr. Von Preeden says,—"It has been established beyond doubt, by "late investigations on the high seas, that *Sea*-water reaches its maximum "density, or in other words its greatest Specific Gravity, in exactly the "same manner as *fresh* water at a temperature of 3° R." Dr. Mühry, also, though espousing the doctrine of a *vertical* circulation between Polar and Equatorial waters, maintained by difference of Temperature, altogether misconceives, under the influence of the error just stated, the effects of this circulation. "The vertical distribution," he says, "is regulated by the "hydrothermal law, according to which *sea*-water, like *fresh* water, attains "its maximum density and gravity at about 39°·2 Fahr. Of two streams "meeting each other, one or the other, the colder or the warmer, will soon "prove relatively less heavy; and this will continue its course on the "surface, while the heavier will submerge, and flow underneath the lighter, "provided that they cannot proceed side by side. *It must and can be* "*assumed* that there is at the bottom of the Polar basin really a tempera-"ture of 39°·2 or nearly so; and that there is a gradual decrease of it to "the surface, where it is 28°, the lowest temperature of flowing water, "just near the point of congelation, below the floating field ice of about "eight feet in thickness, which, at the top, has perhaps a temperature of "—58° (—40° R.)."—"This," continues Dr. Mühry in a Note, "will be "disputed by Physicists, who cling to a few contradictory experiments; "Nature, however, offers more correct and decisive data, which are con-"firmed indisputably by the result of our inquiries into the vertical distri-"bution of the Oceanic currents." And the doctrine of the uniform Deep-

Sea temperature of $39^{\circ}{\cdot}2$, originally propounded by Sir James Ross, and unfortunately adopted (to be afterwards entirely abandoned) by Sir John Herschel, is then formally propounded by Dr. Mühry, as if it had not been completely exploded by the results of the 'Porcupine' and other recent Temperature-soundings.

111. Now the increase of density of Sea-water down to its freezing-point, which may be as low as $25^{\circ}{\cdot}4$, is as well established as any fact in Physics; while there is no *reliable* observation (made with Thermometers protected from the influence of pressure) which indicates a Deep-sea temperature of $39^{\circ}{\cdot}2$ in either the Arctic or the Antarctic basin.

112. It will be unnecessary for me to dwell at any greater length on my points of difference with Dr. Petermann; since it will be obvious to any careful reader of his Memoir, that its main object is to prove the existence of the North-easterly movement of a deep voluminous body of warm water (as distinguished from a surface-drift) in the North Atlantic; *on which point I entirely agree with him.* And although he applies to this flow what I cannot but consider the very inappropriate designation of the "Gulf-stream," he expressly guards himself from assiguing to the initial force of the Florida Current (or Gulf-stream proper) the whole, or even a principal part, of the power by which this movement is sustained.

113. But a doctrine of the Gulf-stream has been put forth by my friend and colleague Prof. Wyville Thomson, to which I regret to find myself in direct opposition: for he regards the North-easterly movement of Dr. Petermann's "deep, voluminous, warm stream" as essentially due to the initial force of the Florida Current; and attributes its ameliorating effect on the temperature of the North Atlantic and Arctic seas to the heat which the Gulf-stream proper has brought from the Intertropical zone. "The basin of the North Atlantic," he says, "forms a kind of *cul de sac*; "and while a large portion of the Gulf-stream water, finding no free outlet "towards the north-east, turns southwards at the Azores, the remainder, in- "stead of thinning-off, has rather a tendency to accumulate in the northern "portions of the trough. We accordingly find that it has a depth, on the "west coast of Ireland, of at least 800 fathoms, with an unknown lateral ex- "tension." Further, he says, in "the shallow, including the Hebrides, the "Orkney and Shetland Islands, and the Faroes, stretching westwards and "northwards nearly to Scotland, the average depth is about 500 fathoms; "and the Gulf-stream, which has a depth in these latitudes in summer of "from 600 to 700 fathoms, occupies the whole of it, giving an abnormal "temperature of something like 12° C."*

114. The foregoing sketch of the recent history of this inquiry presents for our consideration four principal Doctrines respecting the causes of the abnormally high Temperature of the British Islands, and of the ameliora-

* See his Lecture "On Deep-Sea Climates," in 'Nature,' July 28th, 1870, and his "Address to the Scottish Meteorological Association," in 'Nature,' July 27th, 1871.

tion of the Climate of the region still further north, extending from Iceland to the east of Spitzbergen:—

I. That of Mr. Findlay, Mr. Blunt, and Dr. Hayes, who regard this elevation of Temperature as having no immediate relation to the Florida Gulf-stream, and attribute it mainly, if not entirely, to the prevalent influence of South-westerly winds and of the surface-drift they maintain; the surface-temperature of the Sea being in their view entirely dependent on that of the Atmosphere above it.

II. That of Dr. Petermann, who attributes it in great part to the north-easterly flow of a "deep, voluminous, warm stream," which he designates as the "Gulf-stream," while admitting that a large part, both of its movement and its temperature, may be due to the "tributaries" which the Florida current receives in its course. He does not, however, specially indicate any of these tributaries; nor does he attribute the movement of his "deep, voluminous, warm stream" to any other agency than the residual force of the Florida current, aided by surface-drift.

III. That of Prof. Wyville Thomson, who explicitly refers the North-easterly flow of Dr. Petermann's "deep, voluminous, warm stream" to the propulsive force of the Trade-winds acting as a *vis à tergo* through the Florida current, and attributes the Thermal influence of that stream almost exclusively to the heat which the Florida Current transports into high latitudes from the Gulf of Mexico; while he regards the underflow of Cold water into the Equatorial Ocean as an *indraught* to replace the Warm water carried off from its surface by the Equatorial current and Gulf-stream.

IV. My own view, which is partly coincident with the first, and partly with the second of those just enunciated. In common with Mr. Findlay, Mr. Blunt, and Dr. Hayes, I regard the progressively diminishing influence of the Florida Gulf-stream as scarcely traceable, either by movement or temperature, to the European border of the Atlantic; whilst, in common with Dr. Petermann, I maintain (as I have maintained from the first) that a "deep, voluminous, warm stream" of Oceanic water, not to be accounted for by surface-drift, is continually flowing in a North-easterly direction between the parallels of 50° and 60°, the extension of which into still higher parallels is the main source of the amelioration of their climate. But I differ from him and from Prof. Wyville Thomson in maintaining that the northward movement of this deep voluminous stream is produced, not by a *vis à tergo*, but by an *indraught* of the Warm upper stratum of the Mid-Atlantic towards the Polar area, to replace the *outflow* of the Cold under-stratum which is constantly taking place over the Deep-sea bed, in virtue of the increase of Density which the Polar column acquires by the reduction of its Temperature. And I also maintain that the amelioration of the Temperature of the Northern area is only due to the Florida Current, in so far as it raises the temperature of the portion of the North Atlantic that lies between the parallels of 40° and 50° (it being from this portion that the

"deep, voluminous, warm stream" is derived), and imparts additional warmth (either in the form of sensible Caloric, or as the Latent Heat of watery vapour) to the South-westerly winds which move over its surface.

115. Now, as I have already remarked elsewhere*, although "to the "*mere* Physical Geographer it may perhaps seem of little importance which "of these views is the correct one,"—the transfer of a vast amount of heat from the Equator towards the Poles by a continual movement of a great body of water being admitted on both sides,—"the question is one of the "highest *scientific* interest, from its relation to the general theory of Ocean "Currents, past as well as present. For if the doctrine of a General Oceanic "Circulation depending only on differences of Temperature be correct, it "comes in as an important element in the study of all other great Currents "at the present time, and especially of those of the Southern Oceans, which "seem much less attributable than those of the Northern to the *primum* "*mobile* of the Trade-winds. Further, the question is of yet greater im-"portance in its Geological relations; since any Circulation that has its "origin simply in difference of Temperature must have been maintained "throughout all geological epochs; and the formation of Glacial beds, "marked by the presence of the marine types of Polar waters, may have "been taking place at any time and in any part of the Equatorial Ocean, "without any reduction of the land temperature. Whereas if these glacial "currents are dependent for their origin on the motion communicated to "the true Gulf-stream (as Prof. Wyville Thomson maintains), they would "cease to flow over the deep bed of the Atlantic, if that stream were di-"verted by the free passage of the Equatorial current into the Pacific."

116. I propose, then, to consider:—

I. The general Distribution of Heat in the Atlantic Ocean; especially with reference to the differences between the North and the South Atlantic.

II. The Thermal condition of the Gulf-stream, and its influence on that of the North Atlantic.

III. The evidence of amelioration in the Climate of the British Isles and of the regions still further north, by a North-easterly flow of Oceanic water.

IV. The question whether this flow is attributable to the initial force of the Gulf-stream proper, supplemented by the general North-easterly surface-drift of the Atlantic; or whether it is a larger and deeper movement, forming part of a General Oceanic Circulation sustained by difference of Temperature.

117. *General Distribution of Heat in the Atlantic Ocean.*—A comparison of the positions of the Summer and Winter Isotherms† in the North

* Proceedings of the Royal Geographical Society, Jan. 9, 1871, p. 70.

† I adopt as my authority for the Isothermal lines of the South Atlantic, and of the North Atlantic as far as 50° N. Lat., the 'Current and Temperature Chart,' issued by the Hydrographic Department in 1868, and the Monthly Temperature Charts of the North and South Atlantic, recently issued by the Meteorological Department; while

and the South Atlantic respectively (see Plate VII.) serves to show (as was well pointed out in the 'Onderzoekingen met den Zee-thermometer' published in 1861 by the Royal Meteorological Institute of the Netherlands) that, while *the North Atlantic is as a whole warmer than the South,* its Temperature *has also a much wider Annual range*: for the corresponding Isotherms lie at a greater distance from the Equator on its Northern than they do on its Southern side; and the central line of the belt of warmest water does not coincide with the Equator, but lies for the most part on its Northern side; so that the Thermic Equator, or line of *maximum mean* Temperature, falls some degrees to the north of the Geographical Equator. The line which divides the two Hemispheres climatologically of course shifts its position according to the seasons; but all these changes of situation take place to the north of the Equator, except in February and March, when this line has its most southerly position, and crosses the Equator in a few places. Again, comparing points at which the Isotherms cross the Meridian of 30° W. Long. in the *North* Atlantic, with the points at which they cross the Meridian of 20° W. Long. in the *South* Atlantic,—these two meridians being, as it were, the axes of their respective basins,—we observe that in the North Atlantic the distance between the Summer and the Winter Isotherms of 60° is nearly 15° of Latitude, whilst in the South Atlantic the distance between those two Isotherms is only about $7\frac{1}{2}$°; and that a like difference shows itself between the seasonal positions of the Isotherms of 55° and 65° in the Northern and Southern Oceans respectively. The great regularity of the directions and distances of the Isotherms beyond the parallel of 30° S. Lat. in the South Atlantic, also, is very striking when contrasted with the irregularity, both in course and distance, which marks those of the North Atlantic. The Annual Range is observed to diminish in the South Atlantic as we pass from lower to higher latitudes; so that to the southward of the parallel of 42° S. the summer and the winter climates differ comparatively little from one another, whilst near the south point of South America the climate is remarkable for its mildness in winter.

118. Now these general differences between the Thermal conditions of the Northern and the Southern Atlantic are fairly attributable to that great *preponderance in the proportion of Land to Sea in the Northern Hemisphere,* which is fully recognized by all Physical Geographers and Meteorologists as an adequate *vera causa* alike for the higher *average* Temperature of the Northern Hemisphere as a whole and for its greater *annual range**. This preponderance is particularly marked in the Eastern Hemisphere, where, in the 180 degrees of Longitude which include the whole of Europe and very nearly the whole of Asia, the Thermic Equator is almost a great circle inclined at an angle of 15° to the Geogra-

for those of Latitudes beyond 50° North, I rely on the two Charts accompanying Dr. Petermann's Memoir.

* See Sir John Herschel's 'Physical Geography,' §§ 250-264.

phical Equator, crossing East Africa, Arabia, and the Peninsula of India nearly in that latitude north. According to Prof. Dove, the mean Summer temperature in the Northern Hemisphere is 70°·9, while in the Southern it is 59°·5; but, on the other hand, the mean Winter temperature of the Northern Hemisphere is 48°·9, while that of the Southern is 53°·6. Thus the *mean* between the Summer and Winter temperatures is 59°·9 for the Northern Hemisphere, and 56°·5 for the Southern; giving an excess of about 3½° to the Northern. But the average *range* between the mean Summer and Winter temperatures for the whole Northern Hemisphere is 22°, while for the Southern it is only 5°·9. The *highest* observed Annual Range in the whole Southern Hemisphere is only 40°, and this over a small area in Patagonia which does not cover *a hundredth* part of its surface; whilst this range amounts to 40° over about *a third* part of the surface of the Northern Hemisphere, and rises in the great Continental areas of Asia and North America even to 30°; whilst a considerable area of Northern Asia has a range of 90°, rising in some places to 100°, and at Yakutsk (which lies in the centre of it) to 106°*.

119. It may be fairly assumed, therefore, that the general difference between the Thermal condition of the Northern and that of the Southern Atlantic Ocean,—as regards (1) the excess of Mean temperature, and (2) the greater Annual Range,—is essentially dependent on the same agency as that which produces those *much larger* general differences in the Continental Climates of the two Hemispheres, which cannot be attributed, with the least semblance of probability, to a transfer of Heat from the Southern to the Northern by Ocean-currents. For these last differences show themselves most remarkably in the Eastern Hemisphere, in which it is physically impossible that any such northward transfer of superheated surface-water can take place to any but a most limited extent; while there is strong evidence, from the low bottom-temperatures found even in the Arabian Gulf, that Polar Cold is imported to the North of the Equator all the way from the Antarctic Ocean †.

120. On more particularly comparing the course of the Isothermal lines in the North and in the South Atlantic (Plate VII.), we observe that in the *eastern* portion of the Oceanic basin, beneath the parallels of 40° North and 30° South, they tend *towards* the Equator. This is obviously due to the fact that, commencing from the coast of Portugal, a surface-flow proceeds *southwards* along the coast of Northern Africa as far as the Gulf of Guinea, carrying into the Intertropical area the colder water of the Temperate Zone; whilst there is a corresponding surface-flow of yet colder water *northwards*, from the Cape of Good Hope, along the coast of South Africa, as far as the Bight of Biafra. These currents are the main *feeders* of the Equatorial current; and although they may be partly sustained by the agency of prevalent Winds, I am disposed to believe that they are essentially

* See 'Handbook of Physical Geography,' by Keith Johnston, Jun., p. 180.

† See my Report for 1868, p. 187, note; and Report for 1869, p. 473, note.

indraughts maintained by a *vis à fronte* rather than by a *vis à tergo*. For it is obvious that the Trade-winds cannot be continually driving westwards the surface-water of the Intertropical region, unless that water is as continually re-supplied; and it is very curious to observe that while the portions of the North- and South-African currents which lie furthest from the coast are drawn into the general westerly drift that prevails over the Intertropical area, the part of each that lies nearest the coast follows its curvature, so that the two meet in the Bight of Biafra, which may be considered the "head water" of the great *Equatorial* current. The *eastward* deflection of a large part of the North-African current, along the whole line of the Guinea Coast, from Cape Verde and Sierra Leone to Fernando Po, only a little to the north of the *west*-flowing Equatorial current (the boundary between the two being almost as sharply defined as the "cold-wall" which separates the Gulf-stream from the Arctic current), is a fact of peculiar significance. And this significance becomes still more marked, as we find that this *Guinea* current is fed, not merely by the North-African current, but by an Easterly flow that is traceable across the Atlantic, usually between 6° and 8° N. Lat., on an average as far as 35° West, and sometimes almost to the coast of Guiana. This flow intervenes between the well-defined Equatorial current which is driven on by the S.E. Trade, and the more expanded Westerly Drift impelled by the N.E. Trade, of which the southern border has nearly the same rate as the Equatorial Current; and the area over which it takes place appears to correspond pretty closely with that of the "Equatorial Calms,"—the Thermic Equator, as already stated, lying a few degrees to the north of the Geographical Equator. Within this area the winds are variable; and neither here nor along the Guinea Coast does it seem that the easterly flow can be sustained by any prevalent movement of the Air in that direction. I would suggest, therefore, that the "Guinea Current" is mainly an *indraught* or supply-current, contributing, with the South-African current, to keep up the level of the head-water of the Equatorial current in the Bight of Biafra; and that, in like manner, the Guinea current itself draws upon (1) the North-African current, and (2) the "still" region of the Atlantic.

121. But besides the indraught propagated backwards (so to speak) from the Guinea Current, the still water that lies between the two great westerly Drift-currents will be likely to take on an Easterly movement of the kind known as *back-water**; for as the part of it in proximity to the current on either side is carried along by the friction of that current, a void will be created, which must be supplied by a flow of water in the opposite direction. And that this is partly the explanation of the more or less constant easterly movement of the band of Atlantic water which lies immediately to the North of the Equatorial Current, from the Guiana coast to the Bight of

* The phenomenon of "back-water" has been well explained and illustrated by Mr. Laughton in his 'Physical Geography in its Relation to the prevailing Winds and Currents.'

Biafra, appears from the fact that (as was first pointed out by Mr. Findlay*) there is a similar counter-current in the Pacific between the Northern and the Southern Equatorial, and that an analogous current is traceable also in the Indian Ocean, setting towards the coast of Sumatra.

122. With all this evidence of *surface*-indraught, therefore, we need have no difficulty in accounting for the constant supply of the "head-water" of the Equatorial Current, without having recourse to the hypothesis of a *bottom*-indraught of Polar water, which is resorted to by Prof. Wyville Thomson (§ 114, III.) to explain the prevalence of a glacial temperature over the Deep-sea bed. It seems obvious, on the axiomatic principle of "least action," that a *surface*-outflow will always be replaced by a *surface*-indraught, wherever this can be supplied; since a *bottom*-indraught would involve the lifting-up of the whole intervening stratum of water: and I am assured by Mathematicians and Physicists of the highest eminence, that this view is not open to question.—We shall hereafter see that the Westerly Drift which answers to the Equatorial Current in the North Atlantic, in its turn draws upon the terminal expansion of the Gulf-stream proper (§ 147).

123. Passing, now, to the *western* side of the North and South Atlantic, we observe that in both alike the Isothermal lines bend *away from* the Equator, in conformity with the known course of the two great divisions of the *Equatorial* Current. For as this sweeps across from the African to the American side of the Atlantic, and strikes full upon Cape St. Roque, which forms the salient angle of the South American Continent about 5° south of the Equator, it is (as it were) split in two thereby; and while the larger or northern division is slanted in a N.W. direction towards the Caribbean Sea, the smaller or southern division is forced to take a decided bend to the southward along the coast of Brazil, forming what is known as the "Brazil Current." This is traceable Southwards, at a gradually diminishing rate, as far as the estuary of the Rio de la Plata, in S. Lat. 35°, where it appears to turn to the eastward and to recross the Atlantic, forming what is known as the "Southern Connecting Current," which enters the South-African Current near its commencement, and thus completes the comparatively simple circulation of the South Atlantic. The Thermal effect of this southward direction of a portion of the *Equatorial* Current shows itself in a very marked manner in the deflection of the February (or *summer*) Isotherm of 80° nearly as far south as Rio Janeiro, so as to approach the February Isotherm of 75° within *eight* degrees on the meridian of 30° W.; although on the meridian of 20° W. these two Isotherms are *twenty-five* degrees apart. Yet notwithstanding this decided evidence of the thermal power of the *Brazil* current, its warming influence does not seem to extend itself in any considerable degree beyond the parallel of 30° S.; along which we find the February Isotherm of 75° and the August Isotherm of 65° running almost coincidently, until they bend northwards

* 'Directory for the Pacific Ocean,' vol. ii. pp. 1243–7.

under the influence of the *South-African* current. The February Isotherm of 70° and the August Isotherm of 60° follow a nearly similar course about 5° further South; as do the February Isotherm of 65°, and the August Isotherm of 55° near the parallel of 40° S. The marked regularity in the distribution of these lines and in their seasonal movements, as compared with the strong deflection of the Isotherms to the north of 30° S., seems to indicate that the Thermal influence of the Brazil current dies out sooner than its moving force; and this, it seems probable, may be attributed to the circumstance that its superheated stratum has only the ordinary thickness of that of Oceanic water, whilst in the Gulf-stream the excess of heat extends to a much greater depth (§ 127). For it is obvious that the thinner the superheated stratum, the more speedily will its temperature assimilate itself to that of the overlying Atmosphere; and that such is the case with the Brazil current, appears from the comparatively small southward deflection of the August (winter) Isotherms of 75° and 70°, while the Southern Isotherm of 80° moves northwards in that month, so as in September to be found wholly to the *north* of the Equator, continuing so in great part through October and November.

124. In our examination of the course of the Isotherms of the North Atlantic, it will be convenient to restrict ourselves, in the first instance, to that portion of it which, with the large contribution supplied by the Equatorial Current from the southern side of the Equator, becomes the feeder of the Gulf-stream.—In February and March, the northern Isotherm of 80° follows pretty closely the northern boundary of the Equatorial Current; but it does not extend into the Caribbean Sea, the general temperature of which during those months does not seem to be above 76°, or about the average of that of the open ocean between the same parallels. In the Gulf of Mexico the surface-temperature falls still lower, especially within the area over which the comparatively cold water brought down by the Mississippi diffuses itself; the temperature of the head of the Gulf being thus reduced even below 70°.—Now since it is at this period of the year that the temperature of the Southern portion of the Equatorial Current is at its highest, averaging about 81½°, it is obvious that the surplus heat which it carries towards the Caribbean Sea and the Gulf of Mexico is for the most part dissipated before its water re-issues into the Atlantic as the Gulf-stream. This will be seen to be a fact of some importance with reference to the Thermal effect of the Atmosphere upon the surface-temperature of the Sea.—The February and March Isotherm of 70° between the meridians of 35° and 75° W. keeps very close to the parallel of 28° N., passing directly across the area of the Sargasso Sea; east of Long. 35°, however, it bends southward, under the influence of the colder North-African current; whilst to the west of Long. 75° it turns abruptly to the North, under the influence of the Gulf-stream, which issues from the Florida Channel during these months at a temperature of about 75°. Impinging, as it were, on the Isotherm of 70°, the

warmer Stream carries it northwards as a loop, which, however, stops in February at Cape Fear (N. Lat. 34°), and in March at Cape Hatteras (N. Lat. 35¼°); and thus we see that instead of carrying far into the North Atlantic the temperature of the Equatorial current, the Gulf-stream has lost thus early *more than ten degrees of* the heat which its water possessed when it crossed the Equator, and is *only five degrees* warmer than the sea through which it flows.

125. Very little change takes place during March and April in the general position of the Isotherm of 80°; but the temperature of the Caribbean Sea and of the Gulf of Mexico rises considerably, so that the Gulf-stream in May issues from the Florida Channel at the temperature of nearly 80°. The Isotherm of 70° moves northwards with the approach of the Sun to the Tropic of Cancer; but the looped bend in its western portion occasioned by the Gulf-stream still stops at about 37° N. Lat. From June to September, however, the northward range of the Isotherm of 80° extends on its western side over the Caribbean Sea and Gulf of Mexico; whilst it also includes a considerable part of the Sargasso Sea, ranging to the north of the Bermuda group and approaching the American coast off Cape Hatteras. The Gulf-stream during these months issues from the Florida Channel at an average surface-temperature of about 83°, which is *higher* than that of the Equatorial current at its entrance into the Caribbean Sea; and for this there is no difficulty in accounting, when we bear in mind the fact that the Sun shines vertically on the Tropic of Cancer at the Summer solstice. There is nothing special, however, in the surface-temperature of the Caribbean Sea and Gulf of Mexico at this season; for that of the outside Atlantic between the same parallels, though in no way influenced either by the Equatorial current or by the Gulf-stream, is just as high. In the *eastern* portion of the Atlantic, however, the Isotherm of 80° is greatly deflected towards the Equator, not merely by the colder North-African current, but also, it would appear, by the southward flow of that portion of the Gulf-stream which passes round the Azores to return into the Equatorial current (§ 147).—Thus the surplus heat which the Gulf-stream carries northwards during the summer months is not that of the Equatorial region, but that of the northern Tropical region; and we shall presently see (§ 142) that the slow dissipation of its surplus Heat in this part of its course is due to its near equality in Temperature with the Sea and Air through which it passes for a considerable distance.

126. From October to January, on the other hand, there is a progressive return towards the conditions indicated by the course of the Isothermal lines for February and March; with this difference, however, that while the January Isotherm of 70° for the most part lies south of 30° N. Lat., and does not range further north than Cape Hatteras, the Isotherm of 80° extends, in the course of the Equatorial current, into the southern part of the Caribbean Sea. Thus as the temperature of the Gulf-stream at its

exit, during the months of November, December, and January, is about 76°, there is a *loss of heat of about four degrees* in the passage of the water that feeds it from the *north* coast of South America to the *south* coast of North America.

127. *Thermal condition of the Gulf-stream, and its influence on that of the North Atlantic.*—In considering this part of the subject, we have first to inquire whether the Thermal condition of the Caribbean Sea and of the Gulf of Mexico, which last constitutes the head-water of the Gulf-stream, differs in any essential condition from that of the outside Atlantic: and on this point we derive important information from a series of Thermometric observations recently taken in the New York School-ship 'Mercury,' which show that a very close correspondence in *surface*-temperature may veil a considerable difference between the temperatures of the underlying stratum.—The 'Mercury' crossed the Atlantic in the month of March from Sierra Leone to Barbadoes, running before a nearly steady E.N.E. Trade-wind, with an Atmospheric temperature ranging from 77° to 83°, the surface-temperature of the Sea ranging from 75° to 80°. At a depth of 100 fathoms, the temperature was 62°; while at 200 fathoms it was found to be everywhere (save in comparatively shallow water, § 9) between 51° and 53°, the mean of eight observations being exactly 52°. Having proceeded from Barbadoes to St. Thomas, the 'Mercury' sailed, during the first fortnight of April, along the southern side of Porto Rico, St. Domingo, and Cuba, finding deep water everywhere; and then, rounding the western extremity of Cuba, she made her way northwards through the Florida Channel. Now in this part of the course, the temperature of the Air ranged from 80° to 84°, and the surface-temperature of the Sea from 83° to 86°, showing a considerable elevation, which is probably attributable to the influence of the *Equatorial* current, this being at its hottest in March and April. But a still more marked excess shows itself in the temperature of the stratum beneath; for at 100 fathoms the temperature was found to be 72°, and at 200 fathoms 62° (in both cases *ten degrees* above the temperatures at corresponding depths in the outside Atlantic), at 300 fathoms 54°, at 400 fathoms 50°, and at 500 fathoms 48°. —This, as it seems to me, is an indication that the partial separation of the Caribbean Sea from the Basin of the Atlantic produces somewhat the same effect upon the temperature of its deeper water, that is shown in a much more remarkable degree in the Mediterranean (§ 10); for if the bottom-inflow of Polar water be impeded (though not altogether prevented), the temperature of the whole mass of the water that is not directly subjected to solar influence will of course be proportionally higher *.

* It is remarkable, however, that on the Mosquito Coast,—where a very high surface-temperature usually prevails,—the sub-surface temperature seems to correspond more closely to that of the outside Atlantic; the following observations being given by Capt. Maury from the journals of Mr. Dunsterville:—at 240 fathoms, 48°; at 386 fathoms, 43°; at 450 fathoms, 42°. This difference is perhaps attributable to the much

128. We have next to inquire what is the Thermal relation of the *Gulf-stream* to that of the basin from which it issues; and what indications are afforded by its Temperature at different Depths as to the Movement of its different strata.—In the first place it is essential to bear in mind that the southward extension of the *Polar Current* which impinges against the Gulf-stream on the Banks of Newfoundland has been very distinctly traced along the sea-board of the United States, as a distinct band of separation between the coast-line and the Gulf-stream, as far south as the Peninsula of Florida: for although its surface may be warmed by an overflow from the Gulf-stream (as, in the 'Lightning' channel, the glacial stream from the N.E. is overlain by the warm stream from the S.W.), yet its presence is distinctly indicated by the rapid descent of the thermometer at small depths beneath. Thus off Sandy Hook, where the distance of the Gulf-stream from the coast is about 240 miles (Diagram v. Plate V.), the temperature of the surface near the coast is 70° in summer, rises to 75° at about 150 miles distance, and to 83° at about 275 miles, where the section crosses the warmest band or axis of the Gulf-stream: but at a depth of only 20 fathoms in this intervening band, the thermometer falls to 60°; at 100 fathoms it averages about 47°; at 200 fathoms it is about 43°; at 300 fathoms from 39° to 42°; and at 400 fathoms from 37° to 40°. As soon, however, as the "cold wall" has been passed, the thermometer at 20 fathoms rises to 77°; at 100 fathoms to 67°; at 200 fathoms to 62°; at 300 fathoms to 59°; and at 400 fathoms to 55° or even 58°.—The breadth of the Polar Stream gradually diminishes, and the rate of its movement decreases; but that it preserves a steady onward flow is proved by its continuity of temperature, which is distinctly traceable along the whole length of the Floridan Peninsula, from Cape Canaveral, where its breadth is about 35 miles, into the Florida Channel itself. For here the Gulf-stream is separated from the American shore-line by a band about 10 miles broad, in which the temperature falls within 100 fathoms to 50°, whilst in the axis of the Stream it averages 75° at that depth; and very distinct evidence of the *inward* movement of this colder band is afforded by the fact, first pointed out by Prof. Agassiz * and since verified by Capt. E. B. Hunt (of the U.S. Engineers), that the Florida Keys and Reefs are slowly but steadily extending *westwards* †. During rough weather, the sea about the reefs becomes milky from the stirring-up of the deposit at the bottom; and this "white water" is invariably drifted to the westward, the matter it carries being slowly deposited both north and south of the

freer and more direct communication which exists between the deeper parts of the *southern* portion of the Caribbean Sea and of the outside Atlantic, than that which its *northern* portion possesses.—Further inquiry into the sub-surface temperature of different parts of the Caribbean Sea and the Gulf of Mexico is much to be desired on various grounds.

* U.S. Coast Survey Report, 1851; and Appendix 10, p. 145, 1860.

† Silliman's American Journal, vol. xxxv. pp. 197–210 & 388–396.

line of Keys, and 30 or 40 miles to the southward of them. That the inward counter-current, of which this fact affords evidence that may almost be called demonstrative, occupies not only that shallower portion of the channel which follows the coast-line of Florida, but the lower portion of the deepest part of each section, where it underlies the axis of the outflowing Gulf-stream, will be presently shown to be an equally certain inference from the Temperatures met with at different depths in each Section, from the surface downwards (§§ 129–133).

129. It is on the line of Section taken by the United States Coast Survey in 1866, between the Dry Tortugas and Havana (see Diagrams IV., VI., Plate V.), that our detailed knowledge of the Gulf-stream commences. The breadth of this channel is about 82 miles; and its maximum depth, which is met with at about 37 miles from the Cuban shore, is 845 fathoms. On the northern side, the bottom lies in terraces, which are nowhere abrupt; but on the southern side, about halfway between the deepest part and the Cuban shore, there is a steep ridge rising about 400 fathoms from the sea-bed, so as to come to within about 350 fathoms from the surface. The course of the bathymetrical Isotherms in the different parts of this Section is very remarkable. Instead of lying parallel to the surface, they follow the contour of the bottom; *the colder water being found much nearer to the surface in the shallower than in the deeper portion of the channel.* The following is the bathymetrical range of these lines at intervals of 5°:—

Temperature.	Range of Depth.
75°	20 to 130 fathoms.
70°	60 to 180 ,,
65°	75 to 250 ,,
60°	90 to 320 ,,
55°	150 to 380 ,,
50°	200 to 440 ,,
45°	350 to 500 ,,
40°	480 to 550 ,,
35°	580 to 600 ,,

Thus it appears that the lower half of this channel is occupied by water whose temperature is below 50°; and that this colder water is thrown up in the shallower parts of it to within 200 fathoms of the surface. Looking to the steady diminution of temperature with depth, until a temperature of 35° is reached at about 600 fathoms, it can scarcely, I think, be questioned that the whole of the water below the bathymetrical Isotherm of 50° is an *inward* prolongation of the Polar Stream; the *out*flowing Gulf-stream being limited to the stratum of 60° or upwards. This limitation was indicated also by the deflection of the sounding-line occasioned by the current; which led the U.S. Surveyors to the conclusion that the outwardly moving stratum has not above *one third* of the actual depth of the channel.

In the northern half of this section, above the terraces south of the Florida Reefs, the water lies almost motionless; and the eastward flow of the Gulf-stream is limited to the deeper southern half of the channel.

130. Proceeding about 120 miles to the eastward, we find that while the channel (between the Sombrero Lighthouse and the Salt Key Bank) is narrowed to 45 miles (see Diagram IV.), and its maximum depth is reduced to 600 fathoms, the evidence of a division between two strata is still more distinctly marked by the course of the bathymetrical Isotherms (Diagram VII.). The northern slope, as in the previous instance, is the more gradual, the depth being only about 120 fathoms at a distance of 13 miles from the American shore; and here we find that while the temperature at 50 fathoms is 75°, so rapid a reduction shows itself with increase of depth, that the bottom at 120 fathoms is below 50°. In mid-channel, on the other hand, where the depth is the greatest, the line of 50° sinks to 175 fathoms, while towards the Salt Key Bank it is found at 200 fathoms; and the whole mass of water that occupies the deeper portion of the channel has a temperature below 50°, the thermometer showing 45° at 250 fathoms, 40° at 350, and 35° at less than 400.—Thus it seems clear that the colder water which underlies the warm Gulf-stream surges up on the shallower bottom of the northern side of it, so as to rise to within 120 fathoms of the surface, though overlain by a thin stratum of water having nearly the temperature of the Gulf-stream proper.

131. After passing Sombrero Lighthouse, the channel of the Gulf-stream begins to turn northwards (Diagram IV.): and the next line of section, taken between Carysfort Lighthouse and the Great Bahama Bank (Diagram VIII.), shows it to have somewhat widened and at the same time become shallower. The total breadth of the channel is here about 60 miles; but of this, a band of about 15 miles broad on the Florida side appears to be occupied by the Polar Stream. The maximum depth in mid-channel is about 500 fathoms; and the portion of it that exceeds 400 fathoms in depth is about 35 miles broad. The general character of this section very closely approximates to that of the Havana section, except that the slope of its banks is more equal. It is only on the American side, however, that the Isotherms down to 50° run upwards, showing the approach of the colder water to the surface. Thus the temperature of 55° is there found at 125 fathoms, whilst on the other side it is not reached at 250. At 350 fathoms in mid-channel, the temperature is below 50°, and in 60 fathoms more it is reduced to 40°. Although the temperature does not seem to have been observed at depths much greater than 400 fathoms, it can scarcely be doubted that it would be found as low as 35° in the deepest part of this channel, as it is at a less depth in the Sombrero section.

132. The channel again contracts, and at the same time diminishes in depth, as far as Cape Florida (Diagram IV.), between which and the Bemini Isles its sectional area is smallest: for, while its whole breadth is not much

more than 40 miles, the Polar stream occupies 15 miles of this breadth on the Florida side; and while its greatest depth scarcely exceeds 300 fathoms, even this excess only extends over a breadth of about ten miles, forming a valley on either side of a submarine ridge (Diagram IX.). The Florida slope shows a well-marked succession of terraces, whilst the Bemini slope is very steep. It is most singular to see how the bathymetrical Isotherms here follow the undulating contour of the bottom, instead of lying parallel to the surface; plainly indicating that the colder and heavier water has a motion of its own, by which it is carried up the slopes of the hills, instead of finding its level in the valleys. The 75° line, which lies at the surface on the Florida side, sinks to nearly 100 fathoms in the deepest part of the channel; and at ten miles from Cape Florida, where the whole depth but little exceeds 160 fathoms, the water has a temperature of 70° even at 75 fathoms, clearly showing the extension of the warm stratum to that side of the channel. But below 75 fathoms, on this terrace, the temperature falls so rapidly, that 45° is reached at 140 fathoms; though in the deeper channel the water at that depth has a temperature of 65°, sinking to 45° at 250 fathoms. No temperature below 45° is recorded as having been observed in this section; but if the rate of bathymetrical reduction between 200 and 250 fathoms be continued downwards to a bottom below 300, the temperature of the lowest stratum will be found as low as 35°.

133. Although the temperature of 35° was not actually observed either in the Carysfort or in the Bemini sections, the fact that it showed itself in the deepest water of both the Sombrero and the Tortugas sections, and in the latter at a depth of less than 400 fathoms,—taken in connexion with the difficulty, if not the impossibility, of otherwise accounting for the presence of water of 35° within the Narrows,—seems fully to justify the assumption that it has flowed over what may be called the submarine watershed between Cape Florida and the Bemini Isles; and it is clear that in so doing it must have flowed up a very considerable ascent, which it could only do in virtue of a constantly acting propulsive force. This is strikingly shown in Diagram III., Plate IV., which represents the longitudinal course of the bathymetrical Isotherms of 50°, 45°, 40°, along the axis of the Gulf-stream in its passage through the Florida Channel.—The soundings taken in the subsequent course of the Gulf-stream do not anywhere give the depth at which temperatures below 45° exist beneath it; but as that temperature is reached in the Polar current at 200 fathoms, while the thermometer sinks at 300 fathoms to 40°, and at 400 fathoms to 38°, it seems clear that the water occupying the lower stratum of the Florida Channel is derived from the same source.

134. That the Polar current which passes into the Gulf of Mexico beneath the outflowing Gulf-stream is due to the excess of density in the outside column, depending on the depression in the temperature of its lower stratum, was maintained in 1867 by Mr. Mitchell*, who had been engaged in the

* Silliman's American Journal, vol. xliii. (1867) p. 74.

survey of the Tortugas section, and reported very carefully on the phenomena it presents. He regards the Gulf-stream current, like that of the Baltic or the Dardanelles, as tending to the restoration of the surface-level of the basin from which it flows, which the inset of the Equatorial current will as constantly tend to raise; and he gives the following example of "compensating currents," which is peculiarly applicable not merely to the doctrine now under consideration, but also to that of the under-currents of the Straits of Gibraltar and the Baltic Sound.—During the dry season (July), the surface-*out*flow of brackish water through the Narrows of New York harbour occupies *nine* out of the twelve tidal hours; this water chiefly consisting of the water brought down by the Hudson River, with some admixture of salt water. This salt water enters as a lower stratum, the *in*flow of which is nearly uninterrupted, though not constant in velocity; and the same condition extends for some distance up the river. On running a line of levels from New York City to Albany, it was found that the bed of the Hudson lies below the mean level of the sea for nearly 100 miles from its mouth; but that the surface of the fresh water, even in the dry season, is above this level,—not so much so, however, as to equalize the difference of weight between the column of River-water within the Narrows and that of the Sea-water outside; so that the latter during the summer months flows in along the bed of the stream, while the former overflows into the ocean. During other parts of the year, however, the level within the Narrows being higher, the inner column is the heavier, and the inflow of Sea-water does not take place.

135. We are now prepared to examine the influence of the body of warm water discharged by the Gulf-stream into the Mid-Atlantic, on the Temperature of the Oceanic area through which it makes its way.—It is obvious that so long as it retains its Northern course, and does not diffuse itself to the East—while it is completely shut in on its Western border by the Arctic current—it cannot directly affect the temperature of the general surface of the Ocean west of the Meridian of 70°; and we find (as already pointed out) that the Winter Isotherms of 70° and 65° are only carried northwards in the immediate course of the Stream, while the great northward movement of the Summer Isotherms of 80° and 75° appears to be quite sufficiently accounted for by the general influences already referred to (§§ 117–119) as modifying the climate of the North Atlantic.

136. The most striking peculiarity in the Thermal condition of the Gulf-stream, as compared with that of the Ocean through which it flows in the first part of its course, seems to consist, not so much in its more elevated surface-temperature, as in the greater thickness of its superheated stratum; which, as will be presently shown, has a most important influence on the maintenance of its surface-temperature when it is exposed to an atmosphere colder than itself. Recent inquiries have shown that the high surface-temperature of the Gulf-stream is not derived (as commonly supposed) from its circulation within the almost land-locked Gulf of Mexico; for the average

surface-temperature of the Gulf of Mexico is usually *lower* than that of the Caribbean Sea, and during several months in the year it is rather *below* that of the Gulf-stream in the Florida Channel (§ 124). Hence the surface-layer, at least, of the Gulf-stream would appear to be derived from that warmer current which, after flowing westward along the south of Cuba, turns abruptly round Cape Antonio, and then flows eastward towards the Florida Channel.—But we might expect that the Gulf of Mexico, even more than the Caribbean Sea, would have its *sub*-surface temperature elevated by the limitation to the admission of Polar water; and such an elevation we find to be presented in the section between the Tortugas and Havana, which may probably be considered, as regards this stratum, to represent the Gulf of Mexico generally. Accordingly, on comparing its thermal condition (§ 129) with that of the corresponding stratum of the Caribbean Sea, as shown in the temperature-soundings of the 'Mercury' (§ 127), we see that the temperatures between 100 and 400 fathoms are a few degrees *higher* in the Tortugas section than in the Caribbean Sea; the temperature of 62°, which extends in the former to 300 fathoms, ranging in the latter only to 200. But even this downward range contrasts strongly with the limitation of that temperature to the uppermost 100 fathoms in the open Ocean, and the rapid fall in the next 100, as shown in the 'Mercury' soundings to the east of Barbadoes (§ 127). This downward extension of surplus heat is retained through all the early part of the Gulf-stream; as has been already shown in the contrast between the temperatures of the Polar Current and of the Gulf-stream off Sandy Hook (§ 128).

137. It can scarcely be doubted that the extraordinary thickness of the superheated stratum in the Gulf-stream has much to do both with its own retention of a high surface-temperature in its northward course and with its power of imparting heat to the Air above it; for it is obvious that the thinner the superheated stratum, the more speedily will its temperature be brought down by that of a colder superincumbent atmosphere. But it is also clear that its loss of heat may be really much greater than the reduction of its surface-temperature would indicate; for as fast as its superficial stratum is cooled down, its increase of density will cause it to sink until it meets with water as cold as itself, the warmer sub-surface stratum rising into its place. And since this process will continue to take place so long as there is a subjacent stratum of higher temperature than the superincumbent Atmosphere, the *surface*-temperature of the current may be maintained with comparatively little reduction, though the temperature of its subjacent strata may have been reduced almost to that of the ordinary Oceanic water which it overlies. But in the very same proportion that it thus *loses* heat, will it *impart* heat to the Atmosphere above; and may thus, by warming a vast body of air which is rapidly borne towards North-western Europe, furnish an important element in the amelioration of its climate.—The dependence of the retention of a high surface-temperature on the thickness of

the superheated layer is well shown in the contrast between the Mediterranean and the portion of the Gulf-stream that lies between the same parallels. For while the surface-temperature of the former sometimes rises as high as that of the latter, it is subject to a considerable *diurnal* range, in consequence of the thinness of its superheated stratum; while its *seasonal* range closely follows the temperature of the atmosphere, down to its constant winter mean of about 54° (Report for 1870, § 89). But in the latter there is scarcely any diurnal range; while its winter temperature only falls to 72° off Cape Hatteras, and to 67° off Nantucket; for as a temperature of 60° or upwards extends to a depth of from 200 to 300 fathoms, the whole of this *sub-surface* stratum must be reduced to that standard, or below it, before the *surface*-temperature would fall to 60°.—It follows as a Corollary, that a *deep* stratum having a moderate excess of temperature may be a far more effectual carrier of Heat into a colder area than a much warmer superficial flow.

138. The *thermal power* of the Gulf-stream, or of any similar Current, must depend on three factors,—(1) the *Sectional Area* of the Stream; (2) the *Rate* of its flow; and (3) its *Temperature*.

139. It is impossible, in the present state of our knowledge, to arrive at any exact estimate of the *sectional area* of the Stream; since it is for the most part only from the Temperatures of its different strata that we can judge whether they are, or are not, in movement, and what is the direction of their movement. If we accept the conclusion of the U.S. Surveyors (§ 129) that in the *Havana* Section the depth of the *outward* current was not above one-third of the greatest depth of the Channel,—namely, about 200 fathoms,—and take the average temperature at that depth to be about 60°, we may assume that the stratum extending downwards to 60° in other sections represents the real outwardly flowing Stream. Now it will be seen that in the Cape Florida and Bemini Section, the line of 60° in the channel of the Gulf-stream proper has an average depth of about 175 fathoms, or 1050 feet; but the breadth of the outflowing Stream cannot be taken (according to Prof. Bache's estimate) at more than 25 miles.—Mr. Croll's estimate of the quantity of water as equal to a stream 50 miles broad and 1000 feet deep, appears to be based on the sectional area of the entire channel, which he states at 30 miles broad and 1950 feet deep; but it seems clear that the lower portion of this channel must be occupied by an *inflowing* current; and the only question is with respect to the depth at which the reversal of the flow takes place.

140. The *mean annual rate* of the Gulf-stream current is estimated by our Meteorological Department, on the basis of a large body of observations taken at various periods of the year, at not more than *two miles* per hour, or 48 miles per day, even in the "Narrows" where the current is most rapid. The rate stated by Mr. Croll, namely *four* miles per hour, is only seen at the period of the greatest strength of the current.—But the rate given by *surface*-observations affords no indication whatever of the rate o

movement in the sub-surface stratum; and all our knowledge of double currents would lead to the belief that it gradually diminishes from above downwards, until the water becomes motionless, or nearly so, as we approach the plane of reversal. Hence, taking the medium between *two* miles and *zero*, the *average rate* of the whole outflowing stream would be no more than *one mile* per hour.

141. If we assume the limit of the stratum above 60° as that of the real Gulf-stream current, we shall find its *average temperature* to be somewhat higher than it has been stated by Mr. Croll, who seems to have taken 65° as the average of the water flowing through the *entire* channel. The average *surface*-temperature of the Florida Channel for the whole year is 80°; and we may fairly set the average of the entire outgoing Stream, down to the plane of 60°, at 70°, instead of 65° as estimated by Mr. Croll.

142. The prevalent doctrine, however, of the persistence of this Temperature, with but very trifling reduction, nearly as far as the Banks of Newfoundland, is based on observations made during the Summer; when the Isotherm of 70° extends north of the parallel of 40°, and the cooling influence of the atmosphere is consequently at its least. In Winter, on the other hand, when the Isotherm of 60° follows nearly the same line, the surface-temperature of the Gulf-stream is reduced almost to that degree before it reaches the Banks; as is shown in the following Table, derived from the Admiralty Chart:—

	Winter.	Spring.	Summer.	Autumn.	Mean of Year.
	°	°	°	°	°
Gulf of Mexico	73	77	83	80	78¼
Florida Channel..........	77	78	83	82	80
Off Charleston	75	77	82	81	78¾
Off Cape Hatteras	72	73	80	76	75¼
S.E. of Nantucket Shoals ..	67	68	80	72	71¾
South of Nova Scotia......	62	67	78	69	69

Thus it appears that instead of a loss of only 5° in the northward flow of the Gulf-stream from Lat. 25° to Lat. 35°, the average loss for the whole year is 11°. And the cooling influence of the superincumbent Air on the Gulf-stream, *even at its deepest and strongest,* is manfested in its loss of 13° of surface-temperature in the Autumn, and of 15° in the Winter, although its passage thus far is accomplished in from forty to fifty days.—During this time, it must be remembered, it continues to lose heat by evaporation as well as by radiation; the large amount of vapour which is being continually given off being made manifest by its precipitation in the form of fog when the Gulf-stream encounters the Arctic current which meets it before it reaches the Banks of Newfoundland.

143. It is, again, on the contrast in Temperature between the Gulf-

stream and the Arctic current,—not between the Gulf-stream and the ordinary Oceanic water,—that the prevalent notions respecting its special heating-power are mainly founded. Thus Admiral Sir Alexander Milne, proceeding in H.M.S. 'Nile' from Halifax to Bermuda in May 1861, found the temperature 70° at the bow, when it was only 40° at the stern; thus showing a difference of 30° within the ship's length. When, on the other hand, the temperature of the *eastern* edge of the Gulf-stream in the earlier part of its course, and of the *southern* edge in the later part of its course, is compared with the normal of the neighbouring portion of the Atlantic, the difference is found to be comparatively slight, the one graduating into the other.

144. The average rate of two miles per hour, which the Gulf-stream in the Narrows, is maintained to Lat. 30°; but it then begins to show a decided reduction, falling to 40 miles per day between 30° and 33° N. Lat. When the Stream has passed Cape Hatteras, and its land side is pressed on by the Arctic current, this compression seems to have the same effect in increasing its velocity that limitation between banks would exert; for the rate of flow there rises again, sometimes exceeding *four miles* per hour. At the same time the rate of the Stream at its *outer* edge is not greater than from 10 to 20 miles per day. The direction of the Stream is gradually changed by the trend of the coast-line, first from N. to N.E. by N., then to N.E., and subsequently, after being subject to the influence of the Arctic current, to E.N.E. Part of this Easterly deflection, however, is probably to be attributed to the *greater* easterly momentum which this body of water brings with it from its southern source, in virtue of its excess of rotary velocity; as was first pointed out by Capt. Maury, whose view on this point was adopted by Sir John Herschel. And, of course, the further North the Stream advances, the more strongly will this excess show itself, in giving to the movement of the Stream an Easterly direction. Conversely, the Arctic Current coming Southwards will bring with it a *smaller* rotary velocity than that of the parallel it reaches, and will thus have a Westerly tendency, which will keep it close to the coast-line of the United States.

145. Very early in its course, the Gulf-stream begins to show a division into alternate bands of warmer and colder water; and these become very perceptible before it passes Charleston. The cause of this division appears to lie in the contour of the bottom in the Florida Channel; the elevations of which, as already stated (§ 132), throw up the colder water of the deeper stratum nearer to the surface. With the increase in the breadth of the Stream as a whole, there is at the same time an increase in the distance between the bands. Thus at Cape Hatteras, where the "cold wall" separating the Gulf-stream from the Arctic Current is 30 miles from shore, the first or axial band of the Gulf-stream has a breadth of 47 miles; to the east of this there is a cold band 25 miles broad; and this is succeeded by another warm band of 45 miles. These two warm bands, with the intervening colder band, are considered by Prof. Bache as constituting the

Gulf-stream proper; but to the east of this, beyond another cold band of 37 miles breadth, there is still another warm band 75 miles broad.—Off Sandy Hook, where the Stream makes its great bend to the East, and the "cold wall" is at a distance of 240 miles from the shore, the "Gulf-stream proper" has only increased in breadth from 117 miles to 127; but the breadth of the second cold band has now increased from 37 miles to 60, whilst the breadth of the third warm band has diminished from 75 miles to 50; the outer portion of the Stream showing an obvious tendency to lose itself in the general mass of Oceanic water.—The *total* breadth of the Gulf-stream is stated by the American Surveyors to be 350 miles off Cape Hatteras, and 410 miles off Nantucket; but as the outer boundary is not well defined, these estimates are only approximative.

146. The difference in the rate of movement of these bands is probably one source of the discrepancy in the statements given by different authorities as to the rate of the flow of the Gulf-stream as a whole. It is commonly said to pass Nantucket at the rate of about *one* mile an hour; and an observation cited in Blunt's 'Coast Pilot' would give about 0·7 mile per hour as its rate between W. Long. 57° and 55½°, between the 41st and 42nd parallels of Latitude. Many degrees to the east of this, however, a very rapid current—running at the rate of even *four* miles an hour—has been occasionally observed; and this is probably due, like that sometimes seen off Cape Hatteras, to the lateral pressure exerted by the Arctic current, which comes down in full force over the Banks of Newfoundland, sometimes extending far to the southward, directly into the course of the Gulf-stream. It is during the early months of the year that, under the influence of the N. and N.W. winds which then prevail along the coast of Labrador, the Arctic current—bringing with it immense fields of polar ice—is at its strongest; and the Admiralty Chart shows the southward extension of this field-ice, between March and July, reaching even to the 42nd parallel between the meridians of 55° and 45° W. Long.; while between April and June, icebergs range as far South as 39° between the meridians of 50° and 40°. This enormous body of Polar ice-laden water must have a powerful influence both on the movement and on the temperature of the Stream against which it impinges, more especially since the deep-floating icebergs will bring this influence to bear directly on its deeper strata; and it is considered by Mr. Findlay that "by the time the Gulf-stream "has reached this limit, its original character is so thinned out and ex-"panded, and its specific character is so destroyed from this cause, and "from the neutralizing effects of the Labrador currents, that it can no "longer be recognized beyond this cold-water gulf, which cuts off, as it "were, its further progress, and which, it is manifest, it can neither bridge "over nor pass under." (Proc. Roy. Geogr. Soc., Feb. 8, 1869, p. 107.) —That the Stream has here for the most part thinned-out to a comparatively shallow stratum running over much colder water, is indicated by the observations of Capt. Chimmo (*Op. cit.* p. 92 *et seq.*), which were made

in July 1868, on the axis or northern border of the Gulf-stream. Thus near the southern edge of the Grand Bank, the temperature of the surface being 65°, it was found to be only 50° at 100 fathoms; and a large iceberg was met with as far as 30 miles south of the Grand Bank, showing the southward extension of an *underflow* of Arctic water, although the surface-temperature of 62° indicated that the upper stratum consisted of Gulf-stream water flowing in a very different direction. "Although it was still "150 feet high, and nearly 400 immersed, it was quickly and perceptibly "undermining, decomposing, splitting with loud reports, and floating "away in large portions with the easterly current." In another instance, the surface-temperature being 61°, the temperature at only 50 fathoms depth was as low as 43°; but there was here probably a mere overflow of the surface-stratum, corresponding to that which extends beyond the "cold wall" off the coast of the United States (§ 128). That there is still a warm band extending to a considerable depth, seems to be indicated by the sounding taken in Lat. 43° 30′ N. and Long. 38° 50′ W. to test the existence of the supposed "Milne Bank." Here the surface-temperature being 73°, the temperature at 100 fathoms was 62°, and at 300 fathoms was 55°; and thus, although the temperature of the upper stratum was nearly ten degrees lower than that which it showed off Sandy Hook (§ 128), the temperatures at 100 and 300 fathoms were nearly identical with those there found at corresponding depths, and were above those found at the same depths and under almost the same parallel nearer the coast of Europe (§ 13).

147. The direction of the Gulf-stream current is here so nearly due East, that we should naturally look for its continuation across the open Ocean in the same direction. After passing the meridian of 40° W., however, we find the summer Isotherm of 75°, which rises between the meridians of 45° and 65° W. to the north of the parallel of 40° N., and the summer Isotherm of 70°, which rises to 43° N., as well as the winter Isotherm of 60°, which nearly coincides with it, all tending *southwards*,—the first very abruptly, the second and third more gradually (Plate VII.); and this tendency corresponds with the general set of the surface-current. From the coincidence of these facts, there can be no reasonable doubt that a very large portion of the Gulf-stream here takes a Southward direction, passing first S.E. towards the Azores, and then due S. and S.W., so as to return off Cape Verde into the Equatorial Current,—thus completing, with the North-African Current (§ 120), the "shorter circulation" of the North Atlantic. This deflection of the Gulf-stream current seems dependent on an *indraught*, producing a "compensation-current" for the replacement of the water driven westward by the Trade-winds.

148. The question we have next to consider is,—What evidence can be adduced of the extension of any part of the Gulf-stream proper, on the Easterly line of direction which it retains on the Meridian of 40° W., to the western coast of Europe? It seems to be now clearly established that

before reaching the Meridian of 30° W., the Gulf-stream, to use the language of Dr. Hayes, "has lost every distinctive character as a current: first, in "rate of flow, which has become that of the general easterly 'set' of the "Atlantic; second, in temperature, which has become that of the general "temperature of the air; third, in colour of water, which has lost the blue "that it had whene merging from the Gulf of Mexico; in every thing, in "fact, which goes to make up what we designate as an 'ocean-current.'" This general Easterly "set," according to the Admiralty Chart, prevails over the whole area of the Atlantic between the parallels of 43° (Cape Finisterre) and 55° (Belfast), taking a more northerly direction in still higher latitudes. Its usual rate appears to be from 6 to 24 miles per day, varying with the degree in which the westerly "Anti-trades" predominate in force and duration over other winds. That it is essentially independent of the Gulf-stream, seems to be distinctly indicated by the fact that it commences from the very edge of the Arctic current, on the Eastern side of the broad band which it covers, as far as 10° to the north of the Gulf-stream, running parallel to its course. Although mainly due to the dominant influence of the "Anti-trades," yet it may be partly accounted for in another way; for if, as I have already argued (§ 13), the great body of *sub*-surface-water occupying this band has been drawn into it from a lower latitude, this water will tend (like that of the Gulf-stream itself, § 144) to move from West to East, in consequence of the excess of easterly momentum it has brought with it.

149. Now in the midst of this general Easterly "set," the only indication of any continued movement of the Gulf-stream in its original direction is the variable Current known as "Rennell's;" which, flowing Eastwards into the southern part of the Bay of Biscay, is then deflected in a north-westerly direction by the turn of its coast-line, so as to cross the Channel towards the Scilly Islands, and thence passes to the S.W. coast of Ireland. Though this is usually described as "a branch of the Gulf-stream," I look upon it as the principal continuation of that portion of it which does not turn southwards round the Azores; there being no apparent reason for a Northward deflection of its flow, and no indication of such deflection other than that which *surface-drift* is quite sufficient to account for. For this takes up and carries onwards any floating bodies which the Gulf-stream has transported into the Mid-Atlantic; and the fact that these are carried rather to the West of Ireland, the Hebrides, the Orkney, Shetland, and Faroe islands, and even to Spitzbergen, than in the direct Easterly course towards the Bay of Biscay which the Gulf-stream has when it ceases to be recognizable as a distinct Current, seems a clear indication that their transport over the latter part of their course is essentially due, as was long ago suggested by General Sabine (§ 88), to the predominance of South-westerly winds over this portion of the area of the Atlantic.—It is well remarked by Dr. Hayes, that just as a ball fired from a cannon stationed on the top of a hill loses its velocity with every second of its progress, and falls to the

earth when its impelling force is exhausted, but may still roll down hill by the force of gravity, which is a new power applied to it, so may Gulf-stream water, and any thing which it floats, be drifted onwards by the action of the Wind, after the propulsive force of the Gulf-stream has died out.

150. If, then, there be no other Mechanical evidence of the extension of the Gulf-stream proper to Western Europe, than that which is afforded by "Rennell's Current," we have next to inquire what evidence can be adduced of its influence on the Temperature of the Eastern side of the Atlantic.—This question can be best answered by an examination of the course of the Isothermal lines to the eastward of the Meridian of 30°. For their passage across that meridian between the parallels of 34° and 50° at more regular intervals than are seen between them elsewhere, indicates that their position is there comparatively little affected by disturbing causes, either of elevation or depression; and further, the close relation which we there observe between the Winter Isotherms and the Summer Isotherms of 10° higher, indicates that on this meridian the seasonal movement takes place rather in accordance with general Climatic changes than under any special influences. For the *Summer* Isotherms of 75°, 70°, 65°, and 60° are here nearly equidistant, crossing the Meridian in the parallels of 34°, 41°, 46°, and 50°·5 respectively; while the *Winter* Isotherms of 65°, 60°, 55°, and 50° severally cross it at a distance of scarcely a degree from the preceding. Now whilst, as we have seen (§ 147), the summer Isotherm of 70° and the winter Isotherm of 60° turn *southwards* to the east of 30° W., the summer Isotherm of 65° and the winter Isotherm of 55° keep their course nearly due *eastwards* into the Bay of Biscay; while the summer Isotherm of 60° and the winter Isotherm of 50° in like manner keep an easterly course—a little tending to the north—towards the South of Ireland. There seems no reason whatever for believing that the ordinary temperature of the southern part of the Bay of Biscay is at all higher than the normal of the Latitude. Its winter temperature corresponds very closely with that of the Mediterranean at Toulon, which lies under the same parallel; while its summer temperature is much lower than that of the centre of Europe. We may, therefore, regard it as *ordinarily* out of the influence of the Gulf-stream; though *a band of water* possessing a temperature above that of the Ocean generally, is sometimes to be met with in a line which may be regarded as that of the Gulf-stream produced. Thus General Sabine, when proceeding, in January 1822, from Plymouth to Madeira, found the thermometer rise in crossing the Bay of Biscay from 49° in Lat. $47\frac{1}{2}$° to 55°·7 in Lat. $44\frac{1}{3}$°; an increase of 6°·7 being thus shown between one day and the next, although the difference of Latitude alone would account for no more than $2\frac{1}{2}$°. Further south the temperature was found as much *below* the normal as it here was *above* it. So again, Dr. Franklin, in his voyage, in November 1776, between the United States and France, found the temperature along the produced line of the Gulf-stream much higher than has subsequently proved to be usual; the thermometer standing at 62° in Long.

15° W., where subsequently, in August 1785, it only showed 66°. Further west, in the course of the Gulf-stream proper, great diversities are observable between the temperatures found at the same points in corresponding periods of different years *.—On the whole, then, it seems probable that the Gulf-stream varies considerably from time to time in force and in heating power; and that an extension of it into the Bay of Biscay may be sometimes distinguishable by local elevation of the Thermometer, although the *ordinary* temperature of the Atlantic as far north as the British Channel is in no wise dependent on its influence.

151. *Evidence of amelioration in the Climate of the British Isles, and of the region still further North, by a North-easterly flow of Oceanic water.*—It is when we pass northwards along the western shores of Europe, as far as the British Isles, that we first encounter, in the marked northward tendency of the Isothermal lines, especially in winter, an indication of *northerly* movement in the water of the North Atlantic. But it is important here to observe that the winter Isotherms of 55° and 50°, as laid down in Dr. Petermann's Charts, reach a considerably higher Latitude in the Meridian of about 25° W. than they do where they approach the coast of Europe in the Meridian of 10° W.,—the former, which nearly touches the parallel of 50°, turning south to Corunna in 44° N. Lat.; and the latter, which rises on that meridian to 54½° N. Lat., falling to 50° at the entrance of the British Channel. Now this may be taken as a very significant indication that the residual heat which the Gulf-stream may retain, after encountering the Arctic Current, is dissipated in the further passage of its water to the eastward. And the same inference may be drawn from the monthly averages of the observations of surface-temperature taken on board the Cunard Steam-ships between the South of Ireland and the Banks of Newfoundland; which show that while the winter (February) average only falls from 54° in W. Long. 40° to 53° in W. Long. 25° (although most of the change of Latitude occurs in that part of the course), it is reduced to 50° in W. Long. 10°. But the winter Isotherm of 45°, like the summer Isotherm of 55°, tends so continuously to the North, as to pass between the Shetland and the Faroe Islands; so that the whole western coast of the British Islands, having a range of more than *eleven degrees* of Latitude, lies between the July Isotherms of 60° and 55°, and the January Isotherms of 50° and 45°.

152. This northward extension of a Climate so little more severe than that of the South of England, bespeaks some very effective ameliorating agency; and the question now arises whether this agency is solely that of Wind, as maintained by Mr. Findlay and Dr. Hayes, or is in part exerted by Water. It is argued by Dr. Hayes that the temperature of the Sea *follows*, and is therefore *subject to* that of the Air, instead of *governing* it. Thus, he says, "We have seen and examined the Weather-register kept on "board the Light-ship off Sandy Hook. It shows that the temperature of

* See Rennell on Currents, pp. 276–283.

"the Water follows that of the Air with remarkable precision, whether on "the ascending or descending scale. The observations were made three "times daily, and they show a range throughout the year of from 29° "to 78°. This latter temperature was nearly as high as the highest tem-"perature of the Air on that day. The lowest temperature of the Air "was 4°, which brought the water down to freezing, and some ice was "formed. The average temperatures for nine consecutive days in July "were—Air 74°, Water 71°; for a corresponding period in February, "Air 32°, Water 31°." The following observations, again, are cited by Dr. Hayes to prove that comparatively warm water does not impart excess of warmth to the Air:—"At midnight, Feb. 22, 1869, the Pa-"cific Mail-steamer 'Alaska,' on her way to Aspinwall, was 148 miles "to the north of the Gulf-stream; the temperature of the Air at the "time was 22°, and of the Water 39°, the ship steering south. At 2^h "50^m P.M., the Air was 39°, the Water 46°, which is the general Winter "temperature of the Ocean-water in that Latitude on the U.S. coast. At "2^h 54^m, the 'Alaska' having entered the Stream, the Air was 40°, the Water "65°. Afterwards the Water rose to 70°, but the Air remained at 40° until "the stream was crossed: then the Air rose to 44°, while the Water sank "to 62°."—Dr. Hayes further cites, from a paper by Dr. Stork in the 'Nautical Magazine' for 1859, a series of observations which tend to prove that on the coast of Scotland the temperature of the Water of the Atlantic is regulated by that of the Air, rising and falling *after* it; so that the diurnal, monthly, and annual variations of the two show a close parallel, except that the fluctuations in the temperature of the Air are in advance of those of the temperature of the Sea.

153. This view, however, is in opposition to the results of the observations which have now been carried on by the Scottish Meteorological Society through a lengthened term of years*. For they show that on the West Coast of Scotland the *Mean Annual* Temperature of the Sea is from *two to three degrees higher* than that of the Air; whilst still further north, the difference is still more in favour of the Sea (§ 155). And they further show that this Mean Annual excess represents a very much larger *winter* excess; the *summer* temperature of the Sea being *below* that of the Air. For if we divide the whole year into three periods, each of four months, we find the average excess of Sea-temperature at Otter House, Loch Fyne, which is 2°·5 for the whole year, to be 6°·2 during November, December, January, and February, and to be only 2°·3 during March, April, September, and October; whilst during May, June, July, and August there is an average difference of 2°·1 in favour of the Air. These differences clearly show that the Sea here brings with it a temperature of its own, which tends to maintain an equability of Climate by moderating both

* "On the Temperature of the Sea between Scotland, Iceland, and Norway. By Dr. Keith Johnston and Alexander Buchan," in Journal of the Scottish Meteorological Society, April 1871, p. 146.

the summer heat and the winter cold.—It would probably be found, however, on a more detailed comparison, that even in winter the relative temperatures of the Sea and Air are much influenced by the direction of the Wind; as the greatest differences may be expected to present themselves when, after a continuance of S.W. winds which have produced a powerful drift of warmer water towards the coast *, the wind changes to the N.E., immediately reducing the temperature of the Air, and not for some time producing a like fall in that of the Sea.

154. It is further urged by Mr. Buchan, who has paid special attention to the Distribution of Temperature in the British Islands †, that the peculiar course of the *Isocheimal* lines, or lines of mean Winter temperature, indicates that the ameliorating influence comes from the *west*, rather than from the *south-west*, and thus from the Sea as well as from the Air. The course of the Isotherms between the months of April and October has a general relation to the parallels of latitude; the summer-temperatures of Ireland and the West Coast being, however, *lower* in August (Plate VI.) than those of the middle of England, showing that the neighbourhood of the sea has a reducing effect at that period. In November, however, the direction of the Isotherms begins to undergo a marked change; their western portions turning northward, and their eastern portions southward, whilst the intermediate parts run almost due north and south. Thus the Isotherm of 43° passes from Belfast across the Irish Sea to Whitehaven, and then turns southwards through Lancaster to Oxford; while that of 45°, after passing Cape Clear and Wexford towards Anglesea, there takes a direct southward bend which carries it to Exeter. This meridional direction shows itself still more strongly in December,—the line of 42°, which passes through Belfast, turning southwards so as to pass through Liverpool and Bristol, and thence S.E. to Brighton and Dieppe; while the line of 41° ranges nearly north and south from the Shetland and Orkney Islands to Dover, and that of 40° runs parallel to it from the Moray Firth to Cromer. It is in January, however, that this Meridional direction is most pronounced (Plate VI.),—the Isotherm of 39° passing from Unst along the Western coast of Scotland, and then through the centre of England to Hastings; and the Isotherms of 38° and 37° lying parallel to it on the east, while those of 40° and 41° show a like parallelism to it on the west. In Ireland the Isocheimals "seem to envelope the island with their folds, which increase in warmth from the centre of the island outward to the ocean." In February, however, the influence of the sun begins to show itself in the tendency of the Isotherms to exchange their meridional direction for that of the parallels of Latitude; and this is still more marked in the March Isotherm, that of April showing the change completed. Now, as Mr.

* I learn from a friend who resides at Sidmouth, that the Sea-temperature always sensibly rises there with a S.W. gale.

† "The Temperature of the British Islands," in Journal of the Scottish Meteorological Society, vol. iii. 1871, p. 102.

Buchan points out, if the warming influence were derived solely from Winds, the distribution of the Isocheimals or Winter Isotherms would be very different.

155. The recent extension, by the Scottish Meteorological Society, of the same accurate system of observations to the Faroe Islands and Iceland, and by the Meteorological Institute of Norway to various points on the Norwegian Coast, has brought out still more strongly the fact that the Climate of these stations is moderated, not merely by their proximity to the Sea, but by the warmer temperature, derived from a Southern source, which the Sea brings with it. At Thorsavn (Faroe), the Mean Annual excess of Sea-temperature, its maximum in the Winter months, and the reversal of the difference between the Air and Sea in the Summer, correspond closely with the similar averages on the West of Scotland. At Reykjavik the Mean Annual excess of Sea-temperature is a little higher, but the Winter maximum is not so great, and in the Summer the temperatures of the Sea and Air are nearly equal. At Stykkisholm the Mean Annual excess of Sea-temperature rises to 4°·2; the higher average being due not so much to an increase in the Winter excess, but to the continuance of a sensible difference in favour of the Sea through the whole Summer, excepting in the month of May.—It is at the Norwegian Stations, however, that the excess of Sea-temperature shows itself most strongly, and particularly at Fruholm near the North Cape; where, the Mean Annual excess being 6°·1, the mean excess of the four Winter months is 14°·5 (that of December alone being nearly 17°), the mean of the four Spring and Autumn months being 6°·3, while the excess of Air-temperature in the four Summer months averages only 2°·6.

156. It can scarcely be doubted, then, that a general North-easterly surface-movement of Oceanic water is taking place in the portion of the North Atlantic which lies between Iceland and Scandinavia; and of the existence of such a movement adequate evidence has already been adduced (§14). This movement, says Admiral Irminger*, "is the cause why "the harbours of Norway, even further than the North Cape, and as far "as the Fiord of Varanger, are accessible for navigation during the whole "year; just as the warm current which passes Cape Reikianäs, and runs "to the northward along the western shores of Iceland, is the cause of the "south and west coasts of this island being clear of ice, so that even "during the severest winters ships may go to Havne Fiord and other "places in the Faxe Bay of Iceland, where they will always be sure "of finding open sea. If this current to the North in the Atlantic did not "exist, the ice from the sea round Spitzbergen would float down to more "southern latitudes than is now the case; and certainly the coasts of Nor-"way, as well as the sea between Shetland and Iceland, would frequently "be filled with ice from the Icy Sea, and the influence of the ice would "then be felt on the climate of the neighbouring coasts."—That this

* Proceedings of the Royal Geographical Society, May 10, 1869, vol. xiii. p. 227.

"slow current," however, is not a mere superficial drift, will appear from the considerations formerly stated (§ 13).

157. But independently of this general N.E. Oceanic movement, the Temperature-observations which have been correlated by Admiral Irminger *, and another series more recently collected and correlated by Prof. Mohn of Christiania †, indicate that its surface is traversed by *bands* of somewhat greater warmth. Admiral Irminger specially notes the existence of two such bands,—one of them a little to the west of Fair Isle, which is regarded by Dr. Keith Johnston and Mr. Buchan (on the basis of Prof. Mohn's data) as the axis of this "slow current," its temperature being sensibly higher than the temperature to the west or east of it; whilst the other, the position of which is more variable, is met with much further to the westward, sometimes even beyond the meridian of the southernmost point of Iceland. —These bands, the existence of which has been lately confirmed by Von Middendorf (§ 160), are regarded by Admiral Irminger (as it seems to me with great probability) in the light of real continuations of the "Gulf-stream proper," which are not only deflected northwards, but also carried onwards by the general N.E. "set." Admiral Irminger, moreover, without refusing to the "Gulf-stream proper" some share in the amelioration of the Climate of the regions towards which these bands proceed, distinctly expresses his conviction that this amelioration is mainly due to the heat brought by W. and S.W. winds from the "great and broad Atlantic Ocean."

158. Having thus found, in the Temperature-phenomena of the British Islands and of the Seas immediately adjacent to them, very decided indications of a general N.E. movement of Oceanic surface-water, we shall next trace the relation of these phenomena to the Thermal condition of the portion of the North Atlantic that extends from Newfoundland to the Arctic Sea, as indicated by the course of the Isothermal lines laid down in Dr. Petermann's Gulf-stream Charts for January and July. These show (Plate VII.) in a very marked manner the influence of the Labrador current, which bears southward a great body of Polar ice and glacial water, in depressing alike the summer and the winter Isotherms on the western side of the North Atlantic; the influence of this current in *lowering* the temperature of the portion of the Ocean in which it meets the Gulf-stream being probably fully as great as that of the Gulf-stream in *raising* it.—It is curious to see how sharply the *summer* Isotherms of $54\frac{1}{2}°$, $50°$, and $45\frac{1}{2}°$ turn northwards to the east of the Banks of Newfoundland: diverging from one another and from the Summer Isotherm of $60°$ at intervals which are pretty nearly equal almost as far to the East as the meridian of $30°$ W.; but then again trending strongly to the North, so that the summer Isotherm of $54\frac{1}{2}°$ crosses the parallel of $60°$ N. before, by a slight trend to the South, it passes through the Pentland Firth. Thence crossing the North Sea, this Isotherm passes along the coast of Norway as far as Tromsö (very near the parallel

* Journal of the Royal Geographical Society, vol. xl. (1870), p. 441.

† 'Température de la Mer entre l'Islande, l'Écosse, et la Norvége,' Christiania, 1870.

of 70°), and then turns southwards along the land, keeping within the coast-line of Russian Lapland, and passing across the narrow throat of the White Sea. The summer Isotherms of 50° and 45½° cross the mouth of Baffin's Bay, and then follow the curve of the coast of Greenland towards Iceland; when approaching which they turn eastwards, the line of 50° striking the land at Stykkisholm on the N.W., while the line of 45½° passes altogether to the north of the island. To the east of Iceland the Isotherms take a southerly bend, apparently under the influence of a drift of ice from the Polar Sea; but soon turn northwards again,—the line of 50° running nearly parallel to the coast of Norway as far as the North Cape, and then turning southwards along the coast of Russian Lapland, so as to cross the mouth of the White Sea to the base of the Kanin Peninsula; while the line of 45½° runs parallel to this as far north as Lat. 72½°, and then turns southwards, still retaining the same parallelism, so as to strike the coast of Russia beyond that peninsula. Still further north, we find the summer Isotherms of 41° and 36½° showing a nearly W. to E. direction until they have passed the meridian of 10° W., and then suddenly turning northwards; the line of 36½° passing up to the west of Spitzbergen as far as 82° N., and also extending itself irregularly eastwards along the parallel of 75° as far as Nova Zembla.

159. The course of the *winter* Isotherms of 45½°, 41°, 36½°, and 32°, as shown in Dr. Petermann's Chart, is no less significant; for they all turn sharply to the North on the eastern side of the Banks of Newfoundland, cross the entrance of Baffin's Bay, and then keep a course of general parallelism to the coast of Greenland, crossing the meridian of 30° W. at almost equal intervals. The winter Isotherm of 45½° follows almost exactly the course of the summer Isotherm of 54½° as far as the Shetland Islands: but it then turns back on itself so as to form a loop, passing southwards along the Western Hebrides towards Belfast. The course of the winter Isotherm of 40° in like manner at first bears a general correspondence with that of the summer Isotherm of 50°, skirting the south coast of Iceland, and then passing N.E. in the channel between Iceland and Norway; but in Lat. 67½° N. *it* also returns in a loop, which brings it back to the *east* coast of Scotland. The winter Isotherm of 36½°, again, corresponds very closely with the summer Isotherm of 45°; passing through Iceland, and then keeping a N.E. course which carries it far to the north and east of the North Cape, when it, too, forms a loop bringing it back to the coast of Russian Lapland. Finally, the winter Isotherm of 32° proceeds along a similar course from the Banks of Newfoundland to the northernmost point of Iceland, and then onwards towards Jan Mayen, beyond which it has not been traced.

160. That this remarkable course both of the Summer and of the Winter Isotherms can only be accounted for by a N.E. flow of warm water, I am as strongly convinced as Dr. Petermann can be: and that this movement must be something very different from a mere surface-drift, seems to me

equally certain; since, unless the warm stratum is of considerable depth, it could not possibly retain that excess of temperature which it carries with it into high latitudes.—If any further evidence to this effect were wanting, it has been supplied by the recent observations made by Von Middendorf in the Voyage of the Russian Corvette 'Warjäg' between Archangel, Iceland, and Nova Zembla, in the summer of 1870 *; some of the most important of which will now be cited:—

a. The existence of *alternating warm and cold bands*, as affirmed by Admiral Irminger, was confirmed. On the 17th of June, 1870, a temperature of 55°·6 was observed off the coast of Norway, north of 60°; and in July a temperature of 54½° was observed in N. Lat. 69⅝°, nearly in sight of the islands off Tromsö, in N. Lat. 64° in the roads of Reikiavik, and in N. Lat. 61¼° on the meridian of the centre of Iceland. On the other hand, near the Lofoten Islands the surface-temperature fell to 4‾°; and a minimum of 42°·1 was observed in N. Lat. 64½°.

b. The *thickness of the warm stream* is shown by the fact that in N. Lat. 69⅓° and W. Long. 14°, the surface-temperature being 50°·7, the temperature at 40 fathoms was found to be still 46°·4, and at 84 fathoms 45°·5.

c. The North-Cape stream, hardly perceptibly cooled from 54°·5, runs past the White Sea and the Kanin Peninsula toward the entrance of the Kara Sea; so that in the vicinity of Kolgujev Island (N. Lat. 68°) there are still bands which have in July nearly the same temperature.

d. On the meridian of the Kanin Peninsula, the North-Cape stream, which may be there called the Kanin stream, has a breadth of more than 2° of Latitude, with a range of temperature between 55° and 47°·7. The higher the temperature is on the surface, the more rapidly does it fall beneath it; but at 30 fathoms it is still between 42° and 38°·7. The Kanin stream appears to divide at Nova Zembla; its main branch going onwards into the Kara Sea, whilst a side branch turns northwards along the west coast of Nova Zembla. Another portion, however, striking against the Kanin Peninsula, seems to turn inwards along the east coast of the White Sea, the temperature of which is moderated by it (especially with N.E., N., and N.W. winds) as far as Dwina Bay. The western coast of the White Sea, on the other hand, is bordered by a cold stream, the temperature of which is probably the local temperature corresponding to the region. Thus, in passing round Cape Swätoi at the beginning of July, the thermometer fell to 42°·6, and further *south* to 39°·9; whilst a month later, 1⅓° further *north*, on the same meridian, the temperature was 51°·1.

e. It is considered by Von Middendorf that "the Gulf-stream can "still be detected at Kolgujev, not only by the temperature, but also by "the blue colour and high salinity of the sea."—"We sailed there "through water of so deep a violet-blue, that I was confident of finding it "swarming with microscopic animalculæ and plants. My astonishment

* Geographische Mittheilungen, Jan. 1871.

"was great when I could not detect any thing under the microscope."—As this distinctive blue colour has not been observed in the North-Cape stream, I should be disposed to attribute it to the diffusion of the fine sedimentary particles brought down by the Dwina and Mezen rivers (Report for 1870, §§ 98, 99). The Specific Gravity of the water of the Kanin stream being only 1·025, it is high only in relation to that of the water of the White Sea, which is reduced by the large quantity of river-water discharged into it. At Cape Swätoi, which does not lie in the supposed Gulf-stream, the Specific Gravity of the water was found to be 1·026.

f. The Zoological researches of Th. Jarshinski (1869) along the Murmanian (N.E. Lapland) coast of the Polar Sea, are stated by Von Middendorf to prove the affinity of its Fauna with that of the Atlantic Ocean.

g. The remarkable agreement of the temperature of the Air with that of the Water, and the manifest dependence of the temperature of the Air on that of the Water, induce Von Middendorf to adopt, without hesitation, the doctrine of direct heating by warm water. "We should have been "able," he says, "to determine by the temperature of the Air, without "ascertaining that of the Water, whether we were or were not within the "warm water of the extension of the Gulf-stream. The direction of the "wind had evidently but a subordinate influence on the temperature of the "Air."—It is expressly stated by Von Middendorf, that when speaking of "currents" he does not intend to imply more than the result of Temperature-observations, which indicate a flow of warm water from the west.

161. The Temperature-observations recently made by Messrs. Weyprecht and Payer (§ 15) in still higher Northern Latitudes, show that *this warmer surface-layer rapidly thins off towards the north-east*; and that instead of a *rise* of Temperature with increase of depth in the Polar area (which is the doctrine still maintained by many Physical Geographers), there is a rapid *reduction*,—glacial water being found at a less and less depth in proportion to the Northing obtained, as I ventured to predict in my last Report ('Proceedings,' vol. xix. p. 190) would prove to be the case.

162. Further, the observations collected by Dr. Mühry* relating to the Temperature of the Western coast of Greenland, seem to indicate a northward flow of comparatively warm water along that side of Baffin's Bay, in antagonism to the Polar current which flows southwards on the Labrador side. It is certain that the climate of the south-western coast of Greenland is much milder than that of either its eastern coast or the eastern coast of Labrador under the same parallels; and the summer Isotherm of 41° is carried northwards in Dr. Petermann's Chart nearly as far as Upernavik, while the summer Isotherm of 36½° extends in 70° W. Long. to Smith's Sound, as in 10° E. Long. it extends to Spitzbergen. According to Admiral Irminger, a distinct current can be traced as far north as Lat. 64° or even 67°; and this brings with it *Mimosæ* and other tropical products,—such as a mahogany log, found on Disco Island, which was

* Geographische Mittheilungen, 1854, p. 187.

made into a dining-table for the Governor of Holsteenborg. Moreover it has been noticed by several trustworthy observers, that icebergs often move *northwards* in Davis's Straits, in opposition to a *southerly* moving surface-current, as also against the wind. And although it might seem anomalous for a warmer stream to be flowing beneath a colder one, yet the anomaly disappears when we bear in mind that the surface-current, being composed in great degree of water which is the product of the liquefaction of icebergs, may have its salinity so reduced as to be specifically lighter than ordinary Oceanic water of higher temperature (§ 15). Although this warmer north-flowing body of water is assumed by Dr. Mühry to be a branch of the "Gulf-stream," there is a total absence of indication that it is derived from any other source than the general North-Atlantic area.

163. I come, lastly, to consider the question :—*Whether the North-easterly flow of relatively warm water, by which the Climate of the British Islands, and still more that of regions lying within the Arctic circle, is ameliorated, is attributable to the persistence of the initial force of the Gulf-stream proper, supplemented by the general Easterly surface-drift of the North Atlantic ; or whether it is a larger and deeper movement, forming part of a General Oceanic Circulation sustained by difference of Temperature.*—As the direction of the Gulf-stream proper, where it is last to be recognized as a distinct Current, is nearly due East, and as the incidence of the Polar current off Newfoundland on its northern side will tend to give it rather a southward than a northward direction, the *sudden northward deflection* of the Isothermal lines on the eastern border of the Polar current, and their course of parallelism to the Greenland coast (which is as marked as the parallelism of the Gulf-stream to the coast of the United States as far north as Cape Hatteras), do not seem explicable on the idea of a *vis à tergo*; which must not only extend itself over the vast Oceanic area that intervenes between the eastern coast of Greenland and Scandinavia, but must change the direction of its operation without any assignable cause. Still less can it be shown, on this hypothesis, how a branch of the Gulf-stream should actually turn "round the corner" *westwards* into Baffin's Bay, so as to pass from W. Long. 50° to W. Long. 70°; and this notwithstanding its excess of *easterly* momentum, which would tend to carry any *north*-moving stream in the direction of Spitzbergen. Further, when we take account of the retardation which the Gulf-stream is known to undergo when flowing in the early part of its course as a compact and powerful current, it seems scarcely conceivable that it can long retain enough of its initial force to continue to make its way, when superficially extended over so vast a surface, against the retarding influence of the increased friction to which that extension must subject it with the watery bed over which it flows. And though it may be said, in answer to this objection, that its movement will be aided by the prevalent surface-drift, yet the influence of this can only extend to a very small depth ; and the advocates of the "Gulf-stream" doctrine take

their stand upon its prolongation as a "*deep*, warm, voluminous current." Under no other condition than this, indeed, can it retain its heat for any considerable duration, beneath an Atmosphere whose temperature is much below that of its surface (§ 155); for since, as we have seen (§ 142), the Gulf-stream, ***even at its deepest and strongest***, loses in winter ***fifteen degrees*** of surface-heat while passing from the Florida Channel to the south of Nova Scotia, it must part much more rapidly with any excess it may still retain, when its superheated stratum has been thinned out by superficial extension, and when the progressive reduction of its movement to a rate *per day* not exceeding its rate *per hour* in the most rapid part of its course, prolongs its exposure from days to weeks, and from weeks to months.

164. I conclude, then, that ***nothing save the general Northerly movement of a stratum of Water deep enough to retain an excess of heat through a lengthened period, whilst flowing beneath an Atmosphere of lower temperature than its own***, can account for the Thermal phenomena indicated in Dr. Petermann's Gulf-stream Charts. Of such a movement it has been shown that the Temperature-soundings taken in the 'Porcupine' Expeditions afford very distinct evidence (§ 13). On the other hand, the very fact of its extension to so great a depth appears sufficient to negative the idea that it is to be accounted for either by the *vis à tergo* of the Florida current, or by the drift-action of S.W. winds on the surface.

II. On the Dardanelles and Baltic Under-currents.

165. When discussing, in the Report of the 'Porcupine' Expedition for 1870, the Physical Theory of the Gibraltar Current, I endeavoured to strengthen my case by showing the applicability of that Theory, *mutatis mutandis*, to two other cases in which all the conditions of the Gibraltar Current are reversed together,—an Under-current of the heavy water of the German Ocean having been proved to flow inwards through the Baltic Sound, beneath the outward surface-current of lighter Baltic water; whilst the existence of a similar inflow from the Ægean into the Black Sea, beneath the outward surface-current of the Bosphorus and Dardanelles, might be predicated (I ventured to affirm, § 123) "on the double ground of *à priori* and *à posteriori* necessity."—I much regret that I was not at that time acquainted with the experiments and observations which had been made by Capt. Spratt on the Dardanelles Current some years previously*; since, if they *had* fallen under my notice, I should have cited them as affording probative evidence of the truth of my position. For although the conclusion which Capt. Spratt has himself deduced from them is diametrically opposite to my own, yet the fallacy of that conclusion

* In excuse for my ignorance of Capt. Spratt's inquiries on this subject, I may point out that the 'Travels and Researches in Crete,' in which they are published as an Appendix, is not a work into which any one seeking information upon the Dardanelles Current would naturally look.

appears to me so readily demonstrable, that I should have thought it sufficient to point out what I regard as the just interpretation of them, leaving it to the scientific public to decide whether his or my view of the case has the stronger claim to acceptance.

166. But as Capt. Spratt has made his experiments on the Dardanelles Current the basis of an assault*, not merely on the position I had ventured to assume (on theoretical grounds only) in regard to that current, but also on the Under-current doctrine which I had put forward as experimentally substantiated in the case of the Gibraltar and Baltic Currents, and, by implication, on the Doctrine of a General Oceanic Circulation which the inquiries I have recently had a share in conducting appear to me to justify, it is necessary for me to address myself to a more formal refutation of his arguments.

167. It may be thought presumptuous in one who can only claim to possess a merely elementary knowledge of Physical Science, and a very limited amount of practical acquaintance with inquiries of this nature, to call in question the unhesitating *dictum* of a Surveying Officer of Capt. Spratt's acknowledged ability and large experience, in regard to a matter lying completely within his own province. For however simple the case may *appear*, it might naturally be thought more probable that some essential condition had been left out of view on my part, than that Captain Spratt's interpretation of his results should be *precisely the reverse* of that which his own data necessitate. In order, therefore, to secure myself against any such error, I have submitted the case to several Naval Officers who have made a special study, both theoretically and practically, of all matters relating to Currents; and finding that their judgment entirely accords with my own, I venture to believe that the validity of my claim to a complete reversal of Capt. Spratt's conclusion will be generally admitted.

168. *Dardanelles Current.*—The method of experimenting adopted by Capt. Spratt was so nearly the same as that followed by Capt. Nares (under instructions from the Admiralty) in the experiments on the Gibraltar Current, of which the detail has already been given, that it is only requisite to recall its two essential features.—A float or buoy was attached to a sinker resting on the bottom, so as to serve as a fixed point of reference; this we shall call, as before, the *anchored buoy*. To another float or buoy, exactly resembling the first, was suspended at any desired depth a "current-drag," offering a large surface of resistance to any current in which it might hang; and the float which supports this we shall call (as before) the *current-buoy*.

169. Now I quite agree with Capt. Spratt, that if a surface-current flows past the "anchored buoy" and the "current-buoy" *at the same rate*, the current-drag suspended from the latter must be as motionless as the sinker to which the anchored buoy is attached; but I differ from him

* Proceedings of the Royal Society, vol. xix. p. 528 *et seq.*

in toto as to the cause of its stationary condition: for whilst *he* concludes that the current-drag remains stationary because it hangs in *perfectly still water*, it appears to me beyond question that it can only remain stationary whilst acted on by *a current flowing in the opposite direction.*

170. For let us consider (1) the condition of the "anchored buoy" lying in a surface-current that runs at the rate of (say) a mile per hour. The resistance offered to that current, not only by the buoy, but by the upper part of the anchoring line, puts that line on the stretch, so as to bring a very considerable strain alike on the buoy and on the sinker at the bottom.—In our own experiments in the Strait of Gibraltar, this strain was found to be so great, that a current of 1·25 mile per hour was sufficient to submerge a buoy capable of floating a weight of 100 lbs.; and when another buoy was attached to this, so as to keep it up to the surface, the strain of the current on the two proved so great as to lift and displace the sinker at the bottom, though it was a mass of iron weighing 1 cwt. —Let us suppose (2) the "current-drag" to be freely suspended in *perfectly-still water*, at any depth whatever above the bottom; it is obvious that, as the pressure of the surface-current upon its buoy and suspending line is precisely the same as in the preceding case, such pressure will put a strain upon the line, which will draw the "current-drag" along through the still water, in the direction of the surface-current, but at a reduced rate*; the flow of the current past the "current-buoy" being diminished by the amount of that motion.

171. Now if this be admitted, it necessarily follows (3) that if a "current-drag" suspended from a buoy floating in a surface-current remains motionless,—as shown by the equality of the rates of flow of the surface-current past the "current-buoy" and the "anchored buoy,"—the strain put upon its suspending line by the resistance of its current-buoy to the surface-current *must be neutralized by the pressure in an opposite direction* of an Under-current meeting the "current-drag."

172. And since, according to Capt. Spratt, this stationary condition of the "current-drag" was shown at all depths below 40 fathoms in the Sea of Marmora (even down to 400 fathoms), and at all depths below 20 fathoms in the Dardanelles, whilst there is a rapid superficial *out*-current, running in the Dardanelles at the rate of 2½ miles per hour, it seems an irresistible conclusion that there is a deeper Under-current from 20 fathoms to the bottom, running more slowly *in*wards from the Ægean into the Sea of Marmora through the Dardanelles, and thence (it may be presumed) through the Bosphorus into the Black Sea. And this conclusion finds complete confirmation in the results of a comparison between the respective densities and rates of movement of the Dardanelles water at different depths, as observed by Capt. Spratt himself. For whilst the progressive

* It is not a little singular that Capt. Spratt admits this action (p. 535) in a case in which the "current-buoy" continued to move with the surface-current at the rate of 0·1 knot per hour; whilst refusing to admit it when the "current-buoy" was stationary.

decrease in the movement of the "current-buoy," from 2¼ knots at the surface to almost nothing at 20 fathoms, indicates (as just now shown) first a cessation of all movement in the stratum in which the "current-drag" hangs, and then a reversal in the direction of the current as the lower depth is approached,—the Specific Gravity was found to increase from 1020 at the surface to 1028 at 20 fathoms and 1029 at 40 fathoms; the surface-water thus corresponding with that of the Sea of Marmora, whilst the water of the entire stratum from 20 fathoms to the bottom was equal in density to that of the Mediterranean.

173. I hold, then, that the existence of an Under-current of dense Mediterranean water through the Dardanelles into the Sea of Marmora is incontestably proved by the very experiments and observations which have been adduced by Capt. Spratt as demonstrating the unsoundness of the Under-current doctrine*.

[ADDENDUM (Oct. 21, 1872).—Information has just reached this country, through the 'Levant Herald,' that the investigation of the Dardanelles Current made by the Surveying Staff of the 'Shearwater' since the above paragraphs were written on board her, has confirmed my original prediction, and entirely justified my interpretation of Captain Spratt's experiments. For, according to the statement of this journal (obviously made on authority), a boat was carried along by a "current-drag" suspended in the Under-current, *in opposition to the surface-current*, at a rate greater than the speed of the 'Shearwater's' steam-launch; and this under-current has been ascertained to run at a depth of 20 fathoms,—precisely that at which the results of Captain Spratt's experiments led me to predicate its existence.

I venture to think that this complete verification of my prediction will be regarded as a confirmation of the general Physical Theory of Under-currents on which it was based: and it is now incumbent on those who oppose that Theory, to show by *what other force* than the difference in the weight of the Ægean and Black Sea columns, arising from their great difference in Specific Gravity, the Dardanelles Under-current can be sustained.

That a similar Under-current exists in the Bosphorus, has been (as I am assured by my friend Mr. Redhouse, who resided many years at Constantinople as Translator to the British Embassy) long known to the native

* It furnishes no argument against this conclusion, that (as stated by Capt. Spratt) during the winter months, when the Black-Sea rivers are at their lowest, a return *surface*-current is propelled into it by Westerly winds, *from* the Ægean, through the Dardanelles, Sea of Marmora, and Bosphorus. For the direction of the *surface*-current is entirely a question of Wind and of relative level; and the occasional reversal of that direction does not prove the non-existence of an inward *under*-current sustained by difference of vertical pressure. How a *surface* wind-current can bring into the Sea of Marmora the *deep* stratum of Ægean water which is proved by Captain Spratt's own Specific Gravity observations to overlie its bottom, he does not explain.

fishermen of Constantinople, as well as to European residents, who amuse themselves with the sport.

Further, I learn from a very careful observer, that at a time when the surface-current in the Dardanelles, urged by a S.W. wind, was setting *inwards*, as described by Captain Spratt, he noted that the *sub*-surface-movement, indicated by the direction of the water-weeds, was still *outwards*; thus showing how small must be the quantity of Ægean water carried into the Black Sea by this agency.]

174. *Gibraltar Current.*—As the experiments and observations of which I have given the details in the present Report furnish the best reply to Capt. Spratt's criticisms upon the work of the preceding year, I should have left those criticisms entirely unnoticed, were it not that one of them is founded upon a misconception of the facts of the case, which, in justice to my valued fellow-labourer Capt. Calver, I cannot leave uncorrected. It will be remembered that in two cases (Station 64) we actually *did* obtain the evidence, which, on Capt. Spratt's own admission, would be adequate (if valid) to establish the existence of an Under-current; namely, the movement of the boat from which the "current-drag" was suspended, in a direction *opposed* to that of the surface-current in which the boat was floating. So determined, however, is Capt. Spratt not to admit the cogency of this evidence, that, in order to invalidate it, he has recourse to the hypothesis that instead of the boat being carried along by the "current-drag," it was the "current-drag" that was carried along by the action of the wind and surface-current on the boat, which he supposes to have been allowed to drift "exposed nearly broadside on to an easterly wind, and therefore following swell." Now the fact was, that the boat was carefully kept by the two men in her with her stern to the wind; and that the "current-drag" (as was shown by the direction of the line suspending it from the bow of the boat) was always *in advance of it*; the action of the wind being so far from sufficient to neutralize that of the surface-current, that the men were obliged occasionally to use their oars to keep the boat sufficiently up to the drag. The whole operation was most carefully watched by Capt. Calver from the deck of the 'Porcupine,' which was kept close to the boat during the entire experiment; and as he assured me that he considered its result to be most conclusive, and as I reported that result expressly on his authority, I cannot but think that Capt. Spratt was scarcely justified in impugning the validity of the experiment, on no better grounds than a mere surmise suggested by his own unwillingness to accept its result.

175. *Baltic Current.*—While professing to disprove the conclusion drawn by Prof. Forchhammer from his observations on the relative Specific Gravities of the upper and under strata of the water in the Baltic Sound, Capt. Spratt altogether ignores the two facts by which the existence of an inward Under-current in that channel has been demonstrated with a com-

pleteness adequate (it might have been thought) to satisfy even his requirements. For it was stated two hundred years ago by Dr. Smith (who first advanced the hypothesis of the Gibraltar Under-current), on the authority of an intelligent Seaman who took part in the experiment, that a boat having been taken into the mid-stream of the Sound, where it was carried along violently by the outward current, a bucket was sunk with a heavy cannon-ball to a certain depth of water, which gave a check to the boat's motion; and that on sinking the bucket still lower, the boat was carried to windward against the upper current. The surface-current seemed to be not more than four or five fathoms deep; and the under-current was found to increase in strength, the lower the bucket was let fall*. —Again, it was stated by Prof. Forchhammer† that a steamer having been sunk, some years since, by a collision near Elsinore, a diver who went down to save the passengers' goods, found a strong Under-current running towards the Baltic.—Now since demonstrative evidence of the opposite movements of the upper and under strata, to which no exception has been taken by Captain Spratt, is here also in complete accordance with the inferences drawn from the contrast between their Specific Gravities, I must own myself unable to see by what right he excludes the case of the Baltic from the general Physical Theory which I have brought to bear upon it ‡.

176. But further, since the date of Capt. Spratt's communication, an important series of Researches on the Physics of the Baltic § has been published by Dr. H. A. Meyer, who has been led, by his independent and long-continued inquiries, to feel assured of the existence of an inward under-current through the Baltic Sound, and who has been good enough further to inform me by letter that he has obtained additional evidence to the same effect of a still more satisfactory character. He quotes, also, the following observations made by Capt. Patton, R.N., in 1821:—The ship which he commanded having had occasion to anchor some miles from Elsinore, he found a surface-current running from the Baltic at the rate of four miles an hour by the log. Upon dropping the lead, in order to ascertain the depth of water, which was about fourteen fathoms, he found the line continue perpendicular from his hand, while the lead itself was raised a little from the ground. Hence he concluded that an under-current equally rapid with that of the surface had prevented the lead

* Philosophical Transactions, vol. xix. p. 364.

† *Ibid.*, 1865, p. 230.

‡ Here, again, as in the case of the Black-Sea currents, Capt. Spratt considers it a sufficient disproof of the "Under-current Theory" to cite the fact mentioned by Prof. Forchhammer, that for 24 days out of 134 the *surface*-current runs *in*wards past Elsinore. This only shows that the level of the Baltic was then reduced, so that heavier water flowed back into it from the German Ocean; and does not in any way disprove the existence of an *in*ward under-current when the surface-current is running *out*wards.

§ Untersuchungen über physikalische Verhältnisse des westlichen Theiles der Ost-See. Ein Beitrag zur Physik des Meeres, von Dr. H. A. Meyer, Kiel.

and line from yielding to the opposite motion of the fluid, as they would have done had the ship been sailing at that rate through the water*.

177. *General Oceanic Circulation.*—The animadversions which Capt. Spratt has made upon what he terms "the universal Under-current Theory so fascinatingly advocated by Maury and others, and more recently by the late Dr. Forchhammer," have no real bearing upon the doctrine of a General Oceanic Circulation sustained by differences of Temperature alone, which I have recently been led to advocate. I do not in the least call in question the production of a *horizontal* circulation by Wind-currents; on the contrary, I have expressly pointed out, in the case of the Gulf-stream, what I believe to be the mode in which that circulation is completed: and I have no belief that, save under peculiarly exceptional conditions, any thing like a *current* can be produced by differences of Temperature alone. But to the question how the Temperature-phenomena which I have correlated are to be explained, except upon the supposition of a *continual movement* of Polar water, *however slow*, along the Ocean-bottom towards the Equator, involving as its necessary complement a flow of Equatorial water towards the Poles, Capt. Spratt does not seriously address himself; the Temperatures he cites being those collected in the older voyages, to which he has applied a correction of 4°, which is inadequate whenever it has reference to depths beneath 1200 fathoms. When the contrast I have drawn in the first part of my present Report between the Temperature-phenomena of Inland Seas,—such as the Mediterranean, the Red Sea, and the Sulu Sea,—with those of the Oceans with which they respectively communicate, shall have been rationally accounted for on *any other* hypothesis than that of a General Oceanic Circulation, the merits of the two explanations can be fairly discussed. But as such a discussion will be far more satisfactory when that vast mass of additional data shall have been collected which is likely to be furnished by the Scientific Expeditions that are at present either in progress or in preparation, I would suggest the expediency of postponing it until it can be more profitably resumed. At present, as I have already said, I claim for the doctrine of the General Oceanic Circulation no higher a character than that of "a good working hypothesis," consistent with our present knowledge of facts, and therefore entitled to be *provisionally* adopted for the purpose of stimulating and directing further inquiry.

* Edinburgh Philosophical Journal, vol. iv. (1821), p. 245.

DIAGRAM I

Comparison between Deep Temperatures of Open and of Land-locked Seas.

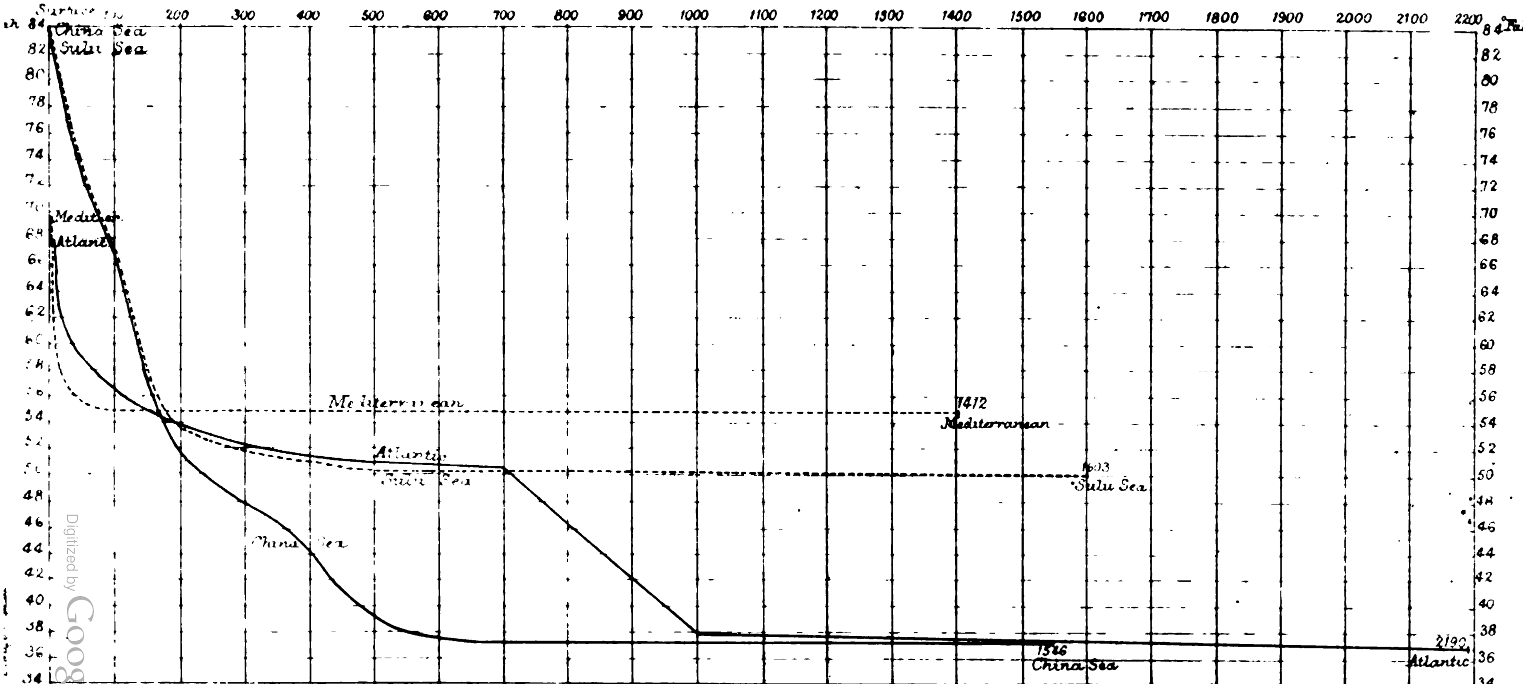


DIAGRAM II.

Rate of Reduction of Temperature in Superficial Stratum of Mediterranean

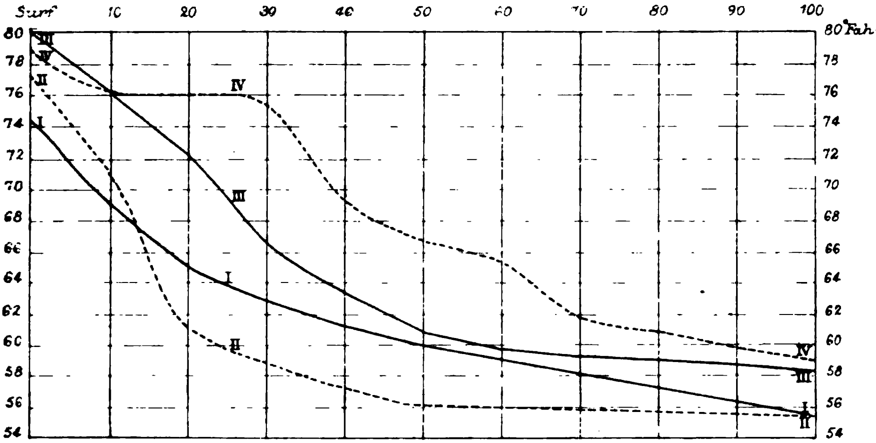


DIAGRAM III.

Longitudinal Section in Course of Gulf Stream showing Bathymetrical Isotherms

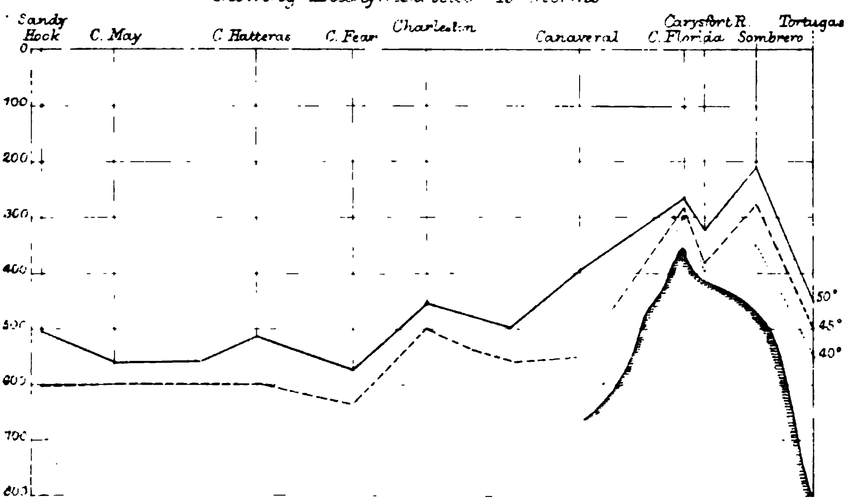


DIAGRAM IV.

Map showing lines of Section across Florida Channel

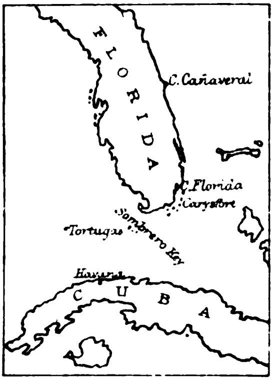


DIAGRAM V

Section off Sandy Hook

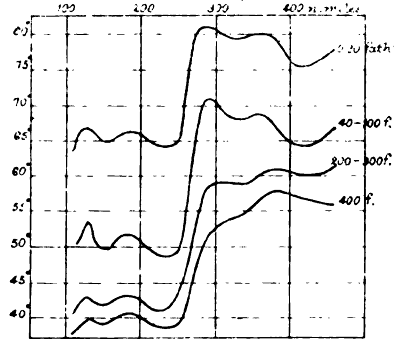


DIAGRAM VI

Havana Section

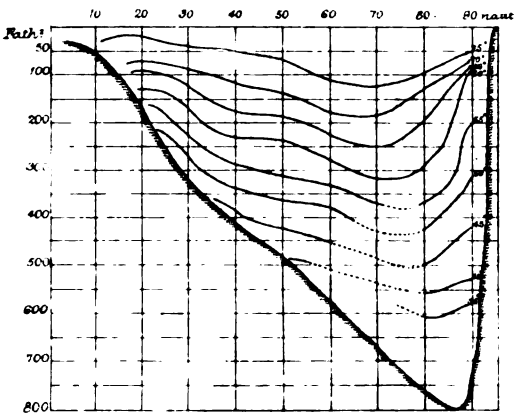


DIAGRAM VII

Sombrero Section

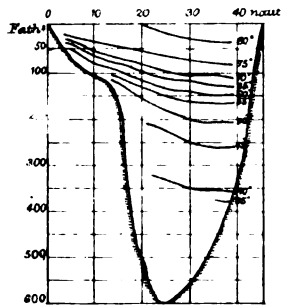


DIAGRAM VIII.

Carysfort Section

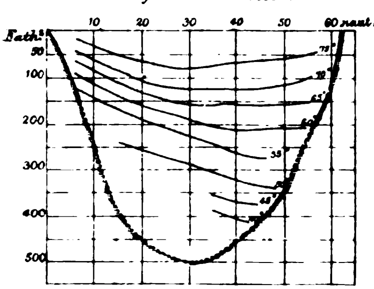
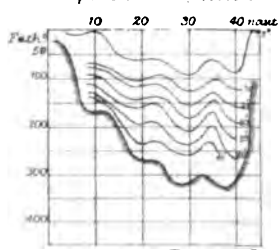
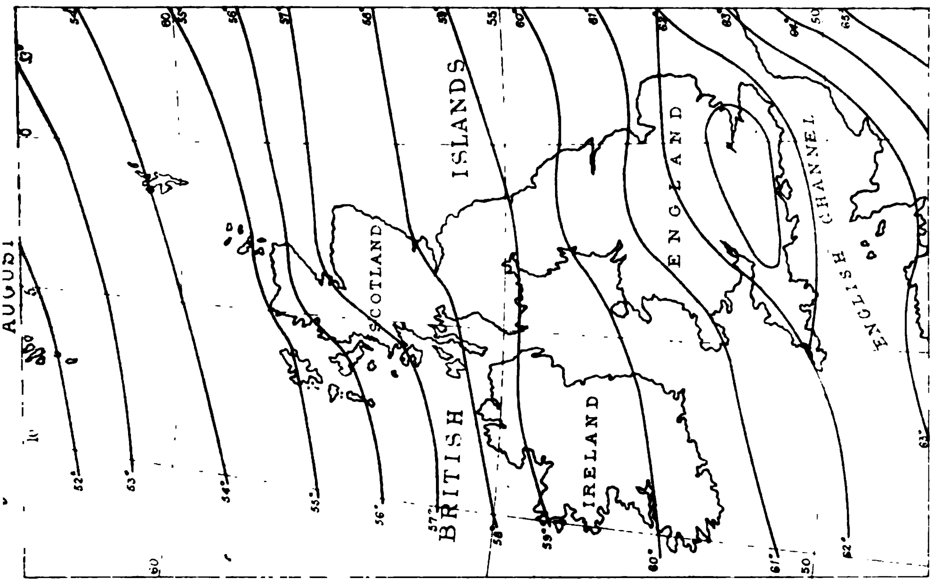
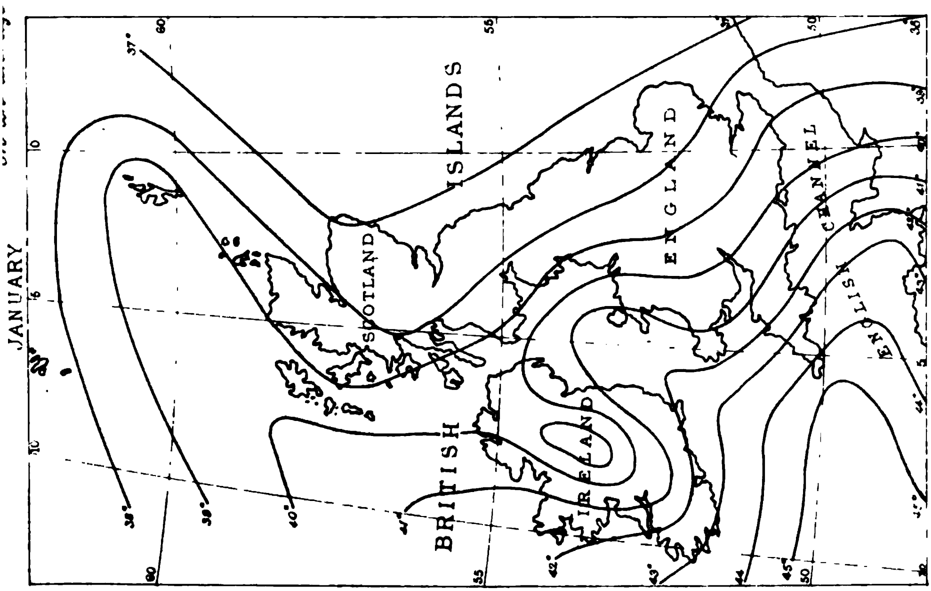


DIAGRAM IX.

Cape Florida Section

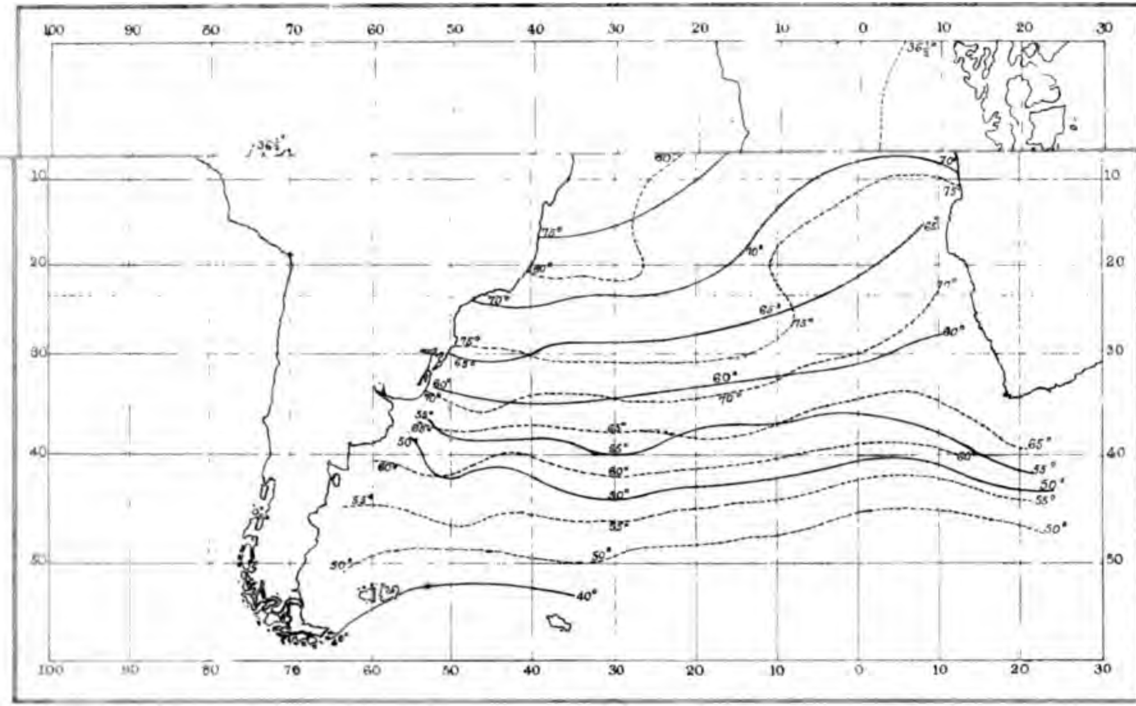






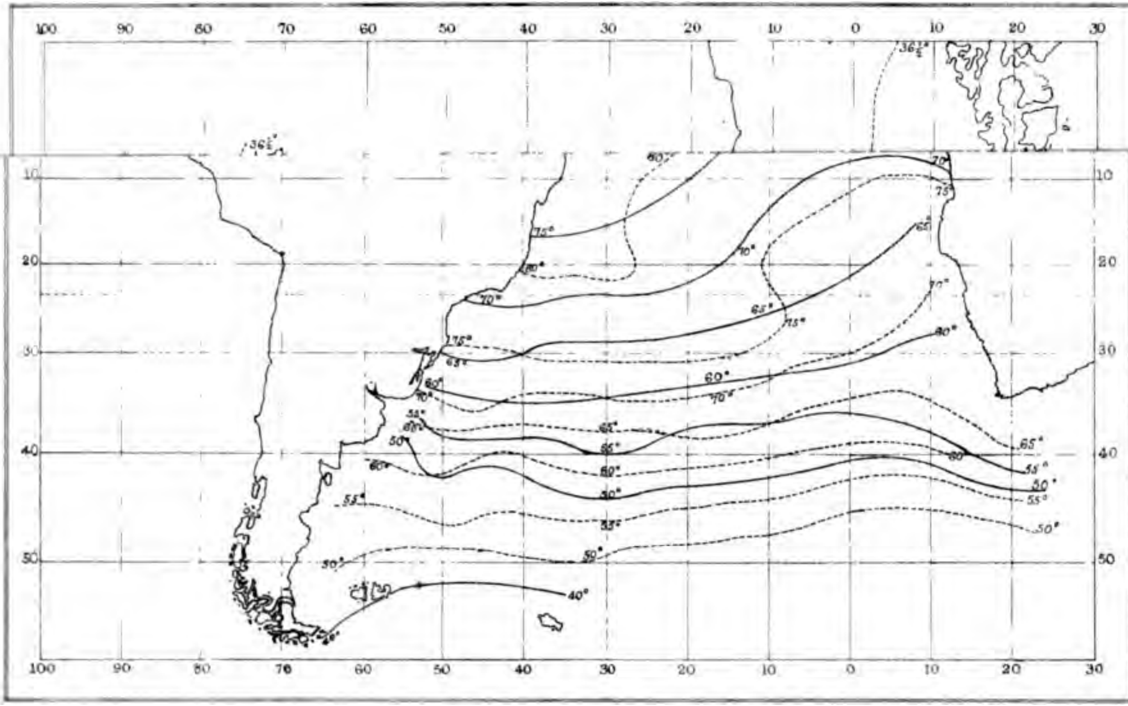
W. West & Co. lith.

Chart of the Atlantic Ocean showing its Isothermals for January (continuous lines) and July (dotted lines)



W. West & Co. sc. et imp.

Chart of the Atlantic Ocean, showing its Isothermals for January (continuous lines) and July (dotted lines)



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END OF THE TWENTIETH VOLUME.

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